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Edited by
AVIEZER TUCKER

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A Companion to the Philosophy of History and Historiography

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Edited by
Aviezer Tucker

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Contents

List of Contributors	ix
Acknowledgments	xi
Glossary of Terms	xii
1 Introduction <i>Aviezer Tucker</i>	1
Part I Major Fields	7
2 Philosophy of Historiography <i>Peter Kosso</i>	9
3 Philosophy of History <i>Zdeněk Vašíček</i>	26
4 Philosophical Issues in Natural History and Its Historiography <i>Carol E. Cleland</i>	44
5 Historians and Philosophy of Historiography <i>John Zammito</i>	63
Part II Basic Problems	85
6 Historiographic Evidence and Confirmation <i>Mark Day and Gregory Radick</i>	87
7 Causation in Historiography <i>Aviezer Tucker</i>	98
8 Historiographic Counterfactuals <i>Elazar Weinryb</i>	109
9 Historical Necessity and Contingency <i>Yemima Ben-Menahem</i>	120

CONTENTS

10	Explanation in Historiography <i>Graham Macdonald and Cynthia Macdonald</i>	131
11	Historiographic Understanding <i>Giuseppina D'Oro</i>	142
12	Colligation <i>C. Behan McCullagh</i>	152
13	The Laws of History <i>Stephan Berry</i>	162
14	Historiographic Objectivity <i>Paul Newall</i>	172
15	Realism about the Past <i>Murray Murphey</i>	181
16	Anti-realism about the Past <i>Fabrice Pataut</i>	190
17	Narrative and Interpretation <i>F. R. Ankersmit</i>	199
18	The Ontology of the Objects of Historiography <i>Lars Udehn</i>	209
19	Origins: Common Causes in Historiographic Reasoning <i>Aviezer Tucker</i>	220
20	Phylogenetic Inference <i>Matt Haber</i>	231
21	Historicism <i>Robert D'Amico</i>	243
22	Ethics and the Writing of Historiography <i>Jonathan Gorman</i>	253
23	Logical Fallacies of Historians <i>Paul Newall</i>	262
24	Historical Fallacies of Historians <i>Carlos Spoerhase and Colin G. King</i>	274
	Part III Philosophy and Sub-fields of Historiography	285
25	Philosophy of History of Science <i>Nicholas Jardine</i>	287
26	Philosophies of Historiography and the Social Sciences <i>Harold Kincaid</i>	297

27	The Philosophy of Evolutionary Theory <i>Michael Ruse</i>	307
28	The Philosophy of Geology <i>Rob Inkpen</i>	318
29	Philosophy of Archaeology <i>Ben Jeffares</i>	330
30	Reductionism: Historiography and Psychology <i>Cynthia Macdonald and Graham Macdonald</i>	342
31	Historiography and Myth <i>Mary Lefkowitz</i>	353
32	Historiography and Memory <i>Marie-Claire Lavabre</i>	362
33	Historiographic Schools <i>Christopher Lloyd</i>	371
	Part IV Classical Schools and Philosophers of Historiography and History	381
34	Leopold Ranke <i>Thomas Gil</i>	383
35	Scientific Historiography <i>Chris Lorenz</i>	393
36	Darwin <i>John S. Wilkins</i>	404
37	Logical Empiricism and Logical Positivism <i>Krzysztof Brzechczyn</i>	416
38	Jewish and Christian Philosophy of History <i>Samuel Moyn</i>	427
39	Muslim Philosophy of History <i>Zaid Ahmad</i>	437
40	Vico <i>Joseph Mali</i>	446
41	Kant and Herder <i>Sharon Anderson-Gold</i>	457
42	Hegel <i>Tom Rockmore</i>	468
43	Neo-Kantianism <i>Charles Bambach</i>	477

CONTENTS

44	Marx <i>Tom Rockmore</i>	488
45	Collingwood and Croce <i>Stein Helgeby</i>	498
46	Phenomenology <i>David Weberman</i>	508
47	Jan Patočka <i>Ivan Chvatík</i>	518
48	Hermeneutics <i>Rudolf A. Makkreel</i>	529
49	Postmodernism <i>Beverley Southgate</i>	540
50	Philosophy of History at the End of the Cold War <i>Krishan Kumar</i>	550
	Index	561

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This Companion has lived with me for three years and has thus become quite a member of the family. As it is about to mature, leave home, and hopefully carve a niche for itself on the shelves of the big wide world out there, it makes room for yet another companion to join myself and my companion for life, Veronika, who has stood by me through each one of my books, knowing it would never be the last. As for that other life-long companion, in the words of a poet of my generation:

Still in the earliest days of history
When the world existed only in theory . . .

Aviezer Tucker,
Bangor, Co. Down,
April 2008

Glossary of Terms

- Historians** People who write about past events. For example, Leopold Ranke.
- Historical** Of past events. For example, Latin was the historical language of the Romans.
- Historiographers** People who write about the history of historiography. For example, Georg Iggers.
- Historiographic** Of historiography, of written accounts of past events. For example, the theoretical assumptions historians make.
- Historiographic narrative** The final result of historiographic research written in narrative form. For example, textbooks.
- Historiographic research** The professional activities of historians. For example, searching the archives.
- Historiography** What historians write, about past events, about history. For example, Leopold Ranke's *History of the Popes*.
- History** Past events, processes, etc. For example, the decline and fall of the Roman Empire.
- Natural historians or historians of nature** People who write about natural history. For example, Stephen J. Gould.
- Natural historiography or historiography of nature** Writings about natural history by natural historians. For example, Stephen Hawking's *A Brief History of Time*.
- Natural history** The history of non-humans. For example, the universe, planet earth, the species.
- Philosophy of historiography** The philosophical examination, study, and theorizing about historiography, about what historians write, and its relation to the evidence, the epistemology of historiography, the ontology of historiographic concepts, etc.
- Philosophy of history** Philosophical examination, study, and theorizing about the past, including substantial/speculative philosophy of history but also issues like contingency and necessity in history. For example, Hegel's *Philosophy of History*.
- Scientific historiography** Historiography that offers scientific-grade probable beliefs about history.

1

Introduction

AVIEZER TUCKER

Editing is never simple, easy, or innocent. I had to make a number of choices at the outset: like editors of other Companions in the series, I had to decide which topics to cover, which entries to include, and who are the best available scholars to present them to a popular readership. But the still fluid and contested state of the philosophies of historiography and history forced me to make fundamental editorial decisions about the very nature of the philosophy of historiography that comparable editors were spared, most notably about what is the scope of this sub-field of philosophy and which terminology is appropriate for analyzing its problems.

In comparison with other meta-disciplinary philosophies, the task of defining the philosophies of historiography and history is particularly challenging. “What is the philosophy of science?” Asks W. H. Newton-Smith in his Introduction to *A Companion to the Philosophy of Science* (2000: 2). Dividing the question in half, Newton-Smith concluded that asking what is philosophy is one of the less fruitful philosophical occupations. Science, as well, has no essence, though philosophers tend to agree on core examples, “deciding just how far to extend the word ‘science’ will not be a substantial matter” (2002). Newton-Smith suggested then looking at what people who call themselves philosophers of science actually care about and do, acknowledging that “[t]here is no hiding the fact that they are an eclectic lot who do a diverse range of things, some of them strange” (2002: 3). It was quite easy though for Newton-Smith to draw a general map of the terrain covered by people who consider themselves philosophers of science and set the scope and details of the companion he edited on that basis.

In the philosophy of historiography, who is a philosopher of historiography is not only contested among philosophers of historiography who wish to exclude philosophers with whom they disagree, as in other philosophic fields, but some philosophers of historiography do not recognize their own contributions to the field, their vocation, and calling. Some philosophers of historiography consider themselves epistemologists, or philosophers of science, or metaphysicians, or philosophers of literature. Even a few of the contributors to this companion had to be told they were philosophers of historiography (who write prose), whether or not they were aware of it.

Still, by far, the greatest challenge was terminological. In the philosophy of science the terminology is entrenched, widely accepted, clear, and distinct. Though philosophers dispute what are “philosophy” and “science,” they can usually agree on proper

and improper use of the word “science.” They can communicate on the basis of shared meanings and mutual interpretations. Most fundamentally, they agree on the distinction between the concepts of science and nature. By contrast, in the philosophies of historiography and history there are no such wide agreements on the uses of words that allow undistorted or at least minimally distorted communication. Even the basic distinction between the events of the past and their representations is difficult to express and comprehend since often the same word, “history,” is used to mean both the events of the past and the texts that historians write about them. In a philosophical context, where we discuss issues concerning the relation between the past, our beliefs about it, our knowledge of it, and how we represent and justify our beliefs and knowledge, using “history” to mean all of the above would have led inevitably to one incredible mess! This is an even greater problem in the English language than, say, in German, which can create easily different new meanings through compounding existing words. In German, *Geschichte* is as ambiguous as *history*. But to distinguish clearly the representation of the past from the past proper, one simply writes *Geschichtswissenschaft*, the science or rigorous discipline of history. To distinguish research about the past from writing about the past in narrative form, one may resort to the distinction between *Geschichtsforschung* (historical research) performed by a *Geschichtsforscher* from *Geschichtsschreibung* (history writing) written by a *Geschichtsschreiber*. The only remaining ambiguity then for the German speaker is that of *Geschichtsphilosophie* that may involve the philosophical analysis of *Geschichtsforschung* or of *Geschichtsschreibung*. To avoid an incredible mess and confusions heaped upon each other, my first task was to introduce a standardized terminology. I attempted, though, to keep terminological innovation to the necessary minimum.

I restricted the use of *history* to refer to past events and processes, thus using the word in a narrower sense than the vague English everyday use. By contrast, I use *historiography* to mean the results of inquiries about history, written accounts of the past. This use of the word *historiography* preserves its standard English use. *The Merriam–Webster Unabridged Dictionary* (3rd edn., 2003) defines *historiography* as “a. The writing of history; especially the writing of history based on the critical examination of sources, the selection of particulars from the authentic materials, and the synthesis of particulars into a narrative that will stand the test of the critical method. b. the principles, theory, and history of historical writing.” *The Shorter Oxford English Dictionary* (5th edn., 2002) defines *historiography* as “the writing of history; written history; the study of history writing.” In accordance with these already established uses of *historiography* I reserve its use here for writings about the past that result from *historiographic research* (*Geschichtsforschung*). The people who produce historiography are historians. *Historiographic narrative* (*Geschichtsschreibung*) is the textbook result of historiographic research.

Another common ambiguity in the ordinary uses of *history* and *historiography* is in their scopes. In a narrow sense, the scope of history is that of literate human civilization and the scope of historiography is the study of documentary evidence generated by such civilizations to infer descriptions of their past and evolution. This narrow sense is closely linked with the Rankean research program in historiography, the inference of historiography from documents. In a broader sense, the scope of history is all of the past: societies have a history, but so do rocks, languages, species, and indeed the

universe. Historiography in this broader sense attempts then to infer descriptions of the histories of everything.

There are two theoretical reasons for upholding the earlier narrower scope of the terms: First, according to the original Rankean research program, reliable historiography, knowledge of the human past, can be inferred only from documents that were not written for posterity, but have been preserved usually in archives. This limitation of the evidential base has become obsolete since historians developed methods for reliable inference of information about the past from material remains, artefacts, shapes of landscapes, genetic analysis of present and fossil DNA, works of art, and so on. Second, some philosophical approaches to historiography consider it special for having a human subject matter. Forms of description, understanding, and explanation in historiography are allegedly different because of this special subject matter. From this perspective, *history* would refer then exclusively to the human past and *historiography* would describe exclusively the human past, though it would not be limited to the inference of descriptions of the past from documentary evidence. Historiography would be limited only by the evidence available for historical forms of the human mind, usually documents and artefacts. Accepting such a limited scope for the terms “history” and “historiography,” would have implied a commitment to the tenets of this particular school of philosophy of historiography. Alternative philosophical approaches argue that there are some common and unique features to all the sciences of the past, sciences that are concerned with the inference of unobservable token events from their traces in the present. To avoid commitment to one school or the other and encourage debate and exchange between them, the terms history and historiography are used here in their broadest and most inclusive scope, as all the past and all that can be known about it respectively.

Sometimes, it is necessary in this philosophical context to distinguish particular realms of history or sub-fields of historiography. On such occasions the terms history and historiography are compounded, as in *natural history* that refers exclusively to non-human history, and the *historiography of nature* that refers exclusively to descriptions of natural history; similarly, social or cultural historiography describe social or cultural history and, and so on. Occasionally, authors refer to academic schools that include a social group of historians, the historiographic theoretical and methodological approach that unites them, and the historical realm to which they apply their historiographic approaches. For example, “Social Science History” has an established use as referring to a school of historians who attempt to use the methods and tools of the social sciences such as statistics to produce a historiography that is quantitative and “scientific.” Terms that refer to a school of historians appear then either capitalized or in quotation marks, or both.

Most significantly, and this is a distinction that does not quite exist in any natural language or philosophic jargon, it was necessary to distinguish the *philosophy of historiography* from the *philosophy of history*. Existing philosophical jargon distinguishes *critical* or *analytic* philosophy of history from *substantive* or *speculative* philosophy of history. This terminology is unsatisfactory because it is too vague and value laden and reflects obsolete philosophical positions and distinctions, rather than the simple distinction in subject matter between the past and knowledge or descriptions of it. The project of *critical* philosophy is closely connected with the Kantian project of examining the

conditions of knowledge. While the philosophy of historiography is certainly interested in the conditions of knowledge of history, there is much more to it than this Kantian project. Likewise, *analytic* philosophy of history, the analysis of the language of historiography and the elucidation of the concepts historians use is certainly part of the philosophy of historiography. But the philosophy of historiography, like the philosophy of science, does much more than the analysis of language and concepts, it examines the epistemology of our knowledge of history, the relation between evidence and historiography, the reliability of the methods historians use to infer beliefs about the past, and so on, beyond the analysis of language. Philosophers of historiography are arguably as synthetic as they are analytic. After Quine, the very distinction between analytic and synthetic has collapsed. *Substantive* philosophy of history implies that its alternatives are ephemeral, while not saying much about what this substantiality actually means. *Speculative* philosophy is essentially a term of abuse.

Instead, philosophy of historiography is simply the philosophical examination of all the aspects of our descriptions, beliefs, and knowledge of the past. The philosophy of historiography parallels other philosophical meta-disciplinary sub-fields such as the philosophy of science or the philosophy of economics. By contrast, the philosophy of history is the direct philosophical examination of history. The philosophy of history examines questions about history such as whether it is necessary or contingent, whether it has a direction or whether it is coincidental, and if it has a direction, what it is, and how and why it is unfolding. The philosophy of history parallels then sub-fields of metaphysics that examine the ultimate constituent parts of everything, such as the philosophy of nature. The distinction between the philosophy of historiography and the philosophy of history is clearer than existing distinctions, descriptive rather than value laden, and parallels terms that designate existing sub-fields of philosophy such as the philosophies of science and nature.

As editor, I ensured that all the entries adhere to this unified terminology, and so the reader can safely assume that terms in different entries have the same meanings. Obviously, I could not interfere with the terminology used by quoted sources. Quotations may use then the ambiguous existing terminology regarding “history,” “philosophy of history,” and so on, and may use the same words to convey different meanings. I hope that the contexts of the quotations will help clarify their meanings. This is as good a solution to the terminological challenge that could be hoped for without violating the sanctity of quoted phrases.

Following the terminology, I had to select the scope of topics to be covered. The main dilemmas were how broad to conceive this field of philosophic research and whether to concentrate exclusively on contemporary research or also pay attention to the history of the field, its major historical traditions, and figures. My approach here has been to be the most inclusive and comprehensive. I interpreted the scope of philosophies of historiography and history most liberally to encompass all significant philosophic topics within the broadest scope of interpretation. The longer first four entries outline the major sub-fields that are covered. In addition to the obvious entries for philosophy of historiography and philosophy of history, there are entries for the philosophy of natural history and its historiography, stressing the inclusion of natural history and historiography within the scope of the philosophies of historiography and history. Since this Companion is intended for historians just as much as for philosophers, the fourth

entry, by a historian, covers the philosophical issues that are particularly relevant for historians.

The second and longest part covers the main problems of the philosophies of historiography and history, evidence, confirmation, causation, counterfactuals, contingency and necessity, explanation and understanding, objectivity, realism, ethics, and narrative. Though the entries are written by philosophers with diverse and indeed opposing approaches to these problems, all the entries in the second part assume that the distinct problems of the philosophy of historiography are deeply intertwined with other areas of philosophy. To borrow Arthur Danto's vivid metaphor, the philosophy of historiography does not exist on some remote atoll where forlorn Second World War soldiers continue fighting an obsolete long extinguished war, oblivious of the results and indeed end of the war elsewhere. Rather, the major problems of the philosophy of historiography from causation to evidence and confirmation to objectivity are connected in their formulations, assumptions, and mooted solutions to similar problems in other philosophical fields, most notably though not exclusively, epistemology, philosophy of science, and metaphysics.

Further, since the philosophy of historiography is a constantly changing dynamic program of research, the entries in the second part of this Companion are strictly up-to-date presentations of the current state of research on the problems they cover. There is more, much more, to the philosophy of historiography than the old debates about the covering law model or *Verstehen*. This Companion demonstrates the breadth as well as the contemporary relevance of philosophy of historiography for other branches of philosophy and general philosophic discussions of causation, evidence, confirmation, origins, laws, explanation and so on. Such general discussions are parochial unless they consider the application of general theories to the special cases of historiography and history. Conversely, philosophies of historiography and history must consider general discussions of the problems they consider to avoid intellectual provincialism, to benefit from the immense strides epistemology, philosophy of science, and metaphysics have made in our understanding of philosophical problems and how to solve them.

The third part examines specific philosophic issues in particular sub-fields of historiography and history such as the historiography and history of science or phylogeny. This part is particularly relevant for historians. The fourth and last part covers schools, traditions, and figures from the history of the philosophies of historiography and history. True to the liberal broad scope of this companion, this part covers Darwin as well as Ranke, phenomenology as well as logical-positivism, Marx as well as Fukuyama.

This companion was conceived with philosophers as well as historians (of nature as well as of humanity) in mind. The entries do not assume prior familiarity with their topics. Students of philosophy and history would likewise find this Companion highly accessible. I hope that this companion will be most useful for spurring research in the philosophies of historiography and history. Each one of the entries and their bibliographies can serve as a springboard for research, for pushing forward the frontiers of knowledge. This is particularly true of the entries where authors interpreted sometimes for the first time the implications of contemporary debated in philosophy in general, epistemology, metaphysics, and the philosophy of science for the philosophies of historiography and history. The philosophies of historiography and history are still very

much a philosophical *terra incognita* for research. I am certain that many of the seeds of successful programs of research can be found throughout this companion.

The significance and importance of the philosophies of historiography and history for philosophy and historiography cannot be overestimated. At the very least, the philosophy of historiography may assist historians and philosophers in avoiding making common mistakes. Historians, who read the entries in this companion, should be able to avoid logical fallacies, confirm better their hypotheses using evidence, have a firmer grasp of the nature of causation and explanation in historiography, and be aware of possible uses of counterfactuals. In other words, historians could improve their methods, inferences, and assumption by becoming aware of best historiographic practice elucidated philosophically. Conversely, philosophers could benefit from avoiding false generalizations and anachronisms by understanding the nature of history and historiography. Philosophers who have been quite innocent of historiography and history have been making patently false generalizations about causation, explanation, counterfactuals, laws, science, understanding, necessity and contingency, and so on that could have been avoided had they taken them into account

We live in a civilization that too often either ignores the past, or takes it for granted. Either way, the result is temporal provincialism, the assumption that the past looked pretty much like the present and so has nothing to teach us. Philosophers who are embedded in this culture compound ignoring the past in favor of false universal statements that are founded on a belief in the eternity of the present, with taking the past for granted, ignoring the epistemic issues involved in our knowledge of the past. Even when philosophers do read historiography and attempt to consider its philosophically relevant results, they too often take it for granted, almost as if it offered pure observation sentences of the past, unmarred by varying degrees of reliability, underdetermination, value ladenness, and narrative construction. Conversely, some philosophers dismiss historiography altogether as a source of knowledge, believing somehow that the only proper science for philosophers to study is physics, which of course has no history . . . Such presumptions can only be based on ignorance of the epistemology of historiography and the history of physics. This companion would fulfil its proper role if it awakens both groups from their dogmatic slumber.

Part I

Major Fields

Philosophy of Historiography

PETER KOSSO

The philosophical issues in the analysis of historiography are almost entirely epistemological. Most of the core concerns of epistemology are present in the analysis of describing and knowing the past. The emphasis, the wording, and the perspective are distinctive, but the questions are basic: Can we know the past? More precisely, what kinds of things can we know and what kinds of things are beyond the limits of knowledge? How can we tell the difference between accurate descriptions of the past and inaccurate descriptions? What distinguishes a justified interpretation from speculation or invention? In other words, how do we know that what we say happened really happened?

Of course, there are issues unique to historiography and its particular challenges, but we will do well to begin with the more general epistemological situation. In historiography as in life and in science, there is a gap between the information that is available and the object of our interest. The epistemic challenge is very general. In the study of history, we are interested in people in the past, their actions, ideas, and accouterments. All we have to go on as evidence are their textual and material remains. Claiming to know about the human past is thus claiming to know about more than is immediately perceived, and this raises questions of accuracy. These questions can only be addressed following a model of epistemic justification.

Crossing the gap between available evidence and the historical objects of inquiry encounters two distinct difficulties. One is that the things we want to know about are in the past; they are dead, gone, and unobservable. The other difficulty is that many of the objects were people. They were willful and idiosyncratic, and they were not of our own culture, so understanding them is bound to be even more difficult than understanding our neighbors and compatriots.

The first half of the challenge, the unobservability of the past, invites a comparison to the challenge of natural science in dealing with things like atoms and black holes. In contrast, the second part of the challenge discourages this comparison, since persons, unlike electrons, are not all alike. People, unlike planets, are expressive and creative. This ambivalence in the comparison of the study of historiography to science fuels one of the main debates of analytic philosophy of historiography: Is historiography a science? Better, in what ways if any are the method and structure of justification the same in historiography and science? This is a recurring theme in what follows: questions about the nature of historiographic facts, evidence, and explanations.

The comparison of historiography to science does not have to yield a yes-or-no result. Nor does it have to be viewed as a way of evaluating historiography (or science). It can be simply a way of understanding historiographic methods by comparing them to something else. The philosophy of science is relatively well developed, and methodological issues of science have been described in detail. We might as well take advantage of this as a source of comparison. We will learn more about historiography in this way, and we will probably learn more about science in the process.

The challenge of the accuracy of knowledge of history raises the question of the structure of justification. Is knowledge of history built on foundations, or is credibility more a matter of coherence among claims with no bottom line of basic beliefs? This general epistemological question is at work in analytic philosophy of historiography, but not in these terms. It is presented in terms of the status of facts and evidence, whether these things are discovered or invented in the inevitable act of interpretation. If facts are there to be discovered, they may serve as the epistemic given and supply the foundations for a bottom-up model of justification. If, on the other hand, facts are constructed, there is an unavoidable influence of established beliefs on new evidence, suggesting a web-like, top-down model.

The basic epistemological issues are often filtered through the role and status of interpretation. Interpretation is important to historians, and it is a way to address issues of relativism, realism, skepticism, and positivism. It sounds extreme to say that historiographic descriptions are nothing but collections of interpretations, but understanding and evaluating this claim requires understanding what is meant by "interpretation" and figuring out how deep it reaches into historiographic representations of the historical process. Is interpretation contrary or even an obstacle to accuracy? If some interpretations are true but others false, *objectively* true or false, then thoroughgoing interpretation and realism are compatible. If there are recognizable and universal standards of distinguishing the true from the false, or at least the likely-to-be-true from the unlikely, then pervasive interpretation is no ally to relativism or skepticism.

Related concerns are about historiographic objectivity and subjectivity. Humans studying and describing other humans risk imposing their own ideas and sensibilities on the others. Considering that the people in the past lived in cultures somewhat different from present ones, describing them in terms and concepts appropriate to our own time may be at least inaccurate. Describing the past in ways that make sense to us may amount to just describing ourselves in different circumstances. But subjectivity might be unavoidable, since we have to describe the past in ways that make sense to us and on our own terms. As Benedetto Croce put it, "The practical requirements which underlie every historical judgment give to all history the character of 'contemporary history'" (Croce 1941: 19).

This indelible subjectivity is sometimes seen to be more than an epistemic concern; it's a moral issue as well. In the extreme it is "intellectual colonialism" (Hodder 1991: 110) to force our ideas and values on the description of people in the past. One way to avoid this is to embrace subjectivity and explicitly renounce all claims to accuracy of description. Historiographies in this view, are about the present, and the primary concern is that they be used responsibly, not, for example, as tools of power or manipulation. Since the story of the past is so fully under our control, it must be used wisely and fairly. But this complete subjectivism is just one position on the role of interpretation,

and its consequence is described here only to show the importance of the question about objectivity and subjectivity. Concerns about objectivity, like concerns about interpretation, show up throughout the philosophy of historiography, in analyses of facts, evidence, justification, and explanation.

The big questions are epistemological, but a little metaphysics shows up as well. Usually the metaphysical questions arise insofar as they are unavoidable for working on the epistemology. You have to know something about the nature of the thing and what influences it has on us in order to assess how we are able to know about it. So there are issues regarding the nature of human beings, the object of historical knowledge. Human beings, for example, are essentially social. The epistemological implication is that individuals in the past can only be understood in their cultural context. Accuracy of description of individual actions, and hence the structure of justification of knowledge of history, must acknowledge the influence of the society on the individual. But individual human action is not *determined* by the cultural or physical circumstances. There is room for agency and spontaneity. This too has epistemological implications, primarily in the nature of historiographic explanation and understanding. Explanations in science usually invoke laws of nature, and understanding is a matter of generalization. But human free will and idiosyncratic actions seem to preclude there being laws of human nature and block all but the most trivial generalizations. The particular nature of the object of interest in history may force a different, distinctly nonscientific, kind of explanation.

Sometimes metaphysical issues follow from the epistemology, rather than the other way around. For example, if interpretation is seen to reach not only the finished historiography, what Goldstein called “the superstructure,” but all the way down to the evidence and the facts themselves, then there is a sense in which every part of historiography is constructed. The past itself, history, exists only as it is described. In this way, a kind of metaphysical anti-realism about the human past might follow from the constructivist epistemology.

Clarity and plausibility on any of these issues, whether epistemology or metaphysics, demands precision in describing and relating the key components of historiography and knowledge of the past. To facilitate this, we will start with the basics, facts.

Facts

The F-word. What is there to say about facts? You would think that facts just are. But several key issues in the philosophy of historiography pivot on the status of historiographic facts. It is a multifaceted concept and meanings of the term vary in unclear and unannounced ways. Consider the variety of dichotomies in which facts appear: We must distinguish between fact and value, between fact and theory, and between fact and interpretation. These are not identical distinctions, indicating that there is a variety of concerns about facts.

Consider as well some of the extreme attitudes about historiographic facts, from positivism to relativism. Perhaps the historian’s attitude and aspiration should be to emulate Thomas Gradgrind in Dickens’ *Hard Times*. “In this life, we want nothing but Facts, sir; nothing but Facts!” But according to Carl Becker, “the facts of history do not

exist for any historian till he creates them" (Becker 1910: 528). This contrast between the status and use of facts, independent and there to be discovered on the one hand, created by us on the other, may be illusive if the two positions are using "facts" in two different ways. Unless we are each of us clear on what we mean by "facts," conflicts may be at cross-purposes. Perhaps historiographic facts are importantly independent of historians *and* historiographic facts are created by historians; it's just that these claims use different meanings of "facts."

It is important to distinguish between facts as real situations and facts as true descriptions of situations. In the context of history and historiography respectively, facts as situations would exist in the past, while facts as true descriptions exist in the present. The confusion between these two meanings of "historical/historiographic fact" is facilitated by two common meanings of the word "history." Sometimes "history" means the events, the people, and objects in the past, what happened, what was there, who did what. At other times, "history" refers to the discipline and what it does, namely describe the human past, which we call in this companion historiography. If you came across a book entitled, *Women in History*, you might rightly wonder whether it was about women in the past or about women historians. This ambivalence in the word "history" affects the concept of historical/historiographic facts. Are facts things (and people and ideas) in the past, or are they descriptions of these things, descriptions in the present?

If the facts are the actual events and people in the past, then not a single book contains a single fact. But historiography books are full of accurate descriptions of historical facts. Furthermore, if facts are events, then talk of a fact/theory distinction is misleading. It contrasts a thing with a description. To say of some idea, that it is a theory not a fact, is a confusion of categories, a comparison of apples and oranges. Facts are; theories describe. This distinction is an ongoing challenge for evolutionary biologists. They must confront the confusion in the claim that evolution is a theory not a fact. The appropriate response is that there is both a fact of evolution, the events in the past, and a theory of evolution, the description of those events.

If the facts are the actual events and people in the past, then facts are not properly said to be true (or false). "True fact" is not redundant; it is confused. Facts in this sense are not true or false; they simply are. It is descriptions of the past that are true or false, the true ones being those that accurately describe the facts. So the case the evolutionary biologist has to make is that there is adequate reason to think the theory of evolution is true. Similarly for the historian, the challenge is to show that there is reason to think particular historiographic descriptions match the facts.

On the alternative use of "facts," as descriptions of the past rather than as actual things in the past, the issues and the appropriate way of talking about them are different. Now facts are part of historiography, part of the discipline and the descriptions in the present. And they are a special part, in that facts are distinguished as the true bits of description. Books may indeed be full of facts in this sense, or they might not be. But now, if there are no facts in the books, or fewer than we thought, it is of serious concern. The phrase "true fact" now does make sense, although it verges on being redundant. And now a contrast between fact and theory is potentially meaningful, although it still needs a clear account of the role of both facts and theories in historiographic description.

The clear account begins by asking whether facts as bits of description are epistemically foundational. Do factual descriptions speak for themselves? Do they consist of direct information from the past, clearly accurate without further proof? Are the facts given to us, or are they collected or constructed? Even if the facts are collected, discovered rather than invented, the assemblage, the facts we choose and the way we put them together, might be contrived. The inevitable process of selection, description, and arrangement in terms of relevance will be done under the influence of our existing beliefs about the past. In this way, even if the facts are individually independent of our present ideas, they collectively form a picture that is a matter of interpretation.

The ramifications of the need to select historiographic facts pose one of the most widely discussed issues of analytic philosophy of historiography. It applies to either sense of the concept of facts, as the events in the past or as descriptions of the events. We are simply not interested in everything that happened in the past. Whether Columbus sneezed an even or odd number of times during his first crossing of the Atlantic is of no consequence and no concern. There is a fact of the matter but not a significant fact. We cannot tell the entire story of the past, in all its detail, and so there is always the concern that the parts that are told are lopsided or misleading in some important way.

This concern could be minimized or even ignored if the facts were self-selecting in a way that the patterns of importance and relevance were explicit and natural. But the concern is real if arrays of facts do not separate themselves into the important and the unimportant, and that task is left to the historian. What to make of the selective nature of the facts, in other words, depends on the details of the selection mechanism.

Some mechanisms for selecting facts are indeed beyond our control. Some records of the past survive; some do not. Some documents are lost or destroyed, and the historical facts they describe are selected out of the record. Stone, metal, and ceramics last much longer than wood, or cloth, or paper, so we have a somewhat skewed record of activities involving lots of evidence of the former but less of the latter. The record of facts is lopsided, but it seems wrong to say this makes it biased, since there is no intentional influence by a person, neither in the past nor in the present. The historical record is selective in the same way the fossil record is selective. Some organisms, like fishes and ferns, leave long-lasting fossil records. Others, like jellyfish, do not. This manner of natural selection of what information is preserved must be factored into interpretation of the assemblage of facts.

Another mechanism of selection of facts depends on what people in the past chose to write about and what they chose to ignore. Facts are selected by the original historical sources. They are often written by the victors and a priori must be written by the literate. These and other constraints pick out only certain kinds of facts, and so the record is biased in favor of information on the powerful, the wealthy, and other past elites, most notably religious ones. Unique and special events were considered noteworthy, while the everyday and mundane were not, and so facts about commoners and the commonplace are missing from the record. Nobody thought to record the precise number of Columbus' sneezes. This manner of selection of the facts does make the record not only lopsided but biased, as the selection process is, at least potentially, intentional. This bias can be an obstacle to getting an accurate description of the past, but it can be beneficial as well. By paying attention to the sorts of things the people in the past thought worthy of writing down, as well as to how they wrote them down,

the historian can gain insight into what they valued. In other words, historians today can exploit the subjectivity in the selection process and use it to learn more about what was on the minds of people in the past.

It is worth noting that the selective nature of the historical record can be complemented to some degree by archaeological remains. A lot of archaeological evidence is in the form of garbage and ruins, the mindlessly left behind. This record is not intentional, nor is it just about the unique and noteworthy. Potsherds, building remains, and tools are leftovers from daily life. The mundane facts, usually overlooked by the written record, often show up in the physical.

There is a third mechanism for selecting facts, and this is the one that initially raised the concern of bias. The facts must be selected by historians in the present. Even if there were records on how many times Columbus sneezed, odd or even, that historical fact would be ignored. It's irrelevant. That is, it is irrelevant to our interests in the past, as judged by what we already believe. But the judgment of relevance is influenced by our own times, by our culture, by our professional training and theoretical perspectives, and even by our individual quirks. This manner of selection is intentional and, despite some denials, unavoidable. The best way to deal with it, that is, the best way to arrive at an accurate description of the past in spite of this selection process, is to recognize its inevitability and to be explicit on the criteria of selection. E. H. Carr lists it as the first of two important truths: "You cannot fully understand or appreciate the work of the historian unless you have first grasped the standpoint from which he himself approached it" (Carr 1961: 48). In other words, know your historian. And, Socrates might add, know thyself.

In general, the worry about the selective nature of factual historiography is that each historiographic fact could be individually true together with other such facts it could collectively add to a misleading or inaccurate picture of the past. To use a mosaic analogy, if we have only a few of the tiles, then even if each is in its proper place we may misinterpret the picture. Choose all blue tiles, for example, and you have evidence of a predominately blue mosaic, perhaps the sea. The sampling is not random, and we are likely to end up with the picture we want or suspected, since we have put it together using only what we judge to be the important, relevant tiles.

The distinction between facts and values is a significantly different issue than either the fact/theory or fact/interpretation distinction. It is not about *our* values, as they might affect the interpretation or theorizing about the past. It's about *their* values, the values held by people in the past. Human history is composed of human beings, as individuals and in social groups; it is not made of stones or wood. There is much more to the human past than the physical facts, the things you would have experienced had you been there on the scene. There is also the mental situation, that is, the ideas, intentions, motives, and values of the participants in historical events. These are things you could not experience directly, even if you were there. The contrast between fact and value is much like this contrast between the physical and the mental aspects of the past.

This is not about a distinction between "is" and "ought." It is not separating claims about what did happen from those about what ought to have happened. Values include more than moral principles; they include as well what a person or society regard as important, as desirable (sometimes the opposite of what is right), what is beautiful, and so on. Values, in this broad sense, are often the reasons for action, and as such

they are a necessary component of our understanding other people. It might even be argued that value judgments are such an integral part of human actions that there is no separating the facts of what people did from their mental decisions. "All history is the history of thought" according to R. G. Collingwood (1946: 215).

The epistemological question is whether matters of fact are known in different ways than matters of value. Is the structure of justification different for claims about historical facts than for claims about historical values? Or perhaps one of these can be known while the other cannot. Contrary to Collingwood, perhaps we can know what people did in the past, but we cannot know why.

A clearer way to frame this issue is in terms of the physical past and the mental past rather than explicitly in terms of facts and values. From an epistemological perspective, there is nothing nonsensical or contradictory in talking about the fact of the matter regarding values. For example, "In fact, Socrates valued intellectual and moral integrity more than wealth." The epistemological question is how, or even whether, we can know this. The important contrast is not so much between facts and values as between physical and mental events in the past.

The epistemological concerns lead directly to issues of historical evidence, that is, the informational remains from the past and what we make of it in the present. Talking about the facts is about the people and events themselves. Next we must consider our access to information about these things, the evidence.

Evidence

Talk of evidence encourages a comparison with science. According to the scientific method, evidence is the source of theorizing and, more importantly, the basis for testing a theory once it has been proposed. Talk of evidence might also suggest a comparison between history and a courtroom trial. This legal analogy is appropriate and helpful. The challenge for a jury is to determine the accurate description of some past event, that is, to figure out who did what. This concerns the physical past. The jury must also consider the mental aspects of events when questions of motive or premeditation are at issue.

The comparison between history and a courtroom trial continues by noting that there are two forms of evidence in both cases. To render an informed verdict, the jury must consider both testimony of witnesses and material evidence. Similarly, evidence of the human past is both in the form of the words of witnesses and material remains. Linguistic evidence is textual, it comes in a variety of forms, including purposeful narrative, journals, letters, literature, bills of trade, and so on. Material, non-linguistic evidence is in the form of buildings, ruined or intact, potsherds, worked stone, art, and the like. The difference in using textual rather than material evidence is often taken as the disciplinary divide between historiography and archaeology. Historians study the past through words; archaeologists study the past through objects.

The distinction between textual and material evidence does not correspond with that between the mental and physical past. Textual evidence may report on physical events in the past, and material evidence can provide insights into what people thought and valued. But perhaps the two kinds of evidence have different strengths, a difference that

is linked to the mental/physical difference. Texts, for example, can be literal and explicit statements of what someone was thinking. They might explain outright what was of value or why something was done. On the other hand, texts are produced intentionally and hence are more susceptible to deceit, misrepresentation, and the sort of selectivity that is controlled by the writer. Material remains are often more candid and more egalitarian in their representation. But material remains require more interpretation to elicit their meaning.

We will focus on the textual remains, since our primary interest is the philosophy of historiography, not archaeology. The comparison to science can still be helpful. Natural sciences are required to be empirical in the sense that conclusions must be based on observations. In what sense is historiography empirical? Scientists have real objects and phenomena to look at and manipulate. What do historians have to point to or touch? As Marc Bloch put the challenge, “No Egyptologist has ever seen Ramses” (Bloch 1953: 48), but physicists routinely see the tangible tracks of elementary particles, as paleontologists pick up and inspect fossils.

The comparison between the historian’s inability to see Ramses and the paleontologist’s ability to see fossils is misleading. It is a confusion of object and evidence, a confusion that needs to be cleared up in order to understand the empirical status of historiography. No paleontologist has ever seen a dinosaur, and that, like Ramses to the historian, is the object of interest. The fossils are evidence, an informational means to knowledge of the unobservable thing we hope to accurately describe. In historiography, the evidence, and hence the appropriate comparison to the fossils or even the tracks in particle detectors, is in the textual remains, which historians routinely see. Just as in the sciences, the important epistemic step is to link the evidence at hand to the object of interest.

The pivotal epistemological concern about evidence is accuracy. In historiography, this concern has two facets, authenticity and meaning. Authenticity of historical evidence is an issue of a document being genuine, written when, where, and by whom it claims. It is also about the author telling the truth and being a reliable witness to the events being described. Meaning, by contrast, is about what the evidence says, and this will depend on our own interpretive influences. The meaning in the evidence is its informational content.

There is a similar pair of concerns with scientific evidence, though the terminology is different. Authenticity corresponds with the reliability of evidence. Galileo had some trouble convincing some of his contemporaries that the telescope was a reliable tool for viewing the heavens. The question was whether the image was caused by the objects in the sky, and was not an artefact produced by the device itself. That is a question of authenticity. Once authenticity has been established, there is the question of meaning. What does pink litmus paper mean about the liquid into which it has been dipped? What is the pink color evidence *of*? Pink means acid, as Doppler red-shifts of distant galaxies mean the universe is expanding. Authenticity and meaning are necessary components of any kind of evidence, in science as in historiography.

Authenticity comes first, as there is no point in interpreting the meaning of bogus or bungled evidence. Hitler’s diaries would be an invaluable insight into the mind of the man and the values of Nazi Germany, but only if the documents are genuine and were really written by Hitler.

David Hume suggested a simple correlation between credibility of historical evidence and the number of years and information transfers, person to person, between the events and the present (Hume 1975 [1740]: book I, part III, xiii). The closer in time we are to what's being described, and the fewer times the information has changed hands, the more likely is the evidence to be accurate. There is something to this, but there is a lot more as well to determining authenticity.

The number of steps in the flow of information is probably not as important as the quality of each. To judge the authenticity of evidence, modern historians need to know who recorded the evidence, how they would know about what they were describing, what their reason for writing was, their special interests, and so on. M. I. Finley summarizes with a few methodological guidelines, one of which advises, "The first questions to be asked of any written source are, why was it written? why was it 'published?'" (Finley 1987: 105). Assessing authenticity, in other words, requires knowing some basic things about the source. And this highlights a more general epistemological point. Even the most basic data require an appeal to background knowledge, in this case an understanding of the source, if the data are to serve as evidence. Arthur Danto put it in more picturesque terms by saying, "One does not go naked into the archives" (Danto 1965: 101). This, the appeal to background knowledge, not the nakedness, will be important to the issue of the structure of justification of knowledge of history. Are there foundations of justification? If even the basic evidence is not foundational in the sense of being epistemically self-sufficient, then what is?

Hume's suggestion for evaluating historical evidence is naïve in focusing on quantity rather than quality of informational transfer. It is also too simple in failing to recognize that in some ways, distance from events can increase the accuracy of reporting. Journalists lose credibility when they become part of their own story or when they become too close, emotionally or intellectually, to their subject. They begin to report more on themselves and their reactions than on the events. Their selection and interpretation of data take over, whether they are aware of it or not. Distance from events, as happens by the passage of time, can alleviate this involvement. It also allows more data to appear and a more dispassionate, disengaged perspective to arise. So, distance from events has both a negative epistemic effect, the further (in time) the hazier the view, and a positive epistemic effect, the less involved the more objective. This is further indication that the evaluation of historical evidence is complicated.

The best evidence, in the sense of being the most candid and insightful, may be indirect and implicit evidence. By asking what sorts of assumptions and values the original author must have had in order to write the things he or she did, the present-day historian gets some insight into the social structure, ethical, and aesthetic standards, and even metaphysical presuppositions in the past. This is information that the original author did not intend to share, and may even have been unaware of. It is the kind of information that makes myths and literature rich sources of historical evidence. These sources may be explicitly fiction, but they are implicitly rich in facts about cultural values.

All of these details about historical evidence, these ways of assessing and finessing credibility and information, are similar to what the jury must do in a courtroom trial. But there is an important difference; the jury is of peers, people who should share a

cultural context with the defendant and the witnesses. The jurors are implicitly using their own context to assess and interpret the words of a witness and apply them to deciding what the defendant did. Historians implicitly use their own context to assess the words of sources and to decide what happened, but the historians are not peers of the people in the past. This raises the concern and the epistemological issue of judging the evidence out of context and hence misjudging it. It is an issue again of accuracy.

The influence of the historian's cultural preconceptions is more an issue in interpretation of evidence than in assessing authenticity. What did the original author mean in writing this text? And of course, the text must be meaningful to us if it is to function as evidence. It must bear information of something of interest, otherwise it is just a useless (meaningless) observation. This requirement of meaning is as important in science as it is in historiography. A fuzzy, curved streak in a particle detector means that an electrically charged particle has passed through. Without the theoretical wherewithal to interpret the datum as a charged particle, it's just an insignificant fuzzy streak.

Interpretation of data, the epistemic promotion from sensation to evidence, must be done within some conceptual, theoretical framework. In science, we appeal to established theories, the things found in textbooks, to understand what the basic observations mean. Look it up and you will find out how the electrically charged particle precipitates the formation of vapor that is visible as the track, and how the magnetic field of the detector causes a curved trajectory. What we already know about nature is always being called on to learn new things. Similarly in historiography; what we already know about the past has an important and inescapable role to play in finding out more about the past. In historiography, it is not so straightforward or as explicit as looking in a textbook for causal laws that link the data to the objects of interest. The influences are various and sometimes subtle.

This raises an important issue about objectivity. Historical evidence must be meaningful to us. It must be interpreted by us. Yet it is supposed to be information from the past. In order to get information from the past we must add some information from the present, and this seems to compromise the objective quality of the evidence. Understanding and dealing with this tension between the need to add information to get information are key issues in the philosophy of historiography. Pure, objective information from the past, passively given to us in the present, seems to be impossible.

One of the ways that the context of the present influences the interpretation of evidence from the past is simply the use of language. The language in the evidence, and the language in our own historiographic descriptions, are each acts of categorizing and conceptualizing. To read of some activity that it is religious, for example, puts it into a category that for us includes faith, authority, and spirituality. But the concept of religion could have been slightly or radically different at the time the evidence was written. To describe some structure as a fortification, to take another example, has a different meaning and makes connections to different ideas than to call it a wall or a mound. The point is simply that the nature of historical evidence (it is in words) and the nature of historiographic descriptions (in words) puts both aspects of the historical process under the sometimes subtle influence of the language and culture of their times.

The conceptual presuppositions imported by using *our* language to describe *their* events can be a threat to accuracy. Historians in the present may just be imprinting their own society on the evidence of the past and describing their own cultural in the circumstances of the past. Some kinds of descriptive claims seem less vulnerable to this manner of subjectivity. In describing a battle, for example, the number of troops and the location of the engagement would be straightforward matters of fact, while the motives to fight and the strategies for victory would be more susceptible to the historian's interpretive influences. This difference may be simply correlated to the distinction between physical and mental events, but even in describing the physical, the choice of words begins the interpretive process. Describing the group of men as "troops" already suggests some intent and purpose in their minds. And as we move toward seemingly more neutral terms like "a group of men," we lose valuable information. This is the tension; meaningful and informative evidence is interpreted evidence, and that requires some conceptual input from the historian in the present.

Writing and reading historiography are social processes; the historian's social setting influences what is written and how it is read. It is helpful to distinguish between two aspects of a historian's society that are influential. Historians live and write in both the context of their public society and culture, and the context of their professional society and training. Historians are human beings studying human beings. They are situated in one society while studying another. Inevitably, the basic, implicit ideas, common sense, social structure, and ways of thinking in their own time will bear upon what they think and say about some other time. This is the influence of culture and public society. But historians are also professionals. They are trained and educated, both generally as historians and specifically with the background and expertise on the particular place and period they study. They usually adopt some professional perspective, some theoretical attitude, focusing, for example, on environmental history, or feminist history, or subaltern studies. This is what they wear so as not to be naked in the archives. And by wearing this perspective rather than that, they select, describe, weigh, and organize evidence this way rather than that.

Part of the training to be a professional historian is aimed at minimizing the influence of one's public social setting. In other words, of the two sources of social influence, the professional works in part to reduce the public. You learn proper methods, including respect for evidence, as a way to overcome your own pettiness and provincial nationalism, religious preconceptions, and so on. The same thing happens in training a scientist; respect for the method and an understanding of the basic theories helps a scientist check their folklore and idiosyncratic wishes at the door to the laboratory. Of course it's not perfect, and this talk of training the subjectivity out of scientists or historians begins to sound like a form of positivism in which historiography is just like science and both deal in pure objectivity. This extreme is unwarranted in science and even more unwarranted in historiography. Almost no natural scientist studies nationalism or religion, and so it is less likely these influences will slip back into their professional activities. Historians are studying exactly the things their professional training is working to exclude; it is harder to keep the personal out of studies of persons.

While professional training works to limit the influence of society in general, it inevitably introduces its own subjective aspects. Professional influences are easier to detect,

because they are more explicit. Historians can be identified with schools and approaches, as Marxists or cultural historians, for example, and so it is clear from the beginning how they will choose and emphasize the evidence. Knowing the perspective helps the reader to compensate for it. The epistemological question is whether the influence of expertise can be identified and transcended. Can we detect the tint of a historian's glasses, and then can we reconstruct things without that coloring, or at least through the tint of our own glasses?

This is the place to talk about paradigms and to refer to Thomas Kuhn's (1996) description of science. In science, Kuhn and others pointed out, evidence, the basis of testing and credibility of theories, is influenced by the professional mindset of the scientists, that is, by the background they have learned in school, by the experimental standards they acquire from mentors, by the sense of important problems to solve they inherit from advisers, and so on. This is the paradigm, and insofar as it controls the process and standards of justification, it can be epistemically insular.

Science is different though because its method emphasizes replication. This seems to be a paradigm-independent, objective standard. Either your results are replicable or they are not, and if not, then your conclusions are suspect. Historiography is at an apparent disadvantage in this respect. It studies apparently unique events and people, things that are over and done with and so cannot be replicated at will. There was only one French Revolution. Natural science, however, studies naturally recurring events. There are plenty of fruit flies to go around, and the same procedure can be done as many times as we like.

Repeatability is linked to a requirement of control; science depends on controlled experiments in which one or more of the relevant parameters is changed at will to see what effect it has on the outcome. But this is impossible in historiography. We simply cannot bring the First World War to a close again, but this time without putting the crippling monetary-reparation demands on Germany, to see if Europe would be drawn to war again so quickly. The methodological question is whether this disability puts historiography at an epistemic disadvantage. It is important to note that some sciences are similarly disabled. Astronomers have no control over the things they study. There is no remaking the sun, only at a different temperature. Geologists are unable to repeat many of the phenomena they are most interested in. The Sierra Nevada are unique, and there is no recreating the circumstances of their formation.

But the sun is a typical star, and the Sierras are mountains. There are lots of stars and lots of mountains, so to regard these examples as unique could be misleading. What then about the French Revolution? It is a revolution, and while there may not be lots of revolutions, there are others. To describe something as a revolution is to put it into a category, and thus to generalize. This opens up the possibility of testing claims about the French Revolution by looking at the details of other revolutions. This is a kind of repetition in the evidence. Looked at in this light, the issue of the role, status, and epistemic advantage of repeatability of evidence is not simple and not to be dismissed too easily.

We have been outlining the issues regarding historical evidence. Evidence is the informational means to the end of accurately describing the past. The evidence forms the building materials. We should look next at the building itself, the final product, the description of the past.

The Structure of Description and Justification

Narrative historiography sometimes has the stigma of being naïve, the style of dilettantes and buffs, and without the necessary theoretical sophistication to really contribute to our understanding of the past or the present. The philosophy of historiography attempts then to understand what narrative is, what it presupposes, and what it can and cannot do.

A narrative is the telling of a story. Events are told in their natural, that is, chronological, order, a sort of this-happened-then-that-happened description of the past. The story line is followed wherever it goes on its own, with the idea that the facts are self-organizing. The facts speak for themselves and develop their own themes and connections of relevance. It is not hard to see why this seems naïve. It ignores the need to select and interpret the evidence from the past. It is oddly like positivism, odd because the narrative structure of history and/or historiography is often the high ground taken by historians who oppose even comparison, let alone similarity, to science. But the passive role of the historian as collector and lister of facts denies the worries of subjectivity, and this is similar to pure empiricism that is associated with positivism.

A more sophisticated view of narrative starts by regarding the individual components of the story as the pieces of evidence used to construct the larger description of what happened in the past. Then we can acknowledge that the evidence, the building materials of the narrative, are indeed influenced by the historian. The professional influence, or, as modern historians often say, their theory, directs the selection and description of the individual pieces. In this way, each piece of evidence is a little theoretical, or as Nelson Goodman puts it, “facts are small theories, and true theories are big facts” (Goodman 1978: 97). The difference between what we call evidence and what we call theory is a matter of scope and connection to text. Evidence is specific in content and is closely linked to some particular text. Theories are more general in content and are not associated with any particular text.

This sort of give and take between theory and evidence can be accommodated in a narrative form, but it is now advisable to be explicit about the theory that is guiding the selection and presentation of the evidence. There are always presumptions about humans and antecedent knowledge about the past that serve as background knowledge and are influential in selecting, ordering, and linking the individual bits of evidence. There is still a story to tell, but the pieces are neither self-selecting nor self-organizing. The narrator has an active part in preparing and presenting the narrative, and the reader deserves to know what guides the process.

In other words, we in the present, we as historians, contribute to the story told about the past. There is an aspect of constructing the description. Important aspects of the message are made, rather than discovered. They are made by selecting, highlighting, connecting, and mindfully describing the events and the people in the story. Noting this invites the possibility of relativism or constructivism. If we contribute to the story, then it may seem to be as much about us as it is about people in the past. There are varieties of relativism that should be distinguished. Descriptive relativism would claim that we can only describe the past in our own terms and so all we can say about the past is determined by ourselves and our language. Epistemic relativism would claim

that all we can *justifiably* say about the past will be determined by ourselves and our current theoretical views because the evidence itself is theoretically laden. Metaphysical relativism claims that all there is to the past, the facts in the sense of the events themselves, is determined by us. We create the past by describing it.

The key to sorting out these claims of relativism and constructivism is in clarifying the role of theory and the constraints of evidence. There is an important difference between theory (and, more generally, the ideas of historians in the present) *influencing* the evidence and theory *determining* the evidence. There are still words on the pages of documents from the past, and any description of the past, any theory proposed, must make sense of these words, at least of most of them. In this way, the evidence constrains the descriptive process. The description cannot be pure construction, nor is it entirely determined by factors in the present. But the issue for philosophy of historiography is to precisely determine how, and how much, the historical evidence constrains the historiographic description. Theory influences evidence, but it does not completely determine it. And evidence influences theory, but it does not completely determine it. Somewhere in the vague reciprocal relation is the precise description of the structure of historiographic knowledge of history.

This structural issue can be linked to a central concern in more general epistemology, a contrast between foundational and coherence models of justification. The need to interpret evidence makes it implausible to regard it as epistemically foundational. The evidence can only justify a theory if the evidence has been justified. But even in a coherence model of justification there are important constraints. The lack of foundations does not mean anything goes. The challenge, in epistemology as in philosophy of historiography, is to specify the role of evidence, giving it the appropriate epistemic and informational authority to increase the likelihood of accuracy.

When coherence is used as an epistemic virtue, broader coherence is better. This has a social manifestation in that broad agreement on some historical description is some reason to think it is accurate. Peer review, the academic version of public justification, enforces a coherence within a network of generally accepted ideas. But this agreement may have been, in some cases, built in and reinforced by uniformity in education. To convince your peers is to convince those who were trained as you were. Broader coherence, and hence more justification, would be in convincing a larger society, your close peers, that is, your theoretical allies, and those outside your school of thought. Better yet is to write genuinely enduring descriptions of the past that will be agreeable to future schools of thought. This is real inter-theoretic coherence. The idea is to avoid, as best we can, the narrow, teleological, and parochial point of view, such as the Whiggish point of view, that leads to inaccuracy.

The underlying proposal here is that objectivity in historiography is to be found, at least in part, in intersubjectivity. Justification of historiographic descriptions is, at least in an important part, a matter of informed agreement.

Explanation and Understanding

There is more to historiography than simply describing the human past; it is possible to explain it as well. In addition to knowing what happened, we can try to understand

what happened. This raises the question of the nature of historiographic explanation, and again, the comparison to science is a common and informative approach. It is important to note though, that scientific explanation is not so clearly understood itself, and there is much disagreement and debate within the philosophy of science regarding the structure, content and epistemic status of explanation.

Carl Hempel (1942), a pioneer in the study of scientific explanation, proposed a straightforward application of what he took to be the basic scientific model to explanations in historiography. This is the covering law model of explanation, so-called because an event or recurring phenomenon is explained by showing that is an instance of a general law, that is, it is covered by the law. For example, a helium balloon rises in air. This is explained by citing the law of buoyancy, along with the specific conditions of the case that show it to be covered by the law. The weight of the balloon is less than the weight of the same volume of air, and the buoyant force is equal to the weight of the displaced fluid, so the net force is up. This is, by Hempel's account, a paradigm scientific example. It requires a law of nature.

In many cases, the specific conditions are unstated. Why does the balloon rise? Buoyancy. In some cases, the law itself is unstated. Why does the balloon rise? Because it weighs less than the air it displaces. The law is unstated, but it is nonetheless at work as a premise in the argument. Sometimes the law is statistical, and not surprisingly, these cases are trickier. Why did this atom radioactively decay? Because of the laws of weak-nuclear forces. But this other *identical* atom has not decayed. The law in this case is inherently probabilistic, but it is still a necessary component of the explanation.

The question is whether historical events and phenomena can be, or should be, explained this way, by citing the law that covers the instance. Examples of covering law explanations are easy to find in science. They are much harder to find in historiography, and those that are suggested often seem forced and artificially reconstructed to fit the form of covering law. This is because laws of human behavior are scarce, perhaps nonexistent. Insofar as historical events are unique, there will be no laws to cover them. But the uniqueness is itself an issue. There was only one Peloponnesian War, but there have been many wars. Important descriptive words are used to cover a variety of cases, otherwise we would never understand what was being said of the past, and that invites generalization.

But not all generalizations are laws. Some generalizations are merely analytic. All revolutions bring about wholesale change. Sure enough, but this is simply expanding on the definition of "revolution." It is not a law, and it cannot explain anything. Why were there such dramatic changes in Russia at the beginning of the twentieth century? Because it was a revolution. That simply restates the question.

Some generalizations express merely contingent repetition. These are not laws either. All Presidents of the United States have been male. True, but this generalization doesn't explain why Chester A. Arthur was male.

Laws, in addition to being generalizations, invoke necessity. It's not just that all As are B; it's that all As *must be* B. So using the covering law model of explanation must assume some important aspects of necessity in human action and some essential aspects of human nature. It precludes the possibility and ability to explain some event in terms of its uniqueness. It ignores any explanatory value in the idiosyncratic

aspects of human behavior, and it disregards the efficacy and importance of human agency.

As an alternative to covering law explanation, it is proposed that narrative itself is a kind of explanation, and that narrative is the only form of explanation suitable to history. Narrative makes no assumptions about laws of human behavior or essential human nature. It does not rely on generalization. Rather, it highlights human individuality and allows for free will. Historical events are explained simply in virtue of fitting into a narrative. They are explained when they are situated in their context. Description in narrative context *is* explanation, and in this way, there is no difference between description and explanation.

In this model, there is no difference between knowledge and understanding. Like putting together a mosaic, seeing how an individual tile fits in with its surrounding tiles is to understand both the role it plays and the correctness of the fit. We see what it means when we see that it fits.

Narrative explanation suits the situation of historiography, but not science. This is because of the human aspect of history. Historians have a special connection to the thing they study, since they are humans studying humans. Biologists have no intellectual rapport with amoeba, so understanding amoeba requires more than merely describing them. Collingwood emphasized the essentially human aspect of history and the resulting difference between historiographic and scientific methods. "All history is the history of thought" (Collingwood 1993 [1946]: 215). The historiographic method therefore requires attention to the mental aspect of actions. The historian must "think himself into this action" (p. 213). This is no subjective empathy he is describing, but a "critical thinking" (p. 215) in which one "re-enacts [the action] in the context of [the historian's] knowledge" (p. 215). That is, the past action, including the mental component, is described by situating it within our description of the historical context. It is described within the larger narrative pattern, and to describe the ideas and events in their narrative place is, at the same time, to understand them.

Collingwood's general method of historiography is similar to Thomas Kuhn's (1996) prescription for the historiography of science. Full immersion in the paradigm of the time is the only way to understand what past scientists were doing. Past theories must be described on their own terms and evaluated by their own standards. Anything else would be Whiggish.

There is an inherent circularity in Collingwood's method. Individual events and actions are understood by being situated in the larger context. But the larger context is understood by being built of individual events. It is a hermeneutic circle, and perhaps the only way to understand other people. Understanding humans may be a fundamentally different process than understanding the mindless objects studied in natural science. Historiography may be fundamentally distinct from science.

Summary

The important issues in analytic philosophy of historiography show up in a variety of ways and places. The issues are epistemological: accuracy, objectivity, and the ability to describe the human past. These concerns surface in describing the status and

structure of historical and historiographic facts, evidence, and justification. One quick way to summarize most of what is at stake is to distinguish between discovering and constructing the human past. How much of what we say about what happened in the past is a matter of discovering what in fact happened, and how much is a matter of imposing ourselves on the past? If you can answer that, you will make progress in philosophy of historiography.

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Philosophy of History

ZDENĚK VAŠÍČEK

“Where do we come from? What are we? Where are we going?” repeated Paul Gauguin with the age-old questions in his famous painting of 1897. Where do we stand today in regard to these perennial meta-narratives and how do they influence history? There are too many answers. As William James acknowledged, philosophy is an umbrella term for any question which has yet to be answered satisfactorily. The philosophy of history proposes one kind of answer. Here, I will try to describe the character and the forms of its solutions.

Let me start by clarifying the term “philosophy of history” itself. Today, the field is divided into “substantive” and “critical” philosophy. “Substantive” philosophy of history addresses history directly, as an object of its statements, and tries to formulate general statements about the past. “Critical” philosophy of historiography, by contrast, studies the propositions of historiography about the past, including the statements of prehistory and archaeology. However, these distinctions are not clear cut. There is much substantive philosophy in historiography proper, and to some extent the substantive philosophy is also assumed unreflectively in critical philosophy. Before proceeding any further, therefore, let me try to clarify the relationship between the past itself and history, historiography, and philosophy.

The Term “Philosophy of History”

What do we understand by philosophy of history? The term is not exact. Its fuzziness may suggest that it is perhaps a collective concept. It was used first by Abbé Bazin in the title of his book *La philosophie de l'histoire* published in Amsterdam in 1765. The author was in fact F. M. Arouet, usually known as Voltaire. Philosophy of history has many near-synonyms, notably: philosophical history, historical philosophy, (meta)theory of history, theoretical history, logic of history, meta-history, historiography, and anachronic historiography. Some time ago the term *histoire raisonnée* or conjectural history was also popular. There is little agreement about the terminology, but every expression indicates some kind of link between philosophy and history – a problematic linkage, since the terms represent an opposition, not unlike that between synchrony and diachrony, generality and singularity, or holism and individualism. The

basic distinction between historiography and philosophy was firstly made by the ancient Greeks. They treated philosophy as theoretical knowledge, historiography as empirical knowledge. However, as Schopenhauer and Nietzsche observed, what can be defined has no history. Jacob Burckhardt put the matter as follows: the philosophy of historiography is a “centaur,” a *contradictio in adjecto*, since historiography (i.e., coordination) is not philosophy and philosophy (i.e., subordination) is not historiography. Yet as Burckhardt claimed, we have much to be grateful for to this unlikely centaur.

The Terms “Philosophy” and “History”

The philosophical and the historical approach taken in themselves are no more clear than their combination for the very reason that they are both fundamental. As the *Encyclopaedia Britannica* has it, philosophy cannot be defined. Nonetheless, I would follow R. G. Collingwood in saying that “philosophy is thought about thought.” Applied to our case, this dictum means that the philosophy of history is meta-thought about history, a kind of theory of history.

The situation is further complicated by the fact that the often the term “history” refers to both the field of knowledge, its results, and what is spoken about. There are many profoundly different views of the subject matter. On the one hand, we find the conviction that the object of history encompasses the whole past of the human species, which has – just like our personal past – no limits. From a non-limited past the historian can choose only according to his own interests or his ideas about the past. We then interpret a selection of historical traces (constructivism, anti-realism). In this case, the historian performs as a creature conditioned by his language, culture, epoch, and personal history.

On the other hand, the opposite cluster of attitudes claims that only certain parts of the past comprise the object of history (what is not progressive is not an object of history, as Schelling observes; history assumes historicity (*Geschichtlichkeit*) and becomes an object of phenomenology). History, which is independent of us, resides in the past as an ore lies hidden in a rock. It can only be noted in terms of regularities, probabilities, trends, laws, invariants, structures in the object and by conscious and free action on the part of the observer. This is a fairly contradictory bunch of attitudes. They are usually supplemented by the assumption that history must have some sort of substance, substratum, a constant frame of reference, transhistorical universals – a God’s eye point of view. History, which is independent of us, thus requires an independent historian. According to Droysen, only thoughtlessness is really objective. He could therefore speak of the historians’ “eunuch objectivity.”

In addition, the conceptions of history (history as object, knowledge about the object, and the discipline) differ radically and are subject to change. To simplify the matter, we can say that in the beginning there prevailed a narration about a series of events, which were mutually only loosely connected and influenced by transgressive, supra-historical, transcendental entities. The goals inherent in the representation of the past were pragmatic (whether good or bad); but in eighteenth-century Europe, history was newly conceived as an independent complex whole, which had its own dynamics.

Transcendence was put aside; history explained itself. Thus we find history about itself and for itself, history in general. This is a new, distinctly substantive, conception (Brunner et al. 1975). Today, this model is being abandoned. The result is that in contemporary thought, on the global scale, many conceptions of history meet: we live in different “histories.”

Historiographical Production

As Kinser has noted, the greatest impetus for the creation of historiographies has come from non-historians or from unorthodox historians, so to speak, from the outside (Kinser 1981). Abstract and general terms are usually adopted from the socio-political language of the historian’s period. In this way historiography is a part of the discourse of its own period, whose basic terms serve as the ultimate point of reference with shared symbols and metaphors. The philosophy of history in the eighteenth century was conditioned by the development of collective singulars (see later). The historians’ individual attitudes, then, are shaped by their epoch – Marrou calls historical theory “a position the historian adopts *vis-à-vis* the past: the choice and delineation of his theme; the questions posed; the concepts used; and above all the types of relationships, the systems of interpretation, and their relative value – i.e., the personal philosophy of the historian” (Marrou 1954: 187). Historical orientations and schools in general arise in similar ways. This is not usually viewed positively by the historian: Febvre considered philosophy in history to be “*un crime capital*.” For his part, the philosopher Fichte compared the business of the historians to picking peas.

Critical Theory of Historiography vs. Substantive Philosophy of History

The reflection on historiographic practice gives rise to methodology and a kind of theory (*ars historica*, historicis, *Historik*), including its “cook-books.” This was the main way in which – above all, thanks to Rank and his school – nineteenth-century scholarly historiography was constituted as a system of procedures for inferring knowledge from historical evidence (i.e., the remains of the past). The systematic reflection on categories such as contingency, objectivity, and causality borders on this methodology and on the philosophy of history itself. If the problems under review relate to historiography rather than to history itself, we are dealing with historiology, the philosophy of historical writing, the philosophy of historiography or with formal, analytic, or critical philosophy of history, or with what the French call *l'épistémologie de l'histoire*. We can further subdivide the field into the philosophy of scientific history (interaction with evidence) and the philosophy of historiographic interpretation (Tucker 2004: 10).

If it is “philosophy” which we emphasize in the expression “philosophy of history,” if the philosopher seeks for backing or inspiration for his statements in the past, if we focus on the object of history – we are then dealing with substantive (substantial, metaphysical, speculative, synthetic, material, chronological, classical, traditional)

philosophy of history (including “philosophy of history” pure) or with one of its products (historical meta-narratives, upper-case history, universal history). Substantive philosophy of history is not limited to the all-encompassing constructions such as the great, so-called “classical” philosophies of history created by Augustine, Vico, and Hegel; it also includes partial, lesser, statements and assumptions. The distinction between the opposite kinds of philosophy of history is also based on the differences between history and historiography, realism and constructivism, speculation and hypothesis. These very often overlap and engage in mutual dialogue. Clearly, historically the substantial approach dominated at first, given the limited amount of available knowledge of the past, the poor scholarly credentials of historiography, and the underdevelopment of the social sciences.

The past (and often the future, too) was initially the object of mythology, tradition, folklore, and epic literature, and, of course, religion. Historical writings came later. To begin with, both in myth and in historiography, events were presented as a story, ordered in cycles or in linear form, and were explained by reference to transcendence.

In ancient times, there were few methods available other than speculation. By “speculation,” Plato understood a statement referring to something we cannot experience directly, something that is not a self-contradiction; which does not conflict with well-known facts; and which cannot be refuted. These are all assumptions that cannot be unambiguously deduced from the field of research, and which are usually formulated in a particular vocabulary. Such speculation accepts the existence of several solutions to the same problem, solutions that are mutually exclusive, but, nonetheless, do not refute one another.

Even statements which are really quite basic, and therefore cannot be falsified (for example, whether history is contingent or not), contain speculative attributes. In larger and less clear-cut cases, such as World History, the speculative philosophy of history has been and continues to be inspirational, whereas the philosophy of historiography has little to offer it. Moreover, given that scientific historiography relies strictly on sources, we encounter many “blank spots” in history; spots whose properties are unknown, yet have influenced history in important ways. What shall we do with them?

Behind the particular stories of the substantive philosophies of history there are hidden sets of mutually exclusive assumptions. These all create a space for speculating about history. This goes not only for history in general but also for particular histories, including Lyotard’s “*petites narratives*.” Such mutually exclusive assumptions can be summarized as follows:

- Philosophy of historiography refers/does not refer to a-historical entities.
- It makes references to transcendence that may create, or finish, or intervene, or not intervene in history. It may also refer to other extra-historical factors, e.g., nature, or powers such as ideas, laws, structures, or human nature.
- Transcendence may act throughout history as in theism or only at the beginning of time as in deism.
- Transcendence’s plans may change or not through history.
- Transcendence’s plans may or may not be knowable.
- Human nature may determine or be determined by history.

- The explanation of history may lie at the beginning or at the end of history.
- History must have a beginning; however, it does not necessarily have an end, there may be an eternal return or it may assume an ideal state for the rest of eternity.
- The end can be utopian, apocalyptic, or mundane.
- History may be continuous or discontinuous.
- The course of history may be progressive, regressive, or stagnant.
- The course of history can be cyclical (i.e., repetitive), linear (i.e., unique), or vertical. Cycles and linearity can be combined, for example in the Marxist spiral view of history.
- There may be regularities, plans, laws, human nature in history or history may be chaotic.
- Nothing significant may change in history, or changes may exist only on a small scale, or history may be the story of significant changes.
- History may or may not have a meaning or goal for both the individual and humankind.
- If history has a meaning it may be intrinsic or extrinsic, discovered or prescribed by the philosopher.
- The decisive subject of history may be the masses or great individuals.
- Immortality may or may not exist, and more significantly immortality may be individual or collective. Collective immortal entities may or may not be then the subject of history.

These opposites form the axes of n -dimensional space, in which it is possible to localize particular philosophies and eventually to try to classify them in typologies. Loewith, for example, differentiates two basic types, or clusters in the philosophy of history: cyclical and linear. The cyclical conception, part of which may be the argument about the essential immutability of events, is usually connected to synchrony, nature, individuals, existence, lyricism, and is also associated with Chinese thought. Linearity, on the other hand, is usually connected to diachrony, transcendence, holism, substance, epic poetry, and European thought.

The Meaning and Function of History

The teleological meaning of a thing is its relationship to something outside itself – function, value, idea, substance, or goal. This relationship is accompanied by an assumption of some kind of neutral place, observer, or a certain end. Thanks to such references we gain the means to describe things as well as to explain them. Regarding any complex whole we may also ask about the meaning (or rather about the function) of its parts; in such cases, it is the whole that serves as the reference point. This also applies to the concept of history as an independent whole. But where shall we locate the observer? Inside or outside? This is a crucial question.

Even understanding as a matter of interpretation or hermeneutics must eventually relate to something: for example, to the “life-world” or “personal existence,” which, alas, disappears at the very moment of its conceptualization. This is the case with all phenomenological and existentialist constructions of history. The philosophy of history

also requires other props: vague entities such as Religion, Ethics, Aesthetics, Language, Text, and, lastly, Culture. Of course, there are more of these, but they are all markedly historical; yet they all have in common a certain vagueness, without which they could not serve as an essential reference. Each of them introduces basic metaphysical assumptions.

For Descartes, the subject was entirely a-historical, whereas Hegel identified it with history itself. Once the subject is rigorously historicized or conditioned by structures, it cannot serve as a reference. Thus, Foucault could claim “the death of the subject,” and, consequently, the death of meaning. Actions of the individual are certainly subjectively meaningful, we can also talk about a social meaning in the case of social groups. Identification and legitimization are created according to a social position. The problem, however, is how we can give meaning to long-term courses of events, to epochs, or cultures. The meaning of history, especially the meaning of national histories, is often represented as a line expressing continuity and direction, which also implies the stability of such a history.

The ever-inspiring and vital question concerning the meaning of history has been asked in many different ways. Philosophies of history are adjusted to the demands of nations, states, and social movements; they change and react to every important event, such as revolutions, wars, and recently, the Holocaust, the end of the Cold War and the beginning of a new conflict between the west and the east. These philosophies reflect their present more than the past. The dependence of this universal knowledge on the mood of a period and on subjective features like experience and personal attitude is quite disturbing.

The meanings of life, society, and history intertwine, especially in literature. Literature seizes upon historiosophy literally, both in the words of its characters and in the plots and forms of literary works. In this light, it seems quite appropriate that Leibniz planned to sum up his philosophy in a novel. Today, the desire for general meaning (which, of course, includes the meaning of history) remains as topical as ever. This leads us back to Nietzsche’s question: what meaning does historiography have for us? And what meaning does it have for historians?

The Evolution of Substantive Philosophies of History

The proof of the pudding is in the eating. It will thus be helpful to sketch at least a brief survey of the evolution of the substantive philosophy of history, treating the most important movements and authors, with special reference to the relevant chapters in this volume. More detailed information can be found in the survey texts contained below in the suggestions for further reading. Before I begin, however, let me stress that we should not always expect a comprehensive system when examining this topic. Often, we only encounter fragments, implications, assumptions, and the echoes of earlier arguments. Some authors believe that the positions have evolved over time. Various authorities contradict each other. All this permits many different approaches. I will attend most to origins, since it was in the earlier periods that some of the perennial constants in the philosophy of history first appeared. Notwithstanding their age, most of us still think and act according to these ancient models.

Mythical times

The more we look back into the past, the more time and space merge. Past, present, and future were barely differentiated for the ancients. The *arché* (ideal state) was important both as origin and principle. Mythology, in which this *arché* is expressed, is the only form of knowledge which explains itself, and therefore cannot inquire into itself and its meaning. The main tool of myth is narration, the story. The poetics of myth, biomorphism, the function of this model, and its claim to explain everything – all these remained basic to the later, explicit philosophies of history for many years to come (Schlobach 1980).

Antiquity

In ancient times, a further medium appeared – the epic (Sauge 1992). In an epic, the heroes are defined by their deeds; however, these are not assigned to them unambiguously. To explain events, an epic must also refer to a higher level. In the first place, this is justice. Thus Anaximander maintained that things are born and die according to necessity: “they suffer punishment and give satisfaction to one another for injustice according to the order of time.” Whereas in the oral mode of the epic, only facts and meanings accepted by society could be preserved, the invention of writing permitted the author to present incongruous events and different opinions. Mythological unity fell apart; the object and subject appear, the personified ego. The epic and its formal rules were followed by prose – the first prosaic text (most of which is made up of digressions) was that of Herodotus. At this point in time, historiography slowly began to surface; later, historiographic records appear; later still, the idea of history itself emerges.

Justice in an epic is exercised by the gods, who may guard or punish man; yet they are usually subjugated to a higher order – namely to fate. This scenario may involve factors such as guilt, punishment, and predestination. Justice, the epic holds, must exist, yet it need not apply to individuals, but, rather, to groups. As we will see, at this point east and west diverge. Gradually, with the development of writing, more typical problems of the philosophy of history emerge, for example, how to distinguish good from evil. Already in Herodotus, we find the conception of a common law, an early version of natural law theories that assume a universal human nature, which are vague and controversial to this day (Pompa 1990).

Various concepts concerning the repetition and succession of historical events appeared in antiquity as analogies to the life of man and to natural phenomena, such as floods and fires. Visions of a world fire, a giant conflagration that would consume and end all things (Greek *ekpyrōsis*; Germanic *ragnarök*) were also common. Antiquity also recognized a certain order in change, as did both Herodotus and Aristotle. A cyclical understanding of the past was already common in Greek thought as well as in late Roman philosophy, although it was not actually common in ancient historiography. As to the ideas of a total end to all history, of a direction and a goal, we only find this among the Pythagorians, the Orphists, and the Neoplatonists. With time, the relationship to transcendence in historical writing became increasingly weak and formalized. The concept of development was not yet known, nor were its mechanisms understood, and historians made do with ideas of human nature and with individual qualities as a

reference. These were far more individuated in the west than in the east, a fact which reflects the castes, centralized governments, and formal behavioral patterns of the Orient. The concept of an immutable nature is accompanied by the absence of any goal or direction, and thus by the absence of history and its meaning: the world does not change, or perhaps just a little; the past does not determine us, it only provides a set of examples. Thus the past and the future lack interest, only the present is decisive – a point which can be connected to those thinkers in both the east and the west who have no interest in the afterlife. In the west, the meaning of life is personal, whether it be located in hedonism for the Epicureans or in virtue for the Stoics. The result of this approach is a pragmatic historiography. Its presentation is markedly literary, not only in style, but also in the organization of the text as well.

China

Chinese thought issued from the state of the world, rather than from the things in the world. It is holistic and its argumentation is rhetorical, it stresses figurativeness rather than logic. As it does not know transcendence, it gives no emphasis to religion. For the ancient Chinese, the order of the world, nature, justice, and morals all coincide. That is why we do not find much in the way of myth or epic in China. Synchrony predominates – and, hence, in literary terms, the lyric. The concept of the golden age prevails as an ideal. Change is cyclical. Only the school of Legism went beyond this framework. China thus developed a very particular concept of past, present, and future. The past stands for a kind of ideal; however, what really matters is only the present. The future lacks interest, and is limited to matters of individual improvement and personal betterment. Life is planned only within the family and according to prescribed patterns; the city and the state are extensions of the family.

The exemplary model for historiography was set by the oldest annals of The Spring and Autumn period (722 to 468 BC). Historiographic records came to be regarded as moral judgments. Since the Han Dynasty (206 BC to AD 220), when most forms of decision were vertical and everything including the past came under the control of the state, the past was dominated by official historians. Their histories were written as encyclopaedic handbooks for State officials. The axis of history was represented by the succession of dynasties granted by a heavenly mandate. This central idea formed the basis for a set of historiographic works written as specialized treatises on particular fields, such as agriculture, warfare and art, as well as annals and biographies. Facts were not ordered in linear form, i.e., in an epic way, but paradigmatically and statically. The relationship to the monuments of the past was that of the antiquary and conservationist.

India

In India, on the other hand, history was always distinctively conceived in an epic way. The oldest Indian epics, such as the Mahabharata, mainly portray the fate of gods. Gods constantly intervene in human history in order to mete out justice. Everything recurs again and again, and individual cycles are arranged in descending phases.

From the time of the Upanishads, pessimism began to appear. What happens in time is only ephemeral; only eternity makes sense. All things are subordinate to the karmic

law, which ensures absolute justice; this is established by a series of individual reincarnations, which end in dissolution or bliss. In this regard, Hinduism accords with Buddhism. However, the cosmic order can apply in society as well – the movement from confused, even impulsive, relationships between individuals towards the state in which society is controlled by the will of its rulers and warriors; and thence to the final state, where individuals gain peace and society gains harmony.

The karmic law rules history, which is a history of individuals, not of society. The common past has no meaning for an individual; the only thing important to him is the present, which influences his future. Such a conception can be called individualist; however, it is far removed from western individualism. This conception of history leaves little room for historiography and, indeed, we have only very few records of older Indian history.

Monotheist religions

The first great world religions are closely related to epic poetry and written records. Their Grand Narrative assumes a beginning, a middle, and an end – it is linear. The end represents an explanation of particular peripetia and is identified with their meaning. The great theist religions (Judaism, Christianity, Islam) basically share the outline given by Zoroaster in the twelfth century BC: a perfect, eternal, and infinitely good God created both nature and man. He established a moral order and endowed human beings with immortality and free will. History is a fight between good and evil, into which God can intervene. History is divided into two main parts by the advent of a Prophet or Messiah, and a Final Judgment will follow the final defeat of evil, whereupon an eternal empire of happiness will begin, with hell as its complementary supplement. Of course we can find many variations on this scheme. Thus, for example, individual immortality was not part of early Judaism, and still plays relatively little part in the religion.

This schema, in particular, constitutes the basic outline of the theology of history. The scheme was later expanded by speculation, and by the interpretations offered in various canonical texts. Thus, for example, in the early middle ages, St. Augustine claimed that because God created the world in seven days, history had to have seven stages. For Augustine, reality is divided into the City of God, which is immutable in time, and the Earthly City, which has no real meaning. There is therefore no perfection possible in worldly history; its linearity is dedicated to *historia sacra*. In the high middle ages, Joachim de Fiore distinguished three phases, imitating the tripartite God. The changes in history involved a development from God-Father to God-Son and the Holy Spirit. This is a process of becoming spiritual, a path to perfection. Some thinkers also applied John the Baptist's apocalyptic prophecy to history: for them, a final battle with the forces of evil will be followed by Resurrection and the Final Judgment.

Various problems which the philosophy of history has tackled in the modern period appeared already in the middle ages: The relation between good god and evil in the history; the attempts to justify God in the form of a theodicy; and the problem of the relation between God's prescience (involving predestination) and free will (*libre arbitre*). In other words, medieval historiography asked whether the subject is a product of history, or the other way around? In the late middle ages, a concept of history as linear succession leading to a certain end gradually turned into concepts of perfectionism,

progress, and endless evolution. Loewith treats the later, progressive philosophies of history as secularizations of Judaism and Christianity (Loewith 1948).

Early modernity

The usual succession of philosophies of history is interrupted by the Arabic historian Ibn Khaldun, today rightly portrayed as a father of sociology and the philosophy of history. However, his epoch-making work *The Muquaddimah* (1375–1378) did not find a follower in Islam; and in Europe it became known as a whole only in the 1850s. For Ibn Khaldun, history was an autonomous cyclical process, independent of God's plan – a man's life is futile and trifling. He notes that a nomadic people is distinguished by strong group cohesion, which, in connection with religion, can gradually lead to a sedentary life, followed by the centralization of power, the development of civilization, and the gradual loss of original cohesion. This leads to a mild decline and the beginning of a new historical cycle. Ibn Khaldun is remarkable in that he begins with the geographical and climatic conditions of history, as well as taking into account economic and cultural relations, institutions and society as a whole.

Attempts to explain history without reference to the transcendental proliferated in early modern Europe. The widespread controversies between states, religious wars, and overseas exploration provided new possibilities for comparison, and laid the groundwork for some form of religious and cultural relativism. We can also observe the development of legal thinking, which led to an advanced form of rhetorical argumentation. Likewise, the interpretation of human behavior changed. In the sixteenth-century Machiavelli and Francis Bacon both shifted the perspective from that of moral philosophy towards the empirical and historical study of human customs, especially political customs.

Let us take France as an example. Jean Bodin connected legal and political theory, considered geographical and climatic conditions, speculated about the nature of man, and had an idea of progress; without knowing it, he vicariously repeated the pattern of thought of Ibn Khaldun. Later, both Pasquier and de la Popelinière actively participated in the public life and were educated in law. They used rhetoric as a model for historical argumentation, and – even if largely by assertion – had the merit of enlarging the object of history (*histoire parfaite, integra*) and postulating a world history. In this they followed the project of *historia universa* inaugurated by Baudouin (Huppert 1970). Later, in the eighteenth century, Montesquieu considered the social, economic, and institutional aspects of history, whilst Voltaire examined the history of culture, customs, and knowledge. The scope of history was thus expanded not only in time and space, but also in its depth and range. At the same time, the possibilities of rational explanation increased. These changes were fundamental, albeit the philosophy of history as such had not yet been formulated and founded.

The grand turn in contemplating history was brought about by Giambattista Vico, a thinker little known in his time. His is the first attempt to conceive history as an independent entity – it is *historia ideale eterna*, complete with its own laws and stages. History is intelligible, since the human who makes history can also know history. Thus, Vico is the first to proceed not only from *what* is expressed, but also from *how* it is expressed. Remarkably, he integrated most of the then current spheres

of knowledge into history, i.e., philosophy, law, and politics, he historicized them (see chapter 40, this volume).

New conceptions of history

In the eighteenth century, history as such began to be reflected, giving rise to philosophical history (Chladenius), world history (Iselin), *nexum rerum universalis* (Gatterer), theoretical history (Steward), hypothetical history (Rousseau, Condorcet), and a priori history (Kant, Fichte). This change can also be seen in the terminology. The singular form *historia*, *die Geschichte*, *l'histoire*, became a collective plural representing history as a whole. The effects of this linguistic transformation was to create general concepts (which need not be closely linked to space and time) connecting past, present, and future.

This movement was universal. Even nature was historicized. Kant set the cosmos in motion, Buffon did the same for animate nature, Winckelmann invented art history as a reflection of individual stories, and with Friedrich Schlegel, the history of literature emerged as a new criterion of (and for) literature. Thus the personal, the cultural, and the historical were newly connected.

The new concept of history, the attempt to explain everything through history, led to a transformation of substantive philosophies of history. From the end of the eighteenth century, the different substantive philosophies began to move in different directions, and reached their heyday in the nineteenth century. One direction is markedly linear and progressive, as with Turgot, Ferguson, Condorcet, and Saint-Simon. These were followed by the positivists, who were inspired by natural science, above all Comte and Spencer. Marx, too, can be seen in this light.

For Kant, the ideal society is represented by the civil state with a perfectly just constitution guaranteeing the rule of reason, i.e., the minimum of oppression compatible with the maximum freedom. However, this was only a regulative idea to be followed, not a historical law. Kant's perfection has no end: we always work for the future happiness of the people. In 1749, Rousseau, by contrast, postulated hypothetical naturalness as a regulative idea, from which, however, civilization departs. For Herder, differently again, history was a step towards the attainment of humanity and perfection via the linearly conceived development of individual national languages and cultures. Nonetheless, for Herder, the most decisive point remained the potential of each person and each epoch to fulfill its potential – in fact, a vertical type of view (see chapter 41, this volume).

In the Romantic era, Schelling and Hegel had a similar point of departure. They conceive of nothing but God (the Absolute, Being): for them, human existence is an expression of the self-consciousness of Being, and therefore also of freedom. Whereas Schelling's views led him to a mystical theosophy, Hegel treated these points systematically. He developed a form of Panlogism, in which he identified the Absolute with Reason: reality is rational, he argued, and therefore what is rational must also be real. For him, the state is an incarnation of morality and justice; whilst individuals are only its constituent parts. World history is then seen as a succession of individual nations and states, leading to perfect self-consciousness and freedom (see chapter 42, this volume).

Whereas Herder shifted his attention towards individual nations, German historicism like Vico in the previous century turned towards epochs as independent coherent formations, each with its own values. All such epochs, though individual and incomparable, are equally close to God, i.e., they maintain a vertical identity. Modern forms of historicism develop the consequences of this idea. They set out to study short periods, varying perspectives and values, but avoid using their own values as measures. Jacob Burckhardt posited anthropological constants and searched for what is repeated in a historical epoch, what is perennial and typical. The concentration on individual wholes was continued by thinkers who consistently differentiated linearly organized socio-economic formations (Marx) and then, in later thinkers, by analyzing separate cultures or civilizations (Danilevskij, Spengler, Toynbee).

Gobineau and Marx aimed to study the driving forces of history. The former found them in race as a biological ground, the latter in economics and the class struggle (see chapter 47, this volume). The encounter between practicing biologists and economists significantly affected the whole of the philosophy of history in the twentieth century. Popper was right to dismiss all such all-embracing philosophies, pointing out that their predictions are mere prophecies. Inevitably, sociologists often proceed by writing history retrogressively. Alternatively, the empiricist John Stuart Mill preferred induction; and Max Weber, preferred historical comparisons. It was Weber who introduced classification and the idea of "ideal types" into historiography. This is a generic concept for an ideal intellectual formation, which he used to attempt to connect explanations with understanding. Sociological and anthropological ideas regarding deep impersonal and inherent law-like generalizations came to the fore especially in the second half of the twentieth century: for example, modernization theory, Braudel's concept of the three levels of history, and structuralism. Even more recent is Gellner's macrosociological outline of world history. In terms of function, these theories cannot be distinguished from old substantive philosophy of history.

History itself became a key term of the socio-political vocabulary in the course of the nineteenth century, and even became an ultimate point of reference as a vulgarized, ideological term. It was personified as a river, as a mother, was gifted with strength and power, was identified with providence, and became increasingly defined in opposition to man. Burckhard's reaction is hardly surprising: he treats history as a continuum without direction and end; likewise, Nietzsche's standpoint, in which he turned to what he called "*das Unhistorische*" (the unhistorical as a basis of history), "*das Überhistorische*" (the suprahistorical) and the eternal return ("*die ewige Wiederkehr*"). Likewise, Schopenhauer and Kierkegaard situated themselves besides history.

Historiography gradually became increasingly a part of history itself, and was thus also historicized – its statements were relativized and brought into a temporal and cultural context. As part of this process, questions regarding the nature of historical knowledge were posed more and more frequently. This can be understood as the starting-point of the philosophy of historiography. Two directions were taken initially. Firstly, the problems of historical synthesis were thought through (de Barante, Droysen, and the Synthèse school represented by Berr). Secondly, questions were posed which not only addressed explanation (which implied the role of a historian as an observer), but regarding understanding (i.e., including the role of a participant). Understanding, retroactive experience (*Nacherleben*), Dilthey's stream of life – all these

ideas then came to be supplemented by a fundamental differentiation between the ideographical, i.e., a study of the particular, the nomothetic, i.e., a study of inherent laws, and knowledge (Windelband, Rickert; see chapter 43, this volume). Simmel then attempted to solve the question of the possibility of a historical scholarship as such.

For Husserl, the historical a priori is a structure of common human consciousness, in other words that which is common to all forms of the life-world. Heidegger by contrast pays attention to spatial-temporal existence (*“Sein zum Tod”*). In contrast to his teacher Husserl, he proceeds from individual, not social experience. After Husserl and Heidegger, some forms of hermeneutics found the basis of the life-world in a linguistic structure, as in Gadamer’s philosophy. Marquard characterizes his attitude as a *“Sein zum Text.”* Man, and thus the historian, is connected to the world and history through historical consciousness (*Geschichtsbewusstsein*). The historian must interpret its manifestations time and again, which results in amorphousness and historical relativism.

There is a strong tendency to emphasize the flow of thought, an attitude that can be called “idealistic.” For Collingwood, history is the imaginative re-enactment of past thought in the context of our present knowledge; whilst for Croce philosophy and history are identical. Although Voegelin conceives history as linear, he also sees it as a movement through a web of meanings, which contains a plurality of nodal points. Another idealist approach is that of Ortega y Gasset, who postulated an a priori core in history.

Moving away from the frustrations of dogmatic Marxism, several thinkers drew radical ontological consequences about history by means of new speculations. Thus, the Frankfurt School understands history more as a process of humankind’s self-negation (negative teleology); Benjamin rejects the continuity of history, which he sees as a sequence of volatile images indicating an infinitude of experiences; Lyotard regards historical grand narratives as fictions *manquées* and prefers a pluralist narration (see chapter 40, this volume). This corresponds to the present condition in western liberal society, which is more open, tolerant of multiple cultures with inconsistent values and reflexive. Against this background, we can better understand Apel’s idea of an ideal communicative community, which inevitably contains some elements of universal history.

Once it has been thrown out of the window, the substantive philosophy of history – hidden by the window – returns as a “master narrative” or integrative story through the main door, whether in terms of the model historians use, or their own conscious or unconscious programs or paradigms. The new integrative stories provide the possibility of presenting diachronic phases, and of integrating fragments of various synchronic relations. In most cases, they are based on popular historiographic ideas and common ideas of history.

We constantly encounter different meanings in the different forms of historiography. The function of the philosophy of history is to identify man or an individual society with values and historic mission, which is generally linked to a perspective or program. This is, of course, preceded by a need, and is usually prompted by marginality, cataclysms or will to power. The attempt to write the history of Lorraine in 1850 or a play about the philosophy of history produced by the students of a Lycée

at Arcis-sur-Aube, did not, fortunately, have the same widespread results as did Russian messianism.

In the nineteenth century, knowledge of world history increased enormously in scope and depth. Even the eighteenth century French *Encyclopaedia* still held that only 7,000 years separated us from the beginning of the world, and that our knowledge of the world beyond Europe had for too long been marginal. In the new construction of world – global or universal – historiography had to start with basic principles of periodization and typology. These were provided by grand sociological theories such as those of Max Weber. Spengler and Toynbee offered other solutions, producing more or less independent monadic wholes. In 1949, Jaspers constructed a periodization of world history based on an axial age, around the fifth century BC, and this has been followed by several historians (Jaspers 1953). The driving forces of world history were based on demographic models, migration, diffusion, evolution, and materialist motives. Wallerstein developed a world system theory based on the theory of labor and trade.

Historiography is more productive than ever and is becoming ever more and more specialized and diverse. Simultaneously, the world is becoming globalized and world history is becoming increasingly relevant. Works by authors such as Sorokin, Rostow, Fukuyama, Huntington, and Thurow mix historical analogies with sociological and philosophical speculations (but then, what else can one do?) not about aims, but about orientations and tendencies. When postmodernism diagnoses the present and compares it with the past, it also implies a form of the philosophy of history (see chapter 50, this volume). Various reflections about the present condition of the world all have a consequence for interpreting history. It is curiously paradoxical that however despised substantive philosophy of history may be by historians and philosophers, its arguments still provide a matrix for contemporary historiography, as during the twentieth century. If they ever paid any attention to historiography, these authorities would probably consider this fact to be a mere symptom of decline.

The Poetics of the Substantive Philosophy of History

The need for meaning is almost an anthropological constant which presupposes some form of organization of the world (in our case: the past) into simplified synoptic stories and patterns. A story is a basic organizational form of communication. It is linked to an explanation, and, thanks to its end (its necessary condition), with a meaning. Rigid organization and a paratactical style are typical of simple stories. These correspond to a stereotypical form of behavior, and regular patterns in the conditions which provide a context for decision-making.

Stories can also occupy larger temporal wholes. If we find a hero, an agent, a subject and a collective identity, it is possible – formally and unproblematically – to depict a large expanse of time ranging from the past to the future as a single story. But this can also be problematic. Large-scale models like this are fuzzy by nature. It is hardly surprising that the philosophy of history as a general approach to history, as a kind of super-history, follows a plan which is similar to that of narrative historiography. Thus, Croce could call Vico's philosophy a cosmological novel; and for Hegel, history is an absolute epos, whose hero is humankind, the *Humanus*. These stories become more

complicated because deviations accrue. That is why Vico introduced *ricorsi* (digressions) which complemented the *corsi* (course) of his ideal history. He was not the only one: introducing “returns,” “anachronisms,” and flexible (i.e., empty, fuzzy, metaphorical) terms is typical of all grand schemes. However, the epic can also be eliminated retrospectively from the historiographic synthesis, as Lyotard’s grand narratives exemplify.

The situation was changed radically by the transformation of the concept of history into an independent (and, moreover, complex and dynamic) entity. Narration still remained dominant, but had to cope with an increasing number of circumstances and conditions, actions, and motives on the one hand, and with an increasing number of processed bits of information on the other. In this style, hypotaxis replaced parataxis. Literature had to cope with similar problems at around the same time, and so the novel with a complicated action became the most important genre in the nineteenth: it presents not only a story, but also a picture. Ranke, as his biographer von Laue claims, introduced the classical German literary style into historiography, together with its perception of the world as self-enclosed whole. It was this move, and not the historiographic scientific method, that helped him get beyond Niebuhr (von Laue 1950). Literature, history, historiography and the philosophy of history are all closely correlated – literature seems to a great extent to move within a space delimited by the same binary oppositions as the philosophy of history.

Droysen was already aware that structures are not narrated, that the form of a story is not sufficient. Both historiography and the philosophy of history derive from a story in attempting to form a picture – whether a pattern, synthesis, synopsis, configuration, or historical textbook (Vašíček 1994). A picture can be considered as a complex formed of the representations of objects and their relations, leading to closure, orderliness, and lucidity. Thus it has to have a perceptible extension; it has to be divided into parts, which are (even if only vicariously) related. It is directed at an object; and nothing can be added or omitted from its perspective. It differs from a story, especially by the implication that it does not accentuate chronology, that the order of the reception of its parts is not prescribed, and that it allows more readings, more partial linearities than a story. For this reason, a picture, in contrast to a story, does not have an unambiguous end or an obvious meaning. We can (and even must) have more than one picture of the same thing. A picture gives us more opportunities than a story. It enables reflexivity, ambiguity, different perspectives, local contexts, and reciprocal transformations of the object and the subject. These are processes that are also characteristic of modern art. Similarly, an intelligible synthesis integrating existing knowledge is considered to be the acme of the historian’s endeavor. The same also holds true in every philosophy of history. Such syntheses seem to follow the same rules as a picture and (thanks to these rules) contains similar generalizations and meanings. The attributes of a good picture as explained here are also shared by a theory. However, it does not mean that a picture, i.e., a synthesis, is an explanation, although this may play a part – in a similar way to a story.

General concepts allow us to move swiftly through time and space – and this is exactly what substantive philosophies of history like to do. One way to achieve general description is to assume a combination of elements, a “grammatics of all possible situations” (Hedinger 1969). Various typologies accompany this assumption. Rösen

presupposes a system of historical universals such as progress, process, transformation, structure (Rüsen 1986). The extent to which it is possible to compare analogous periods of history can also offer a criterion for a general framework: in other words, the extent to which we can construct a space of possible variants correlates to the extent to which we can construct a “possible” history (Koselleck 2004). This is not very far removed from postulating a historical substratum. Whether these are factual explanations, general descriptions, or just some form of shorthand, is open to question. The issue is complicated by the fact that for obvious reasons historians generally replace general terms with insufficiently reasoned generalizations and metaphors.

The relations between history as a science and/or as an art have a long history. The same holds for universal history and the philosophy of history. The smaller the number of generalities and structures, the more the philosophy of history is subject to style and organization, which – as we have seen – are both associated with meaning. Auerbach has shown on the basis of a detailed textual analysis how different styles entail different conceptions of experience (and vice versa). In these examples, the hidden paradigms or what may be called the subconscious of an epoch manifest themselves in the text. As early as 1889, George Saintsbury recognized in Gibbon’s style the wave motion of a current, giving a rolling impression. These issues are systematically addressed by Hayden White, who combines typology and structuralism in a style resembling a Rubik’s cube (White 1973). Ontology, epistemology, rhetoric, and form all unite in his approach. We can look at the philosophies of history from the same perspective, although literary form plays a less important role in their case, since recent substantive philosophies of history rarely embody an integrated narrative form.

The Arrival of the Philosophy of Historiography

The concept of history as an autonomous whole originated as a reflection of and on a transformed reality. However, beginning with the 1950s, history does not seem to form a self-explaining whole, and ceases to guarantee meaning. Thinkers pose the relations between subject and object anew, and the old concept of history is emptied, the pattern of expectations is narrowed. The subject itself, *res gestae*, becomes more and more self-conscious (reflexive turn). Historiography has to cope with this situation, and it has to introduce new methods, which differ significantly from the previous ones. Concurrently, we reach an anagram: the problems are transferred from history to historiography. The same move can be seen in the social sciences, in literature and the arts. In short, from an orderly homeric *Odyssey* we reach a stream of consciousness, and the one great digression of Joyce’s *Ulysses*.

The conception of history as a substantive whole brought the philosophy of history to think through the problems of objectivity, laws, contingency, and necessity. The past was presented on this basis. Yet if we abandon the assumption of a history such as this, we must necessarily focus on the ideas of historiography about the wholes of the past – these are no longer stories, but pictures, and can be read variously. This means that existing problems are reformulated, new questions posed. Contemporary

historiography and the philosophy of history thus move in a more complex field of force. The parameters include, for example, the un contemporaneity of the contemporaneous, synchrony/diachrony, structure/events, holism/individualism, global/universal, explanation/understanding, foreground/background, alternation of measures, the relation of grand and small histories, isochronology (various events in one time)/isophenomenology (similar events in different times). No less distinctive is the aporia of Schleiermacher's hermeneutic circle – we cannot understand the whole other than from its parts and, unfortunately, *vice versa – regres ad infinitum*. The extent to which these are problems not only of historiography, but also of history, remains a matter for debate.

The concepts of history, historiography, the philosophy of history, and historiography, as well as philosophy, social science, and literature are always of their time. That means they are always historical. To find their conjoint constants is therefore difficult. Perhaps it is possible to replace them with these various aporias.

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Philosophical Issues in Natural History and Its Historiography

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Introduction

This chapter explores the nature, scope, and justification of the scientific historiography of nature (a.k.a. historical natural science). Historiographic research is remarkably common in natural science, occurring in fields as diverse as paleontology, geology, biology, planetary science, astronomy, and astrophysics. Hypotheses in the scientific historiography of nature postulate physical causes in the remote past to explain puzzling, present-day, natural phenomena. Some salient examples are: the hypothesis that the continents were once joined together into a super continent (Pangaea), which explains surprising patterns of “frozen” magnetism found in ancient igneous rocks; the Alvarez meteorite-impact hypothesis (for the extinction of the dinosaurs), which explains the high concentrations of iridium and shocked quartz found in the mysterious K–T (Cretaceous-Tertiary) boundary marking the end of the fossil record of the dinosaurs; and the “Big-Bang” theory of the origin of the universe, which explains the puzzling isotropic, three-degree, background radiation first detected by satellites in the mid-1960s. As these examples illustrate, the length of time separating a postulated cause from its evidential effects (traces) can be very long, typically in the tens of millions to billions of years.

Historiographic science differs in important ways from experimental science. The hypotheses of experimental science typically postulate regularities among kinds or types of events, e.g., the behavior of any piece of copper whatsoever when heated. In contrast, the hypotheses of scientific historiography typically postulate particular events, e.g., a specific meteorite impact that occurred 65 million years ago. Furthermore, the manner in which hypotheses are “tested” in scientific historiography differs from that of experimental research. The role classically attributed to experiment is that of testing hypotheses in controlled laboratory settings. Historiographic hypotheses usually cannot be tested in this manner. Not only do most historiographic hypotheses postulate singular or unique, and hence unrepeatable, occurrences, but even more importantly, these events are typically highly complex, involving the intersection of many independent, spatially and temporally extended, causal chains. It is extremely difficult, and in many cases impossible, to artificially produce analogous events in a laboratory setting, let alone run the experiment long enough to confirm that the pertinent

evidential traces will be available at some distant future time. To put the point vividly, one cannot bring about a meteorite impact in a laboratory of the scale and complexity postulated by the Alvarez hypothesis, in an environment similar to that which obtained on earth, and subsequently determine that iridium will remain concentrated in a K–T-like boundary layer for 65 million years.

Because experimental science is commonly held up as the paradigm of “good” science, historiographic research is sometimes criticized on the grounds that it does not “test” hypotheses in a manner equivalent to experimental science. Recent debates in the United States (e.g., in Kansas and Pennsylvania) over whether Darwinian evolution should be taught in public schools as a science, or alternatively whether some form of “creationism” (young earth creationism or intelligent design theory) should be taught alongside it as a legitimate scientific competitor, provide good examples. As a web document supplied in 2002 by the Intelligent Design Network (www.intelligentdesignnetwork.org), an organization devoted to fighting the teaching of evolution in public schools, counsels: “Explanations regarding origins are controversial because they consist of subjective ‘historical narratives’ that can not be confirmed by experiment as is the case with experimental sciences like physics and chemistry” (p. 2). A startling number of experimental scientists seem to concur. In 2001 a group of approximately one hundred, mostly experimental, scientists – some of whom are quite eminent – signed a manifesto, “A Scientific Dissent from Darwinism,” which appeared in the *New York Review of Books* (Dec. 1); their fields were listed after their names. The most sweeping condemnation of scientific historiography, however, comes from Henry Gee, an editor of the prestigious, mainstream, science journal *Nature*. Taking aim at *all* historiographic research, Gee writes, “they [historiographic hypotheses] can never be tested by experiment, and so they are unscientific . . . No science can ever be historical” (2000: 5–8). For Gee and fellow travelers a genuine test of a hypothesis requires experimentation. Historiographic hypotheses cannot be tested in this manner. Hence they are unscientific.

The belief that “good” scientific practice requires experimentation goes back to the work of Galileo, Newton, and especially Robert Boyle (Shapin and Schaffer 1985), who articulated the now classic account of experimentation in terms of carefully performed, controlled experiments specifically designed to test a specific hypothesis. By the end of the nineteenth century, experimental research was widely accepted as the paradigm for all science. To the extent that it supplies evidence in the form of shared, replicable observations, experiment seems to provide a truly objective (and hence uncontroversial) court of appeal for evaluating hypotheses (and theories). Starting as early as the work of Francis Bacon, accounts of “the scientific method” were developed to explain how unbiased observations support or undermine hypotheses. These accounts portrayed the scientific method as supremely rational, firmly grounded in logical and mathematical reasoning. Science became celebrated as a quintessentially rational and objective experimental enterprise.

Many scientists and most laypersons are still enthralled by this conception of science as a fundamentally experimental enterprise, which helps to explain some of the trenchant criticisms mounted against scientific historiography. However, this view is no longer popular among philosophers of science. The late nineteenth and early twentieth centuries were marked by rapid advances in experimental science, particularly

chemistry and physics. A group of philosophers, mathematicians, and scientists, known as “logical positivists,” met in Vienna, Austria, in the 1920s for the purpose of developing a model of scientific methodology couched solely in observation and logic that could explain these remarkable successes (Ray 2001; Salmon 2001). As time went on, they and their successors, the early “logical empiricists,” encountered increasingly serious difficulties. Logically sophisticated versions of inductivism (theories of confirmation) were plagued by paradoxes of relevance and induction (Trout 2001). Karl Popper’s highly influential alternative to inductivism, falsificationism, was undermined by the discovery that theoretical and background assumptions play integral roles in the rejection of hypotheses (the Quine–Duhem thesis), as well as in their acceptance (Gooding 2001). Besides, as Thomas Kuhn (1962: 1–9) famously pointed out, neither the history of science nor its current practice conform to the analyses of scientific methodology provided by the logical positivists. The “logic” of the methodology of science was not the only victim of post-positivist critiques of science. Hanson (1958) and Kuhn (1962) attacked the idea that observation, however carefully conducted, is unbiased, establishing that all evidence is irrevocably contaminated by theory; in order to count as evidence, observations must be interpreted and interpretation inevitably utilizes theoretical concepts and assumptions. By the late twentieth century, Hacking (1983) and Franklin (1999) had shown that experiment plays a variety of roles in experimental science besides the testing of hypotheses and theories, thereby undermining the view that there is a single comprehensive method for experimental science, let alone all of science. In short, experimental science had failed to live up to its early promise as a model for all of science.

The effects of these findings on philosophy of science were profound. Philosophers grew reluctant to generalize, let alone make value judgments, across different scientific disciplines. Consequently, recent philosophical attempts to understand the practices of scientific historians have not (like those of scientists and certain members of the general public) been shackled to an overly romantic conception of experimental science. Contemporary philosophers of science have nonetheless nursed other concerns about the rationality and objectivity of scientific historiography of nature. This chapter explains what these concerns are and why they are unfounded. It begins, however, by explaining how the findings of post-positivistic philosophy of science undermine common criticisms of scientific historiography, criticisms such as those mounted by Gee and members of the Intelligent Design Network.

The Scientific Method of Yore

Philosophically unsophisticated criticisms of scientific historiography are usually couched negatively, in terms of its inability to “test” hypotheses in the manner of experimental science. Such objections reflect the widely held view that there is a single method for the practice of all good science and that classical experimental science best exemplifies it. To the extent that it does not conform to the practices of classical experimental science (and as we have seen, it does not) historiographic research is often judged inferior or even worse, unscientific.

Some distinctions will help to facilitate our discussion. The hypotheses “tested” in classical experimental science are typically generalizations, potential candidates for (basic or subordinate) natural laws. From a hypothesis one may infer test implications. Test implications are conditional statements describing what must happen if the hypothesis is true. To make matters more concrete, consider the “toy” hypothesis “all copper expands when heated.” The following test implication may be inferred from it: if a condition (heating a piece of copper) is brought about, an event (the expansion of copper) will occur. Test implications like this provide the basis for experimental tests. The condition is artificially produced in a laboratory and investigators subsequently look for an instance of the event. Because they are concerned with kinds or types of things (e.g., any piece of copper) vs. individual things (the piece of native copper in my rock collection), the hypotheses of classical experimental science may be repeatedly “tested” in a laboratory setting.

How are hypotheses accepted and rejected on the basis of observations obtained from experiments? Under the rubric “the scientific method,” many science textbooks, beginning in grade school and continuing through college, present one or a combination of two accounts, (scientific) inductivism or falsificationism. According to inductivism, the occurrence of a predicted event under specified conditions provides confirming evidence for a particular hypothesis, and if enough confirming evidence of the right sort is obtained, the scientific community should accept that hypothesis. Unfortunately such inductivism faces David Hume’s (*Treatise of Human Nature* 1739: book I, part III, sec. VI) intractable problem of induction: No finite body of evidence, however varied and extensive, can conclusively establish the truth of a universal generalization applying to unobserved as well as observed cases (Newton-Smith 2001). The fate of the hypothesis “all swans are white” provides the well-worn example. Although it was supported by an enormous number of observations from all over Europe and Asia, it was nonetheless rejected when black swans were discovered in Australia. The moral is that no scientific generalization, however well supported by observation, can be viewed as conclusively proven because one can never eliminate the possibility of discovering an exception. Attempts to circumvent the problem of induction by appealing to objective probabilities face similar problems. One can never eliminate the possibility that relative frequencies acquired through observation will cease to hold with subsequent observations, undermining whatever numerical value was assigned to the probability of the hypothesis on the basis of earlier observations.

Convinced that the problem of induction is insurmountable, Karl Popper (1959) defended falsificationism, which many (if not most) natural scientists continue to embrace, although rarely in the extreme form that Popper had in mind. Unlike inductivism, falsificationism receives support from formal logic, more specifically, a logically true inference rule, *modus tollens*. *Modus tollens* asserts that a universal generalization is false if it has at least one counter-instance. Thus “all swans are white” is refuted (falsified) by the discovery of a single black swan. The appeal of falsificationism should be obvious: although one can never confirm a scientific hypothesis, even in the modest probabilistic sense of showing that it is more likely true than not, it seems that one can conclusively falsify a hypothesis. Popper went further, however, insisting that good science is characterized exclusively by attempts to refute hypotheses via “risky tests,”

tests that are highly likely to produce failed (vs. successful) predictions. Moreover, on his account, the probability that a hypothesis is true cannot be viewed as increasing with the number of risky tests that it survives because (and he is right about this) that would involve an illicit appeal to induction. Few scientists are willing to embrace such a pessimistic recipe for the practice of science. But the less strict falsificationist position, emphasizing the power of refutation while tacitly embracing the inductivist tenet that hypotheses that survive stringent tests are more likely to be true, remains very popular.

As philosophers have known since the 1950s, the claim that hypotheses can be conclusively falsified by observation is false. There are two central problems. First, the evaluation of a hypothesis in light of experimental results implicitly presupposes an enormous number of ancillary assumptions about equipment and background conditions as well as the truth of other widely accepted theories. When these diverse supplemental assumptions are explicitly included, the logical inference licensed by *modus tollens* is radically altered. The falsity of an ancillary assumption and not the target hypothesis could be responsible for a failed prediction. Every student of science is implicitly aware of this because repetitions of classic experiments in laboratory exercises often go wrong, not because the hypothesis being tested is false but because a piece of equipment is malfunctioning or the sample is contaminated, to mention two among many possibilities. Varying the conditions under which a hypothesis is tested cannot solve this problem because the number of ancillary assumptions tacitly involved in any concrete “test” of a hypothesis is unknown and potentially infinite; it is impossible to “control” for them all. Thus the famous Popperian directive to bite the bullet and reject a hypothesis in the face of a failed prediction has little logical force. Furthermore, as Kuhn (1962) pointed out, scientists almost never practice falsificationism. Faced with a failed prediction, they mount a sustained search for intervening variables, external conditions that might be responsible. This amounts to exercising the logically permissible option of salvaging a hypothesis by rejecting an ancillary assumption. The classic example is the response of nineteenth-century astronomers to the perturbations in the orbit of Uranus, which violated the orbit predicted by Newtonian celestial mechanics. Rather than behaving like good falsificationists and rejecting Newton’s theory, astronomers rejected the assumption that there are no planets beyond Uranus, and (most importantly) discovered a new planet, Neptune. In other words, rejecting a hypothesis in the face of a failed prediction is sometimes the wrong thing to do; it is not an accident that logic gives us the option of rejecting an ancillary assumption instead! In sum, logic does not dictate that scientists behave like good falsificationists, nor do they behave like falsificationists in practice. Falsificationism, in either its weak or its strong form, does not provide a satisfactory account of scientific practice.

What lessons can be drawn from this discussion? First, the celebrated scientific method of yore (whether construed as inductivist and/or falsificationist) is logically flawed, historically inaccurate, and does not provide an adequate characterization of the practices of contemporary experimental scientists. As a consequence it cannot be used to support the claim that historiographic research is inferior to classical experimental research. Yet this seems to be exactly what is going on when the former is condemned on the grounds that it cannot “test” (“confirm” or “falsify”) its hypotheses “by experiment.” If this is not the case, then critics of scientific historiography are using these

terms in informal idiosyncratic ways, and this alone is enough to undercut the plausibility of their arguments. To be compelling, their arguments must be grounded in widely accepted, precisely described concepts of “testing.” Unfortunately, post-positivist research in philosophy of science, which has explored various ways of making these concepts precise (e.g., Kuhn, Lakatos, Laudan), has found that, even in experimental science, “confirmation” and “disconfirmation” (a.k.a. falsification) is a highly complex affair involving much more than simple logical relations of induction and/or deduction between hypothesis and observation. In addition, the dogmatic view that the success of science must be attributable to some as yet unspecified, universal scientific method, for which experimental science provides the prototype, is undermined by studies showing that even experimental scientists employ a variety of different methods in their research practices (Gooding 2001; Hacking 1983; Franklin 1999). In short, popular claims about the inferiority of scientific historiography vis-à-vis experimental science rest upon misconceptions about the nature of scientific methodology in general and the practices of experimental science in particular.

The Structure and Research Practices of Scientific Historiography of Nature

Philosophers of science have paid little attention to the simmering controversy among scientists and members of the public over the scientific status of historiographic research into natural history. Unable to provide an account of the rationality and objectivity of science that is firmly rooted in logic and theory-neutral observation, and skeptical about the existence of a single method for all of science, they have been reluctant to generalize, let alone make normative (value) judgments across scientific disciplines as different as, for instance, paleontology and experimental physics. Consequently, contemporary philosophers of science tend to restrict their attention to issues specific to particular disciplines. Most philosophers of biology, for instance, focus almost exclusively on understanding the structure and justification of neo-Darwinian evolution, which lies at the center of most contemporary biological thought, and display little interest in comparing it to the structure or justification of theories in other scientific fields. But as discussed below, most historiographic research shares certain core characteristics, and some of them have raised legitimate concerns among philosophers of science about the epistemic status of scientific historiography of nature independently of its differences from experimental science.

Historiographic hypotheses and theories are primarily concerned with unique, individual events. As an example, the Alvarez hypothesis for the K–T extinctions conjectures that a particular meteor, 10–15 km across, slammed into earth 65 million years ago (mya), triggering the mass extinction that famously took out the dinosaurs, along with at least 75 percent of all species then existing on earth. In contrast, the focus of experimental science is on regularities among generic (types of) events, e.g., the hypothesis that copper is a good conductor of electricity is concerned with the behavior of any piece of copper, when heated, as opposed to the behavior of a particular piece of copper. Given this difference in subject matter, it is hardly surprising that the practice of scientific historiography exhibits significant differences from that

of classical experimental science. Hypothetical regularities can be (at least in principle) tested through repeated experiments but the occurrence of a unique event happens once and only once. Moreover, the events postulated in scientific historiography took place in the remote past; they are not accessible to “direct” observation, however generously it might be defined. Establishing a convincing evidentiary relationship between a unique, long past event and its contemporary observable traces is challenging because of the complexity, contingency, and temporal length of the causal chain involved.

The debate over the Alvarez hypothesis provides a good illustration (Powell 1998). For the sake of simplicity, I focus on the first part of the hypothesis (the meteorite impact), ignoring the issue of whether the conjectured impact is responsible for the mass extinction. The evidence that convinced most scientists was the presence of surprisingly large concentrations of the element iridium coupled with deposits of a rare form of “shocked” (fractured) quartz grains in K–T boundary sediments collected from around the world. The discovery that K–T boundary sediments were enriched in iridium – up to 30 times that found in sediments on either side of the boundary – focused the attention of researchers on two possibilities, volcanism or meteorite (asteroid or comet) impact. The earth’s crust is “low” in iridium because, like iron, iridium is a heavy element and most of it sank into the mantle and core when earth was forming. Asteroids and comets left over from the formation of the solar system typically have higher concentrations of iridium than the earth’s crust. Thus the impact of a huge meteorite provides a plausible explanation for the global iridium anomaly. On the other hand, volcanic processes bring mantle material to the surface, and there is evidence of extensive volcanism in the Deccan traps region of India approximately 65 million years ago. So volcanism provides a *prima facie* plausible alternative explanation for the excess iridium.

Further investigation provided additional evidence, which supported the meteorite impact hypothesis over the volcanism hypothesis. Mineral grains, particularly of quartz, with an unusual pattern (crosshatched, parallel sets) of fractures were discovered in K–T boundary sediments. It takes enormous pressures to fracture minerals in this way. Indeed, there are only two places on earth where they are found, the sites of nuclear explosions and meteor craters. The combination of excess iridium and shocked quartz in K–T boundary sediments was thus enough to convince most members of the scientific community that a huge meteorite hit earth 65 mya. In this context it is worth noting that although the discovery of the Chicxulub crater, which straddles the northern coast of Mexico’s Yucatan Peninsula, is often cited as pivotal, it was not. Seventy percent of earth’s surface is covered by ocean, making an ocean impact more probable than a land impact. Had impact occurred in an ocean, the crater almost certainly would have been obliterated by the active geology of the seafloor, which moves in conveyor like fashion away from mid-ocean ridges, where it forms, to the margins of continents, where it sinks back into the mantle at subduction zones. Indeed, many geologists were pleasantly surprised when a crater of the right size and age was identified straddling a landmass.

The evidence that was decisive for the acceptance of the meteorite-impact hypothesis is striking because it did not result from a successful prediction. Consider the iridium anomaly. Not all meteorites are rich in iridium. Moreover, geological processes

consolidate as well as disperse material. For example, high-density minerals such as gold can be concentrated by flowing water into placer deposits; indeed, it is estimated that more than half of all the gold that has ever been recovered by humans has come from placer deposits! To make matters worse, outcrops of K–T boundary sediments are very rare, many are still deeply buried and of those that have been exposed, most have long since been removed by weathering and erosion. This raises the question of whether the currently exposed K–T boundary sediments are truly representative. It is just not possible to deduce or predict the presence of “tell-tale” iridium in the contemporary environment from the conjecture that a huge meteorite hit earth 65 mya. Indeed, the discovery of excess iridium in the K–T boundary was serendipitous. Walter Alvarez, a geologist, was interested in finding a method to date the time span represented by the boundary and his father Luis, a Nobel Laureate in physics, suggested that iridium would make a good “clock” since it is supplied by meteoritic dust at a known constant rate. They were shocked to find so much iridium in the boundary, and the only plausible explanations that they could think of were meteorite impact, which they personally favored, and volcanism. This reveals an important characteristic of historiographic research. Most historiographic hypotheses are supported or “confirmed” by evidence in virtue of the power of the hypothesis to *explain* (vs. predict) the evidence.

Explanation and Confirmation in Scientific Historiography

The idea that explanation and confirmation are closely related is old. It lies at the core of the traditional covering-law or deductive-nomological (D–N) model of scientific explanation, one of the pillars of twentieth century positivistic philosophy of science. Formulated by Carl Hempel (1965), the D–N model assumes that deduction and explanation are symmetrical. An explanation is an argument, whose premises cite initial circumstances and natural laws and whose conclusion cites the phenomenon (event or fact) to be explained; more precisely, an explanation has the following structure:

1. C_1, C_2, \dots, C_n
 2. L_1, L_2, \dots, L_n
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- E

where C_1, C_2, \dots, C_n are statements describing particular circumstances, L_1, L_2, \dots, L_n are general statements of law, and E is a statement describing the phenomenon to be explained. To constitute a full explanation, an argument of this form must be deductively valid and the premises (statements describing the initial conditions and laws) must all be true. If the premises are known and the conclusion unknown the argument functions as a prediction whereas if the conclusion is known and (one or more of) the premises unknown the argument functions as an explanation. More specifically, I can *predict* that a particular piece of copper will expand from the fact that it is heated and the law “all copper expands when heated,” and I can *explain* why a particular piece of copper expanded by referring to either the aforementioned circumstance or law. It is in this sense that the D–N model treats prediction and explanation

as symmetrical. Insofar as confirmation is traditionally associated with successful prediction, the idea that compelling explanations provide confirmatory evidence for hypotheses does not seem very problematic.

Unfortunately historiographic explanation in natural science does not fit the D–N model very well. D–N explanations require universal generalizations in order to validate the inferences; it is not an accident that it is also called the “covering law” model. Without such laws explanations are incomplete. Experimental science is rife with general laws. It thus comes as no surprise that explanations in the experimental sciences commonly cite them. In contrast, explanations in scientific historiography rarely invoke general laws. The Alvarez team did not cite any general laws in their explanation for the excess iridium found in the K–T boundary. How could they, given that not all meteorites are rich in iridium, not to mention the fact that it is well known that geological processes sometimes concentrate material that was originally widely dispersed?! Instead, they cited a particular event, a 65-million-year-old meteorite impact. Contemporary earth scientists almost universally concur with them that this is the best explanation for the iridium and shocked quartz, and, moreover, view the presence of this material in the K–T boundary as powerful confirmatory evidence for the hypothesis that such an event occurred. In other words, explanation and prediction come apart in the Alvarez hypothesis. This runs counter to the long tradition in philosophy of science, captured in the D–N model of explanation, of associating confirmation with successful prediction.

Hempel was fully aware of the difficulty of fitting historiographic explanations into the D–N model. His response was to demote them to mere “explanatory sketches,” thus reinforcing the common view that scientific historiography is inferior. Hempel attributed the compelling nature of some historiographic explanations to a tacit assumption of unspecified natural laws. He also broadened the concept of natural law to include statistical regularities such as those found in quantum physics. Thus, on the D–N model, compelling historiographic explanations could be analyzed as involving reference to assumed but unstated statistical or universal laws.

Experience suggests, however, that if there are regularities relating particular evidential traces to their long past causes they are extremely messy and rough, riddled with exceptions and contingencies, and thus not at all like the stereotypical laws of physics, e.g., Newton’s second law of motion, or for that matter, the Schrödinger equation for the evolution of the quantum mechanical wave function. Calling such patterns “laws” is more a courtesy than an accurate description. Moreover, even supposing that there are bona fide laws of some sort buried deep beneath the appearance of exception and contingency, it is unlikely that human beings could use them to infer evidential traces (e.g., excess iridium) from their conjectured long past causes (meteorite impact). The appearance of irregularity would merely be transferred from laws to particular circumstances, and every link in the long causal chain stretching between postulated cause and present-day trace would be potentially affected. In short, the D–N model can neither account for the compelling character of many historiographic explanations nor for the central role played by explanation in the confirmation of historiographic hypotheses.

In some ways it is surprising that any contemporary philosopher of science would worry about the failure of historiographic explanations to fit the D–N model. It is well

known that the D–N model is riddled with conceptual problems and counter-examples that have nothing to do with historiography (Newton-Smith 2001). For example, while one can explain the length of a shadow cast by a flagpole in terms of the height of the flagpole, one cannot explain the height of the flagpole in terms of the length of its shadow even though one can predict it (given the position of the sun and the laws of optics). Thus we have a clear-cut non-historiographic case of prediction without explanation, which is impossible according to the D–N model.

Difficulties such as these have led many philosophers to pursue alternative accounts of scientific explanation. Causal theories are some of the most promising. Many of the problems with the D–N model seem to reflect an omission of reference to causation. It is in virtue of the fact that the flagpole causes (a.k.a. produces) the shadow that its height explains the length of the shadow. The length of the shadow fails to explain the height of the flagpole in virtue of the fact that the shadow does not cause the flagpole. The preference for general statements of law over reference to causation in the D–N model undoubtedly reflects David Hume's famous analysis of causation as *de facto* regularity, which appears in both of his great philosophical works, the *Treatise of Human Nature* (1739) and the *Enquiry Concerning Human Understanding* (1748). On Hume's account, there is no fundamental difference between causal relations and general laws. But as is well known, Hume's account of causation faces serious problems, some of which are the same as those facing the D–N model of explanation. It is generally conceded, even among neo-Humeans, that the relation between law-likeness (*qua mere regularity*) and causation is not as simple as Hume believed.

Causal theories of explanation do not identify explanations with arguments, nor do they place the explanatory burden on laws of nature. Instead, the focus is on causal relations, chains, or processes involving particular events. The point is to draw attention to what is at least in part causally responsible for the phenomenon to be explained. Causal theories of explanation are well suited for making sense of historiographic explanation in natural science because the latter typically involves explaining specific contemporary phenomena (traces) in terms of particular long past causes. It should thus come as no surprise that they dominate contemporary philosophical thought about explanation and confirmation in scientific historiography of nature.

There are two main causal theories of historiographic explanation, one emphasizes constructing narratives and the other emphasizes strategies for identifying common causes from their effects. Despite the fact that some philosophers combine them, there are important differences between them.

Narrative Explanation

Narrative accounts construe explanations as stories, and hence may have a significant fictional component, involving both omissions and additions, even when they are based on fact. The justification for a narrative approach to scientific explanation of events in natural history is grounded in the notion that contemporary phenomena have a potentially infinite number of causally relevant, highly contingent, antecedent events. It is impossible to know them all. The job of the scientific historian is to tell a coherent causal story about how a puzzling contemporary phenomenon, a trace, was produced. To be

compelling the story needs to be convincingly continuous despite the fact that much is, and always will remain, unknown. The basic idea is to reconstruct a causal chain of antecedent events in light of the present-day phenomena in need of explanation, utilizing whatever additional empirical evidence happens to be available and our current scientific understanding of the putative phenomena involved.

Narrative accounts were popular in philosophic accounts of explanation in human historiography, where unobservable human intensions may play key explanatory roles. As an example, a satisfactory explanation of why Julius Caesar crossed the Rubicon may require reference to Caesar's motivations. Since explanations in evolutionary biology seemingly involve appeals to natural ends (e.g., explaining the ability of bats to echo-locate as an adaptation to a nocturnal, aerial lifestyle), some philosophers of science, e.g., Hull, Kitcher, Ruse, have advocated narrative accounts of biological explanation. Narrative accounts of explanation are not nearly as compelling, however, in non-biological scientific historiographies of nature, which, significantly, do not involve even *prima facie* appeals to purposes or ends. For instance, the iridium in the K–T boundary is not even metaphorically a goal or purpose of the meteorite impact that produced it; it is merely a serendipitous (from our point of view) by-product of the impact. In other words, a narrative approach to explanation is not as universal in scientific historiography as sometimes suggested.

The central problem with narrative accounts of scientific historiographic explanation is the stress placed upon formulating a coherent story over empirically validating it: since many of the events in the narrative sequence are invented to provide coherence and continuity, justification is relegated to a minor role. This conflicts with the traditional emphasis in natural science on evidential warrant. The problem is exacerbated by the crucial role played by explanation in the confirmation of historiographic hypotheses. If the main reason for accepting a historiographic hypothesis is its explanatory power and it draws its explanatory power primarily from the coherence and continuity of a quasi-fictional story, then scientific historiography really does seem inferior to experimental science.

In contrast to narrative accounts of explanation, which, like the D–N model, focus almost exclusively on form or structure, common cause explanation emphasizes evidential warrant. The basic idea is to formulate reliable inferential methods for identifying when a diversity of contemporary traces are the effects of a long past, common cause token, as opposed to separate cause tokens.

Although some philosophers of science (e.g., Hull) view the search for common cause explanations as subordinate to the formation of narrative explanations, an investigation of the practices of the non-biological historiographic sciences suggests that the former frequently has a life of its own. Many historiographic explanations in the natural sciences involve inferences to common causes but do not take on a narrative form. Impact geologists are not interested in reconstructing a quasi-continuous causal chain between the pertinent contemporary traces found in the K–T boundary and their postulated, 65-million-year-old cause. What matters to them is that these traces are currently present in K–T boundary sediments from around the world, regardless of how they managed to survive mundane geological processes of degradation and dispersion during the very long timespan involved. In other words, there seems to be little interest in filling in the yawning gaps between the postulated ancient impact event and the

traces currently found in the boundary with additional events, fictional or otherwise. Yet such temporal gaps are not viewed as threatening the integrity of the explanation.

Another good example is paleontologist Mary Schweitzer's (2005) explanation for what appears to be medullary bone inside the leg bones of a *Tyrannosaurus rex*. Medullary bone is a calcium-rich layer that develops in the long bones of contemporary female birds during the egg laying process, providing a readily accessible supply of calcium for building eggshells. Schweitzer discovered an analogous layer in a fossilized leg bone from a *T. rex* and concluded that it was female. Significantly, she did not attempt to reconstruct any of the antecedent events in the long causal chain stretching between the death of this unfortunate *T. rex* and the preservation of its leg bone for millions of years in the Montana desert. Indeed, a detailed story of this sort seems irrelevant to her purpose, which is to support her conjecture that this particular ancient bone is from a female *T. rex*. To this end, she appeals to facts about modern birds and similarities between the structure of their bones and the fossilized remains of this particular *T. rex*, drawing extensively upon well accepted background beliefs about the close phylogenetic relationship between modern birds and the "bird-hipped" dinosaurs. The point is the explanation she gives for the medullary-like bone does not resemble a story: it is simply too minimal to meet the threshold required for a genuine narrative.

In short, like their experimental colleagues, the central focus of historians of nature is on securing empirically well-founded connections between evidence and hypothesis. Except perhaps in evolutionary biology, this rarely involves reconstructing a quasi-fictional, intuitively continuous causal chain between the phenomena concerned. In truth, most natural scientists are loath to engage in the kind of empirically ungrounded speculation. Instead they emphasize more direct inferential strategies for inferring long past common causes from present-day effects. The similarities in the approaches of historians studying human history and natural history are thus fewer than many scholars, e.g., Stephen J. Gould (1989), have supposed. To the extent that one views the teleological, goal directed, character of evolutionary explanations as a crutch, a result of the overwhelming historical contingencies of evolution, narrative explanations are perhaps unavoidable but certainly not ideal in biology either. The crucial issue is how to validate inferences to long past, common cause tokens from particular collections of contemporary traces without reconstructing the sequence of intervening events between them.

Common Cause Explanation

Common cause accounts of explanation are founded upon Hans Reichenbach's (1956) "principle of the common cause." Roughly speaking, the principle of the common cause asserts that seemingly improbable coincidences, correlations, or similarities among events or states, are best explained by reference to a shared common cause. As an example, if two students in a class turn in identical term papers, and it is clear that they didn't copy from one another, the most plausible explanation is not that they wrote them independently (too improbable) but that they got them from a common source, e.g., the same friend or the same internet-based term paper service. Reichenbach introduced

the principle of the common cause in the context of a theory of probabilistic causality, articulating it in terms of probabilities, i.e., when $P(A \& B) > P(A) \times P(B)$, where A and B represent jointly occurring phenomena (events and states), then it is likely that A and B share a common cause. The principle of the common cause is not, however, a logical consequence of the mathematical theory of probability. It represents a metaphysical conjecture about the way phenomena hang together in time in our universe, namely, seemingly improbable coincidences among particular phenomena are likely to be the result of a common cause; genuine coincidences are rare in nature. Attributing similarities and correlations among phenomena to a shared common cause has great explanatory power, for it makes their joint occurrence credible. Attributing their concurrency to chance, on the other hand, explains nothing; we are left with an intractable mystery. But, and this is what makes it a metaphysical thesis, the universe might have been such that coincidences are the rule rather than the exception.

The principle of the common cause provides a potentially powerful tool for understanding explanation in scientific historiography of nature. A good example is the remarkable scientific consensus that has been achieved on the hypothesis that contemporary life on earth descends from a last universal common ancestor (a.k.a. LUCA). All life on earth today (from the lowliest bacterium to the most sophisticated mammal) is built from proteins and nucleic acids. Proteins supply the bulk of the structural material for building bodies as well as the enzymatic material for powering them. Despite the fact that over 100 amino acids are available in nature and biochemists have shown that perfectly functional proteins can be constructed from alternative suites of amino acids, life on earth utilizes the same 20 amino acids to build all of its proteins. The hereditary material (nucleic acids) of all life on earth today displays equally striking, contingent molecular similarities. The best explanation for these remarkable similarities in molecular composition is not that they represent a fantastic coincidence but that all life on earth today inherited them from a last universal common ancestor.

The more improbable the correlations or similarities among a collection of evidential traces the more convincing the claim that they were produced by a common cause. Indeed, if the ostensible coincidence is improbable enough, one doesn't need to even partially reconstruct the long causal chain of events lying between evidence and hypothesized common cause in order to make a convincing case for the hypothesis. This helps to explain why scientists who study natural history rarely produce narratives of the sort allegedly popular among historians of human history. Not only are they, like most natural scientists, reluctant to engage in empirically unwarranted speculation, they frequently don't need to do so. If they encounter traces in the field whose joint occurrence is highly improbable they ipso facto have good reason to suspect a common cause. The iridium and shocked quartz in the K-T boundary provide a particularly salient example. The only event that renders their global concurrence in a thin layer of sediment of the same age plausible is a massive meteorite impact. As a consequence, geologists have not attempted to reconstruct even a partial chain of events linking the hypothetical impact event to the iridium and shocked quartz found today in K-T boundary sediments. Similarly, the best explanation for the surprising similarity in medullary bone between Schweitzer's *T. rex* and modern birds is that the former is female. Viewed from this perspective, it is hardly surprising that scientific historians of nature tend to focus their research on the most puzzling, improbable, correlations

and similarities among contemporary phenomena; they provide the best empirical window on the past.

As Elliot Sober (1988, 2001) observes, however, common cause is not the only possible explanation for puzzling correlations and similarities among contemporary phenomena. Separate causal processes operating independently of one another can also produce them. Sometimes these processes are of the same type, representing instances of the same law-like pattern, and sometimes they are of different types but just happen to produce the same kinds of effects. In either case, the results are the same, a spurious correlation or similarity due neither to chance nor a common cause token. Evolutionary biology is a good source of examples. Bats, birds, and insects resemble each other in having wings but do not share a common ancestor with wings; they evolved wings separately, presumably in response to similar environmental selection pressures. In contrast, lions, whales, elephants, and human females, which have mammary glands, do share a common ancestor. The question is how can one discriminate between biological similarities of the former kind (known as “analogies”), which are not the result of a common cause (or chance), and biological similarities of the latter kind (“homologies”), which are the result of a common cause?

Sober (1988, 2001) and Tucker (2004) utilize Bayesian comparisons of likelihoods of the evidence to distinguish inference to a common cause from inference to separate causes. The best hypothesis is the one that makes the evidence most likely, given shared background beliefs. Their accounts differ in important ways, however. Sober starts with precisely specified common cause and separate causes hypotheses, whereas Tucker advocates settling the issue of whether the evidence was produced by a common cause vs. separate causes before speculating about the properties of the cause(s).

Tucker contends that his strategy is more in keeping with the actual historical development of scientific historiography, especially in comparative linguistics and evolutionary biology, as well as with the practices of scientific historians, considered generally. But this is not obvious. Paleontologists did not begin by postulating an unspecified common cause for the abrupt end of the fossil record of the dinosaurs. They began by proliferating a large number of fairly specific hypotheses, some postulating common causes (e.g., a meteorite impact, extensive rift volcanism, global climate change, a nearby supernova, evolutionary senescence) and others postulating separate causes, e.g., global climate change decimated them and a subsequent meteorite impact delivered the coup de grace. With a collection of “plausible” rival hypotheses in hand, they set about comparing them, searching for additional considerations, both theoretical and empirical, to discriminate among them. During different eras different sets of background beliefs rendered some hypotheses more credible than others. Evolutionary senescence was taken seriously in the early part of the twentieth century, when Lyell’s *uniformitarianism* (which rejected global catastrophes as explanations of geological phenomena) still dominated thinking in geology, but this is no longer the case. To wit, it is now widely believed that the dinosaurs were doing just fine until they had a *really* bad day 65 mya! Importantly, certain possibilities, e.g., a nuclear war among intelligent dinosaurs or a planet-wide ecological experiment conducted by evil space-faring extraterrestrials, were never considered viable.

The above discussion underscores an important point. Background beliefs play crucial roles at every stage of historiographic research, and this means that scientists

do not begin by deciding between an unspecified common cause hypothesis and an unspecified separate causes hypothesis. On the contrary, they start with tentative conjectures about what might have produced the puzzling traces under investigation. This is true even in evolutionary biology, one of the examples that Tucker cites in support of his view; he cites Darwin's inference of a common origin for the Galapagos finches. Our understanding of the phylogenetic significance of the mammary glands of female lions, whales, elephants, and humans is reinforced by anatomical and genetic studies revealing even deeper similarities among them. Analogous studies of bats, birds, and insects do not reveal such similarities, strongly suggesting that their wings are not inherited from a common ancestor. The decision to attribute biological similarities to analogy (separate causes) or homology (common cause) thus crucially depends upon additional information, both empirical and theoretical, at the disposal of investigators. This information colors their speculation from the very beginning, leading to tentative conjectures about the character of the cause or causes of the similarities under investigation; certain candidates are never entertained because they conflict with widely accepted background beliefs. The upshot is that Tucker's position is too weak. Scientific historiography begins with tentative speculations about the nature (pertinent causal characteristics) of the events being postulated as ancient causes.

On the other hand, Sober's position seems too strong. Scientists do not begin with precisely specified hypotheses. As the history of the debate over the end-Cretaceous extinctions illustrates, they typically begin with several tentative hypotheses, which are invariably vaguely specified. As additional evidence, both empirical and theoretical, accumulates, these hypotheses are refined, culled, and sometimes augmented with new alternatives. If one hypothesis emerges from this drawn out process as the best explanation for the collection of traces available, it is considered confirmed (but not of course proved). This methodology, which is distinctive of most scientific historiographic work in natural history, is not readily explicable on either Sober's or Tucker's Bayesian model.

Carol Cleland (2001, 2002) pursues a different account of the methodology and justification of historiographic natural science. She argues that the distinctive practices of scientists engaged in historiographic research are best understood in terms of a pervasive physical feature of our universe, a time asymmetry of causation known as "the asymmetry of overdetermination" (Lewis 1991). The asymmetry of overdetermination amounts to the claim that localized (vs. global) events typically leave widespread and diverse effects on their environment, many of which persist for long periods of time, providing extensive traces of the past for future historiographic investigations. Consider, for example, an ancient volcanic eruption. Its effects are extensive and diverse, including deposits of ash, pyroclastic debris, masses of basalt, and a cone shaped mountain. Only a small fraction of this material is required in order to infer that the eruption occurred; put bluntly, one does not need every single particle of ash! Indeed, any one of an enormous number of remarkably small subcollections of these effects will do. In contrast, predicting a future volcanic eruption is much more difficult. There are too many causally relevant conditions (known and unknown) in the absence of which an eruption will not occur.

The asymmetry of overdetermination holds that most localized events epistemically *overdetermine* their past causes (because the latter typically leave extensive and

diverse effects) and *underdetermine* their future effects (because they rarely constitute the full cause of an effect). The overdetermination of the localized past by the localized present explains how geologists can confidently infer the occurrence of individual volcanic eruptions that occurred tens of millions of years ago. The underdetermination of the localized future by the localized present explains why it is so much more difficult for geologists to predict the occurrence of even imminent future eruptions. But perhaps the best way to appreciate the extent of the asymmetry of overdetermination is to consider the difficulty involved in committing a truly perfect crime. Footprints, fingerprints, disturbed dust, fibers of cloth, particles of skin, even light waves radiating outward into space must be eliminated. It is not enough to eliminate just a few of them; anything missed may be discovered by a Sherlock Holmes-like sleuth and used to discover and convict the criminal. Furthermore, each trace must be independently undone. You cannot remove a footprint by eliminating a particle of skin or, for that matter, another footprint. In contrast, erasing all traces of a crime before it occurs is remarkably easy, typically requiring the removal of just a single causal factor, e.g., do not fire the gun. This is, of course, the other side of the asymmetry of overdetermination.

The physical source of the asymmetry of overdetermination is controversial. Examples such as a volcanic eruption are commonly attributed to the second law of thermodynamics, which, statistically interpreted, says that the natural tendency of physical systems is to move from more to less ordered states; volcanic eruptions do not reverse themselves, swallowing up all the material that they spewed forth! The asymmetry of overdetermination also extends to radiative phenomena, which do not obviously admit of a thermodynamic explanation. Although traditionally associated with electromagnetic radiation (light, radio waves, etc.), the “radiative asymmetry” characterizes all wave phenomena, including disturbances in water and air. The asymmetry is constituted by the fact that waves invariably spread outwards, as opposed to inwards, as time progresses. Popper’s (1956) example of a stone dropped into a still pool of water provides the classic illustration. Expanding concentric ripples spread outwards from the point of impact, eventually covering the entire surface of the pool. It is easy to explain these ripples in terms of a stone entering the water at a small region of the surface of the pool. Indeed, one could pinpoint where the stone entered by examining a small segment of the pool’s surface. But now consider reversing the process and eliminating all traces of the impact. An enormous number of separate and independent interventions are required all over the surface of the pool. Conversely, try explaining the time-reversed process. Ripples, which expand outwards from the point of impact, now contract inwards to the point of impact. But there is no center of action to explain the simultaneous and coordinated behavior of the individual water molecules involved. From this time reversed perspective, the contracting concentric waves seem to be a miracle; we can understand them causally only by running the process in the other direction in time.

Between the second law of thermodynamics and the radiative asymmetry, all physical phenomena (both particle and wave) are subject to the asymmetry of overdetermination. Thus the asymmetry of overdetermination represents an objective and pervasive physical feature of our universe. It provides the needed empirical underpinning for the principle of the common cause. According to the asymmetry of overdetermination, the present is literally filled with overdetermining traces of long past

events. Hence it is likely (but not certain) that a puzzling association (correlation or similarity) among present-day phenomena is due to a last common cause.

According to Cleland, the distinctive practices of scientific historiography are underwritten by the asymmetry of overdetermination. Just as there are many different possibilities (subcollections of traces) for catching criminals, so there are many different possibilities for identifying past causes of puzzling, contemporary, natural phenomena. Like criminal investigators, scientific historians collect evidence, consider different suspects, and follow leads. More precisely, they proliferate alternative explanations (historiographic hypotheses) for the puzzling correlations and similarities that they observe in nature, and search for additional traces to discriminate among them. The goal is to discover a “smoking gun” that will elevate one hypothesis above the others as the best explanation for the total body of traces currently available. The overdetermination of the past by the localized present, a physical fact about our universe, ensures that such traces are likely to exist if the initial collection of traces shares a last common cause. For insofar as past events typically leave numerous and diverse effects, only a small fraction of which are required to identify them, the contemporary environment is likely to contain many, as yet undiscovered, smoking guns for discriminating among rival common cause hypotheses. Furthermore, if the collection of traces does not share a common cause, the environment is likely to contain evidence that certain subcollections of the original collection of traces are the products of separate common causes. In short, given the asymmetry of overdetermination, it is *highly likely* that the contemporary environment contains enough evidence to discriminate among competing common cause and separate cause hypotheses. The trick of course is finding these traces and recognizing what they represent.

The asymmetry of overdetermination does not *guarantee* that every past event can be identified from contemporary traces. It is extremely unlikely but nonetheless possible for an event to leave no traces; prime candidates are events occurring before the big bang of cosmology. More significantly, with the passage of time, the causal information carried by traces becomes more and more attenuated. It may become so degraded that it cannot be recognized for what it represents, and eventually disappear altogether. Consequently, a significant portion of historiographic research is devoted to analyzing and sharpening attenuated traces so that they can be identified and properly interpreted (Cleland 2001, 2002; Tucker 2004). Identifying traces often requires developing new technologies. Iridium, for instance, cannot be seen with the naked eye. The Alvarez team utilized a particle accelerator (cyclotron) to determine its concentration in K–T boundary samples. Similarly, the 3-degree (Kelvin) background radiation, which provided the pivotal evidence for the Big-Bang theory of the origin of the universe, had not been detected until the development of highly sensitive antennas for communicating with satellites (Kaufmann 1977: 267–77).

Sober (1988) and Turner (2004) worry that information-destroying processes are so pervasive in nature that our ability to distinguish among historiographic hypotheses is, with a few exceptions, fatally compromised. They are assuming, however, that the information is completely lost as opposed to merely difficult to extract. There is good reason to believe that the possibilities for extracting useful information from degraded traces are much greater than they believe. Ancient meteorite craters, for instance, become slowly buried over time until they are no longer detectable from surface features. The

Chixulub crater, thought to be ground zero for the impact responsible for the K–T extinctions, was identified by means of aerial surveys of the northern coast of the Yucatan Peninsula utilizing sophisticated geophysical instruments, which revealed a gigantic (at least 170 km in diameter), circular, gravity anomaly buried a kilometer beneath younger sedimentary rock. Analogously, speculation that life on earth goes back 3.8 billion years rests upon laboratory analyses of carbon isotope ratios in grains of rock as small as 10 μm across weighing only 20×10^{-15} g (Mojzsis et al. 1996: 56). Remarkably, these analyses reveal an enrichment of the lighter isotope of carbon, which is preferred by life, over the heavier isotope, a correlation that is difficult to explain in terms of non-living processes. Who would have thought that convincing evidence for long dead microscopic forms of life could be extracted from rocks this old. As these examples illustrate, our ability to extract information about the past from contemporary phenomena is rapidly increasing, so much so that I suspect the twenty-first century may become the age of scientific historiography!

Turner, nonetheless, believes that such cases are the exception rather than the rule. He cites a hypothesis about the color of a dinosaur as an example. Admittedly we currently have no idea how to determine the color of a dinosaur from its fossil remains. But this doesn't mean that the information is not there. Indeed, this example bears an uncanny resemblance to the claim that one cannot sex a dinosaur from its fossilized remains. A few years ago this claim would have been just about as plausible but, as Schweitzer recently demonstrated, it is false in the case of a certain female T. rex. Even though information degrading processes are common, there is little evidence that they completely destroy information as opposed to merely make it difficult to extract. The extent to which information degrading processes completely remove identifying information about long past causes from their effects with the passage of time is an empirical question, and if recent technological advances are any guide, it may be much less than Sober and Turner believe. Cleland argues that the overdetermination of causes by their effects is extensive and pervasive in our universe. Scientific historians of nature cannot assess the purview and limits of all possible knowledge of the past since they cannot rule out in principle the possibility of discovering a smoking gun for any hypothesis about the past, however far fetched this possibility may currently seem.

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Historians and Philosophy of Historiography

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We have just gone through a period of peculiarly high anxiety for professional historians. Such disciplinary *Angst* was not peculiar to historians, however. Across many of the “human sciences,” the *fin de siècle* appeared a moment of acute crisis, particularly in the disciplines of anthropology and sociology (Zammito 2006). The very sense of the phrase *fin de siècle* entails periodization via *Angst* (Alexander 1995), and the vogue of postmodernism (along with all its constitutive “posts”) might well be historicized in those terms. But my main argument is that, from the outset, historiography has had a very hard time with “philosophy.”

The link of *historiography* with *crisis* has certainly found its way into many prominent recent titles (e.g., Scott 1989). A formidable characterization of this crisis for American historiography came in the last chapters of Peter Novick’s *That Noble Dream*, with their evocative titles: “Every group its own historian,” “The center does not hold,” and especially “There was no king in Israel.” The crowning point of Novick’s final chapter was: “As a broad community of discourse, as a community of scholars united by common aims, common standards, and common purposes, the discipline of history had ceased to exist” (Novick 1988: 628). The philosopher Frank Ankersmit, concurs: “history has irreparably lost what made it into a discipline” (Ankersmit 2001: 152). That is, “there no longer is one or more self-evident disciplinary center from which knowledge of the past is organized” (2001: 153) While others have urged that the “fragmentation” that Novick finds within the discipline might be more positively glossed as democratization, Ankersmit is unwilling to endorse such comfort. Focusing on “the sudden predominance of the notion of memory in contemporary historical consciousness,” Ankersmit notes: “making the word ‘memory’ mean what was formerly meant by the word ‘history’ is a sure sign of a personalization or privatization of our relationship to the past” (2001: 154) Considering the wider political conspectus in which “memory” achieved currency, he sharply distinguishes privatization from democratization.

In yet another vein, Ankersmit cuts deeper into the self-respect of professional historians:

historians feel more insecure about the scientific status of their discipline than the practitioners of any other field of scholarly research. They are painfully aware that historical

debate rarely leads to conclusive results and that such regrettable things as intellectual fashions or political preference may strongly color their opinions about the past. In short, deep in their hearts historians know that . . . history ranks lowest of all the disciplines that are taught at a university. (2001: 250)

In this light, Ankersmit urges historians to face up to the disciplinary crisis: “we must not cowardly shun its boundaries, but always courageously probe and explore the area where the discipline begins to lose its grasp” (2001: 260). There is much food for thought in these comments, and what follows is an effort to come to terms with them.

While the crisis in anthropology and in sociology was a crisis of confidence in the *theory* each discipline had wrought for itself over a century of institutionalization, the crisis in historiography appears to have been a crisis over the *absence* of theory. The gallingly frequent allegation has been that historians appear constitutionally incapable of theoretical self-scrutiny. They have reveled in their craftwork, their “archive-fetishism,” their insistence upon the sufficiency of source-criticism. As much as they have insisted on their autonomy from other disciplines – and especially from philosophy or sociology – they have also insisted that they practice “science” of a sort – though, as Ankersmit pointed out in the passage above, they are also anxiously aware that it is hard to make out historiography to be science at all. A few historians simply opted for the view that what they practiced was art, but this has been highly unfashionable within the discipline, where the distinction of historiography from fiction – and thus ostensibly, though not necessarily, from artifice – has proven constitutive. While even into the most recent past for the majority of practicing historians “theory” or “philosophy” constitute unnecessary distractions from their “real work,” there has been a growing concern with *method*, and beyond that, in the wake of a searing sandstorm of criticism from outside the discipline, with *epistemological* self-examination.

It is far from clear what exactly such terms as *method*, *theory*, and above all *epistemology* should be taken to signify, and, more concretely, what a professional historian is obligated to *know* about them. As I once put it and still believe, “Historians qua historians are not philosophers, and it does not serve them well either to turn into philosophers or to despair of their projects for not being philosophy” (Zammito 1992: 812). One of the essential problems that has made for such fraught relations between historiography and philosophy is the need to spell out an appropriate division of labor and responsibility in these matters. Philosophers have never felt the least hesitancy in explaining what historians were doing (or still better, *ought* to be doing) even if historians did not know it. Thus, Aviezer Tucker observes: “Historians who deny the use of theory are unaware of the theoretical underpinnings of their own practices. Such theoretically unconscious historians may accompany their methodological practices with rituals” (Tucker 2004: 83). Disdainful of such “craft” conceptions, Tucker asserts the philosopher’s prerogative to elucidate what is really going on in historiography. A consideration of the last century shows repeated instances of philosophy coming forward to serve as conceptual warden for historiography, uninvited and unappreciated by historians. In the last decades a similar intervention has arisen from literary “theory,” and, again, historiography has been “invited” to construe its disciplinary practice under the auspices of another. The question posed by this so-called *linguistic turn*

bluntly reads: “is historiography fiction?” (Curthoys and Docker 2005). The question is *quite* “rhetorical.”

A discipline is indeed on very shaky ground if it must make itself the ward of another. It is not surprising that historians feel called upon to offer a “defense,” and to query whether one is needed seems a bit deaf to the undertone of hegemony in the interventions of other disciplines. But what, exactly, is it that historians are to defend? What *kind* of defense is needed? Assuredly, historians are motivated to affirm and enforce disciplinary practices in order to perpetuate those practices. Thus one might best construe such notions as “method” as *internal* forms of self-clarification.

One asks not simply *how*, but *why it is possible* to recount the past, and *how* it is possible to judge *how well* it has been done. These latter questions are *epistemological*. They belong largely within disciplinary philosophy, but they bear heavily and inescapably on historiographic practice. To be ignorant of these considerations is not to be independent; it is to be naive. It is, in Tucker’s terms, to reduce a discipline to a ritual. On the other hand, to carry on as if epistemological issues have been *resolved* is a naivety of a different sort, from which philosophers must regularly rescue one another.

Martin Bunzl put it clearly: “the practice of history has to contend with philosophers of history that exist in two different worlds. One is inhabited by philosophers, the other by historians” (Bunzl 1997: 1). The challenge is to integrate these two perspectives. While Bunzl makes the classic philosophical posit in this situation, i.e., “practice may not be capable of providing the brute data to decide between competing philosophies” (1997: 3), a practicing historian might make the converse observation, namely that what Bunzl offers is so esoterically philosophical as to lose purchase on the problem of historiographic practice. Bunzl draws from Peter Novick’s “exhaustive” treatment of American historiographic practice the conclusion that it is “obvious just how little *philosophical* aspects of the question of objectivity in history have played a role in the last one hundred years of the profession” (1997). And he then writes a philosophical analysis of the problem of realism that is remarkable for the absence of historiographic practice except as the ostensible beneficiary of his elucidation, and even then, with no self-evident implications for the manner in which historians should conduct that practice. That is the ironic core of the problem.

What follows has three parts. First, I review very briefly the longstanding disciplinary concern of historians regarding philosophy. Second, I evoke the more recent challenge posed by the “linguistic turn,” or postmodernism in historiography. Finally, I address a body of recent writing by practicing historians seeking to achieve a responsible command of the relation of historiography to philosophy.

A Perennial Crisis? When “Historiography” Faces “Philosophy”

The relation between historians and “philosophy” has been fraught from the outset. When historiography was only a discourse and not yet a discipline, already Aristotle (1977) disparaged it not only vis-à-vis philosophy but even vis-à-vis poetry for its failure to achieve the vantage of the universal. As historiography established itself as

a discipline between the late eighteenth and the mid-nineteenth century, it struggled explicitly to free itself from philosophy of history. Historiography achieved its “paradigmatic” form in the mid-nineteenth century, in Ranke and his school, as explicitly *anti-philosophical*.

Ranke has been all too frequently encapsulated in one German phrase, *wie es eigentlich gewesen*, and dismissed as unrealistic. That hardly does justice to his monumental stature as the founder of scientific historiography. The impact of his historical seminar, of his philological method, of his emphasis on archival research, and of his vision of letting the facts speak for themselves (however unrealistic) stamped the balance of historical practice irrevocably (Iggers and Powell 1990). In that measure, Aviezer Tucker is quite right to argue that Ranke set the paradigm for historiography, but he is also right to suggest that this was a paradigm that could only partially order the practice of the discipline, and the residual would remain unruly (Tucker 2004: 209ff). Indeed, some of those residual elements were palpable within Rankean practice, rendering it conflicted. On the one hand, however resolutely anti-philosophical, Ranke could not practice his source-criticism after the fashion of a crude empiricism. Instead, he operated with tacit metaphysical presuppositions. Similarly, he could not organize his monographs without the intervention of his personal tastes and faiths – religious, political, and aesthetic – however much he wished to efface his presence as author. And the discipline that crystallized around his model could do no better. While there was a clear effort to systematize and make explicit the method Ranke exemplified, to articulate what the Germans called *Historik*, there was simultaneously a more blatant embrace of the political and other values that had been beneath the surface in Ranke (Iggers 1968; Lieberson 2002). Droysen (1977) is exemplary of both impulses: toward methodological explicitness and towards political advocacy. The “scientific” historiography of the rest of the nineteenth and much of the early twentieth century manifested both this methodological allegiance to Rankean approaches and political allegiance to the nation as the authentic bearer of historical meaning and purpose (Baker 2002; Novick 1988).

Even as Ranke and his associates promoted historiography to a disciplinary “human science,” the question of the “logic” appropriate to such inquiry became vexed over the course of the nineteenth century. John Stuart Mill (1988) particularly probed these questions. His notion of the “moral sciences” came to be translated as *Geisteswissenschaften* in German, and in the late nineteenth century the legitimacy of the methods associated with these pursuits, especially their claim to “science” (even in the more generous German sense of *Wissenschaft*), came to be challenged by the mounting positivism of the age, and in the German context more specifically by the rise of Neo-Kantianism. Dilthey’s lifelong project of a “critique of historical reason” and Rickert’s systematic efforts to distinguish between the natural and the human sciences constituted the decisive discourse of late-nineteenth century Germany (Dilthey 1989, 1996, 2002; Rickert 1986). It was paralleled by the famous *Methodenstreit* in which the idea of a “social science” was hammered out that would be both methodologically and epistemologically superior to historiographic practices (Weber 1949; Antoni 1959).

The tension between the logics of inquiry espoused by the natural and the human sciences, resulted, by the early twentieth century, in what came to be called the “crisis

of historicism.” This crisis dominated the decade of the 1920s in German philosophical and historiographic circles (Bambach 1995). While historians were troubled, it would appear that it was “theorists” who mainly explored the consequences of this “crisis,” whether sociologists of knowledge like Karl Mannheim (1924) or philosophers of “historicity” like Martin Heidegger (1962).

Simultaneously, the received notion of “scientific” historiography in the United States – often directly transferred from the German, and at most mediated rather transparently via British or French methodological manuals – came to similar crisis in the relativism associated with the “new historians” Carl Becker (1935) and Charles Beard. “Becker and Beard were not driven primarily by philosophical considerations. Their concerns were much more attuned to purely internal debates about the function and role of history and its relationship to presentist interests” (Bunzl 1997: 5). While professional philosophers like Maurice Mandelbaum (1967) vituperated against the historians’ relativism, the latter were unshaken in their understanding of the character of disciplinary practices. This relativism troubled historiography substantially until the end of the Second World War and the dawn of the Cold War, when political exigency seemed to override epistemological nicety and, at the same time, a new surge of social-scientific confidence in “modernization,” under the auspices of Talcott Parson’s social-theoretical synthesis, became attractive to historians seeking foundations (Novick 1988). The consolidation of “Social-Science History” or simply social historiography was the defining feature of disciplinary life in the post-war epoch, whether in British Marxism, the French *Annales* program or American and German “Social-Science History.” It was only as this self-confident impulse faltered at the close of the 1970s that the moment arose for the postmodern intervention, which was associated but not identical with the shift to a “New Cultural History” (Stone 1979; Hunt 1989).

Meanwhile, philosophy once more intervened to trouble historians. Carl Hempel (1959), simply as a matter of testing a general theory against a recalcitrant case, proposed his famous “covering-law” model of historiographic explanation, plunging historians into one of their rare, but for that very reason raw confrontations with philosophy. In fact, it occasioned one of the few formal and collective negotiations to sort out the relative perceptions of the two disciplines, the encounter at the fifth annual New York University Institute of Philosophy in 1962 (Hook 1963). The “covering-law” model proved, even for philosophers, a poor conceptualization of historiographic practice, but that seemed to raise questions about whether historiography deserved *any* status as science (Dray 1957, 1966; White 1965; Danto 1965). Historians rallied to the defense of their practice, even if it meant forsaking their “scientific” status for “hermeneutics” and “rhetoric” along lines sketched most influentially by Clifford Geertz (1973, 1983).

A key exemplar of this response to philosophy was formulated by Jack Hexter (1971a, 1971b). Hexter had become by the late 1960s one of the most powerful and discerning commentators within historiography, respected not only in his own field of early modern European social historiography, but more widely. His judgments of historians and historiographic controversies, pronounced with wit and verve, carried enormous weight in the discipline. Yet when he set about disputing the claims of philosophers, in particular those of the covering-law persuasion, his recourse to “rhetoric” appeared gravely wanting to a philosophical critic. Bruce Kuklick (1972) presented a withering criticism of Hexter’s arguments, finding them philosophically insipid. Here

was another painful failure to communicate across the disciplinary divide, though what was at stake was of urgent interest to both disciplines.

As it happens, a more powerful rejoinder was taking form. Willard van Orman Quine had already raised some trenchant philosophical objections to the logical empiricism with which Hempel was affiliated, and after Thomas Kuhn's *Structure of Scientific Revolutions* [1962] (1970), the whole received view of science came unraveled, leaving the "covering-law" model a relic of a displaced philosophical system (Zammito 2004). The prospect of retrieving the notion of "science" for historiography emerged following the new philosophical understanding of science as vastly more subtle and heterogeneous, even hermeneutical (Galison and Stump 1996). Yet, instead, the "rhetorical" and the "interpretive" or hermeneutic self-defense galvanized against the "covering-law" intervention made historiography all the more open to the intervention of literary "theorists."

The Poststructuralist/Postmodernist Challenge

Frank Ankersmit has analyzed an important ambiguity in the notion of the "linguistic turn." It refers at once to a development in the analytic philosophy of language and to a development in literary theory associated with the assimilation of poststructuralism. As Ankersmit argues, they can have radically disparate implications for historical practice (Ankersmit 2001: 29–39; Zammito 2005). The challenge that sent historiography into acute crisis at the end of the twentieth century was clearly grounded in the second of these two "linguistic turns," the one emanating out of literary "theory" from poststructuralism. This is what has come to be taken as the postmodern moment.

It will not do to minimize the radicalism of the challenge that poststructuralism posed to historiography. While, to be sure, there have been hysterical responses to this intervention (Windshuttle 1997; Marwick 1995), the insinuation that the claims propounded by poststructuralism have been misrepresented in alarmist terms is false. The claims were articulated in the boldest and most aggressive of manners, and it is simply bad faith to pretend they were never so meant. Hyperbole pervades the rhetoric of poststructuralism and postmodernism (Zammito 1993, 1998, 2000). Historiographic practice was a prime target of these hyperbolic gambits, beginning already with Lévi-Strauss's (1985) dismissal of historiography in favor of a structuralist synchronicity modeled upon Saussurean linguistics. Derrida (1978) radicalized Saussure into a full-fledge pantextualism with the explicit intention of problematizing the possibility of determinate reference and hence of empirically warranted intersubjective knowledge. Roland Barthes (1981) took the linguistic/rhetorical approach a step further in radically querying all referentiality in historiographic discourse as a mere "reality effect," assimilating historiography entirely into fiction. And Jean-François Lyotard (1984) argued that the possibility of grand narratives had been definitively refuted. As Iggers puts it, "were one to accept the premises of this critique, meaningful historical writing would be impossible" (1997: 11). That was why David Harlan (1989) could celebrate that poststructuralism "plunged historical studies into an extended epistemological crisis."

“The basic idea of postmodern theory of historiography is the denial that historical writing refers to an actual historical past” (Iggers 1997: 118). To explain the undeniable impact of this “sublime” rhetoric, it is necessary to do a bit of contextualizing. Within the human sciences in the Anglo-American tradition, an “interpretive turn” had already asserted itself against the once-orthodox structural-functionalist social theory of Talcott Parsons (Hiley et al. 1991; Gouldner 1970; Cole 2001; Long 1997). Symbolic interactionism in sociology was paralleled by symbolic anthropology, above all in the work of Clifford Geertz. Hermeneutic historicism, drawing on Hans-Georg Gadamer’s pivotal *Wahrheit und Methode* (1960), had made similar inroads within historiographic theory. Peter Winch (1958) had drawn upon the writings of the late Wittgenstein to argue for a starkly interpretivist social science. Perhaps the most fortuitous or epochal convergence arose in the arguments which Michel Foucault (1970, 1972) made for the radical alterity among historical epistemes, on the one hand, and Thomas Kuhn’s roughly simultaneous articulation of a theory of paradigm incommensurability, on the other. This was the methodological milieu into which the post-structuralist critique inserted itself.

The scale of American historiography shifted so dramatically in the second half of the twentieth century that its internal infrastructure could no more be sustained on the old basis than the folkways of a traditional nineteenth-century village could orchestrate the commerce of the “instant cities” of the twentieth century. “Over the last thirty years there has been an almost exponential increase in the total volume of historical knowledge, occasioned not least by the unprecedented growth in the number of historians that has taken place over this period” (Evans 1999: 153). Within each of the so-called “fragments” of historiography there were as diverse a set of projects of inquiry as had characterized traditional historical practice in the “good old days” of ostensible disciplinary unity. There is no ground to suspect (indeed, much to the contrary!) that the rigor of their mutual critique has been in any measure less effective than what obtained in the old, WASP male profession of the pre-war epoch.

As the historiographic profession went through a dramatic crisis of employment and configuration across the 1970s and 1980s, sharply new fields of inquiry were paralleled by dramatically reconstituted demographics of representation within the discipline. The 1960s had brought about a pervasive political-social upheaval. The so-called “new social movements” drew in their wake important academic articulations. Most prominently, second-wave feminism unleashed a transfiguring burst of new historiography – of women, of sexual orientation, and of gendered bias in “scientific” discourses. The postmodern embrace of “theory” was taken to be the concomitant of the new social movements’ embrace of democratization, of the empowerment of the hitherto excluded. One of the features of the moment was to presume that to be democratic in the institutional life of the academy or the society entailed becoming postmodernist in one’s theoretical orientation (e.g., Scott 1988). That illusion did not last long, and soon a vitriolic leftist critique of poststructuralism/postmodernism suggested that it was in fact ivory tower-quietist or even elitist-conservative (Said 1983; Jameson 1991).

That did not restrain the real conservatives from yoking the two and demonizing democratization with postmodernism. In the context of a turn to the right in the politics of English speaking countries, state support for universities suffered economic cutbacks and social decline (Thompson 2000; Evans 1999: 171). “The relative economic

position of academics declined, as their salaries now fell behind those of other groups . . . with whom they had traditionally been compared.” Evans suggests “many aspects of postmodernism can be understood, sociologically, as a way of compensating for this loss of power” (Evans 1999: 172; Vincent 1995). While postmodernists whined about their “marginalization” within the disciplines, the real marginalization was vis-à-vis society at large: what Wulf Kansteiner (1996) termed “the growing insignificance of traditional historiography” in the public sphere. Within the especially beleaguered humanities in American academia, a “ghetto” mentality of aggressive but ineffectual politicization proliferated under the rubric of “cultural studies” (Grossberg et al. 1992; Warren and Vavrus 2002). Ostensibly, “interdisciplinary,” it carried out an *anti-disciplinary* agenda, grounded in the view that the claims of poststructuralism/postmodernism had been definitively established. It was in this context that the climactic debate about postmodernism in historiography emerged.

The postmodern challenge arrived with Hayden White’s *Metahistory* (1973). While the reception of this work within the historiographic community took time and remained often grudging, it eventually spurred a train of more energetic interventions – by White himself (1978, 1987) and by figures like Dominick La Capra (1983, 1985, 1989), Hans Kellner (1989), Linda Orr (1986), Lionel Gossman (1989, 1990), and Stephan Bann (1981, 1983, 1984, 1990) who added to and supplemented White’s work and gave substance to the form of his textualism. More systematic articulations of the significance of White’s linguistic turn for historians came from Hans Kellner and Frank Ankersmit in a special issue of *History and Theory* in 1987 and an influential anthology, *A New Philosophy of History* (1995). There were other uptakes of the two dominant poststructuralist gurus, Derrida and Foucault. Following Derrida, Derek Attridge, Geoff Bennington, and Robert Young published their collection exposition, *Post-Structuralism and the Question of History* in 1987, to be followed by Young’s solo venture, *White Mythologies: Writing History and the West* in 1990. On Foucault a huge literature proliferated among historians and social theorists, including Hayden White himself (Dreyfus and Rabniow 1983; Gutting 1989; Caputo and Young 1993; Goldstein 1994; Dean 1994; Noiriel 1994; Gutting 1994). Reverberations proliferated across the discipline in the major journals either in the form of individual overviews or sharp exchanges (see Jenkins 1997). David Harlan (1989) and Sande Cohen (1988) represented particularly striking instances. Keith Jenkins (1991, 2003) put himself forward as the most energetic propagandist for postmodern historiography, both in polemical manifestos and in a key anthology of the important contributions to the debate that had caught up the discipline. His associate, Alun Munslow (1997), added his own overview. Perhaps the most discerning and demanding formulation of the postmodern position came in Robert Berkhofer’s *Beyond the Great Story* (1995). As Richard Evans aptly observed, “by the late 1990s, therefore, there can be little doubt that the debate about history, truth, and objectivity unleashed by postmodernism has become too widespread for all but the most obscurantist to ignore” (Evans 1999: 5–6).

As Evans puts it elsewhere in his work, “The question is now not so much ‘What is History?’ as ‘Is it Possible to Do History at All?’” (1999: 3). The sense of disciplinary crisis that arose from all this is clear not only in the little overview from Norman Wilson, *History in Crisis? Recent Directions in Historiography* (1999) but in the title of Georg Iggers’ work, *Historiography in the Twentieth Century: From Scientific Objectivity to the*

Postmodern Challenge (1997), and in that of Ernst Breisach, *On the Future of History: The Postmodernist Challenge and Its Aftermath* (2003). Yet, as the latter title hints, there is a sense that the discipline may have passed through the extremity of the crisis, a sense that is confirmed, with some regret, in *Is History Fiction?* (Curthoys and Docker 2005). As Iggers concludes, “nowhere has the belief that language refers to reality been given up, as it was in the reinterpretation of Saussurean linguistic theory by Barthes, Derrida, and Lyotard” (1997: 126).

One powerful piece of evidence for the ultimate failure of postmodernism in historiography has been the want of effective exemplars in practice (see Burke 2001). After the “decades long, ardent push toward radical change in historiographical practice,” Ernst Breisach notes, “the harvest was poor of historical accounts that fully qualified as poststructuralist postmodernist.” And “the absence of persuasive successes in the translation of postmodernist theories into viable historical writings” is, he concludes justly, “the most telling indicator” of the problems of those theories (2003: 200–1). The one instance to which most commentators point is Simon Shama’s *Dead Certainties* (1991), but it is unequivocal that this work has roused far more criticism than emulation (Strout 1992). Patrick Joyce (1998) might choose to celebrate “The Quiet Victory” of postmodernism, but Iggers has the stronger case: “from the perspective of the 1990s, Joyce’s position seems much less convincing than it did a decade earlier” (1997: 135). The question that concerns us here is how historians *now* construe their relation to philosophy.

Practicing Historians and the Challenge of Philosophy

Richard Evans makes the essential point: “very few historians have possessed the necessary expertise to discuss the theory of history at a level that a trained philosopher would consider acceptable.” And, consequently, “some have indeed argued that the nature of historical explanation is best left to philosophers.” But he is clear that historians cannot relinquish the matter to philosophers for two reasons: first, because it is not clear that their interests are shared or, second, that philosophers really have a clue what it is that historians do. This is the vexed and essential issue that needs to be explored, here, precisely through the endeavors of practicing historians to grasp the philosophical issues of their practice. As Evans rightly urges, “Historians themselves have an important contribution to make to this debate. The theory of history is too important a matter to be left to the theoreticians” (Evans 1999: 8–12). But Georg Iggers makes the inverse and equally essential point: “A history of historiography that takes into consideration only factors internal to the discipline is not possible” (1997: 18). As Ludmilla Jordanova has observed, “attacks upon the status of historical knowledge have come largely from other humanities disciplines” (Jordanova 2000: 92). Clarification can only come in an authentically “interdisciplinary” negotiation in the difficult terrain *between* historiography and philosophy.

Telling the Truth about History (1994) is a revealing document about how practicing historians come to terms with philosophical issues. Written collaboratively by three of the most distinguished historians in the United States, it is a work that is as much an intervention in the public discourse of American political life as it is a focused

investigation of the issues of philosophy and historiography. The three authors write emphatically from the situatedness of their own careers and practices in American academia in the late twentieth century. They view shifts in historiographic practice against the backdrop of shifts in the political-cultural life of the nation (and the wider world), and make their major argument that the specifically philosophical issues revolving around the postmodern challenge arose and can only be construed in that wider context. They set out from the observation that “confidence in the value and truth of knowledge eludes just about everyone,” that “a fluid skepticism now covers the intellectual landscape, encroaching upon one body of thought after another” (Appleby et al. 1994: 243). The *Angst* about knowledge is clear. What the authors offer is a particular conception of how and why it has arisen: “The chief cause of the present crisis of knowledge is the collapse on all fronts of intellectual and political absolutism . . . giving way to the recognition of the multiplicity of points of view and their importance in generating knowledge” (1994: 276). That is, “our central argument is that skepticism and relativism about truth, not only in science but also in history and politics, have grown out of the insistent democratization of American society” (1994: 3). With the “democratization of the university,” entailing both its quantitative mushrooming and its ethnic diversification, recognition of the bias that pervaded the academic establishment and its links to dubious political and cultural impulses in the wider society led to sharp skepticism among the new entrants. “By the last quarter of the twentieth century the Enlightenment’s vision of disinterested, unfettered, value-free truth conquering superstition and ignorance, and always in the service of progress, no longer appeared entirely relevant or even credible” (1994: 189). Thus, “groups only recently admitted to the university proved especially receptive to skeptical postmodernist claims” (1994: 217). Identifying themselves, not least as women, with this disenchantment, the authors urge, “the democratization of the university has made the dilemma posed by skepticism and relativism especially urgent. Relativism is now an issue in every branch of knowledge,” and, “given the issues about truth and relativism that were raised late in the twentieth century, historians cannot pretend that it is business as usual. It is essential to rethink the understanding of truth and objectivity” (1994: 10, 194). Accordingly, they felt compelled to take a stance: “It is time we historians took responsibility for explaining what we do, how we do it, and why it is worth doing” (1994: 9).

Appleby, Hunt, and Jacob recount three peripeties over the course of the twentieth century: in the idea of science, in the idea of a “scientific” historiography, and in the privileging of the nation as the vehicle of history. What they call “heroic science” is *scientism*, the positivist model of scientific method as the standard of all truth and as the engine of all meaningful human progress. “Heroic science went wrong by grounding objectivity upon value-free, neutral experimentation” (1994: 260). One of the most important endeavors of their account is to debunk this vision of science, utilizing the work in science studies since the Second World War. They suggest, persuasively, that disciplinary historiography came under the sway of this “heroic” model of science in the constitution of its own practices, with historians until very recently striving to “turn themselves into neutral and passionless investigators in order to reconstruct the past exactly as it happened” (1994). Thus, in both the natural and the human sciences, “positivism has left as its principal legacy an enduring dichotomy between absolute

objectivity and totally arbitrary interpretations of the world of objects" (1994: 246). Neither of these extremes is reasonable, and the authors are entirely correct to believe that no resolution of current issues of knowledge can be attained until this dichotomy has been definitively set aside and a rethinking carefully undertaken. But what they add into their account, attuned as they are to the social and political context in which the disciplinary developments have arisen, is that there was a powerful parallel current in the constitution of historiography from the nineteenth into the late twentieth century: "Building the nation became an absolute value" (1994: 241).

It was this idea of the nation that came to be dramatically challenged in the era of democratization and multiculturalism. That conflated epistemological challenges with political ones, which has occasioned delusions on all sides. While the authors "embrace . . . a multicultural approach to human history," they ask themselves: "Is history supposed to create ethnic pride and self-confidence? Or should history convey some kind of objective truth about the past?" And: "Does every group or nation have its own version of truth? Is one history as good as another?" (1994: 4–6). The authors see themselves personally invested in constructive pursuit of a democratic, multicultural historiographic practice and yet find themselves both unwilling to accept the extreme versions to which such an approach has been taken by its enthusiasts and outraged by a "traditionalist" – or, more accurately, right-wing – penchant to lump their concerns with these extremists: "postmodernism is attached, like a clanking tin can, to the tail of multiculturalism," as right-wing advocates "use the most extreme views of some postmodernists to disparage all critics of the status quo" by "lumping postmodernism, multiculturalism and even social theory together as one target" (1994: 272, 275).

In light of this political-cultural contextualization of the disciplinary crisis, the authors situate the challenge in terms of the "text analogy," i.e., the amenability of all aspects of cultural or historical life to construction as a text, on the one hand, and on the other, their inscrutability in just that same measure. If everything can be construed as a text and all texts are inscrutable, then everything becomes inscrutable. With "the rising influence of literary theory in all forms of cultural studies in the last two decades," postmodernism came to "challenge convictions about the objectivity of knowledge and the stability of language." Quite simply, "no reality can possibly transcend the discourse in which it is expressed" (1994: 204). The authors go along with this critique a considerable way, but they draw the line at its hyperbolic extrapolation into a total skepticism and relativism. Recognizing this hyperbole as in fact an "inverted positivism," i.e., an argument drawing on the unwarranted dichotomy posed by "heroic science," they urge a provisional, "practical realism" (1994: 247). This "practical realism thwarts the relativists by reminding them that some words and conventions, however socially constructed, reach out to the world and give a reasonably true description of its contents" (1994: 250).

In just that measure, they believe, historiography resists dissolution into mere fiction. The constraint evidence from the past exercises upon historiographic reconstruction, while never complete, cannot be presumed inconsequential. Accordingly, knowledge claims can be advanced. These claims, the authors elaborate, find their appraisal in a social context: the relevant scholarly community, i.e., academic historiography. This is the new and relevant sense of "objectivity" in a post-positivist context: "the objective does not simply reside within each individual, but, rather, is

achieved by criticism, contention, and exchange . . . Objectivity is not a stance arrived at by sheer willpower . . . Instead it is the result of the clash of social interests, ideologies, and social conventions within the framework of object-oriented and disciplined knowledge-seeking” (1994: 195). This emphasis on the social dimension of epistemology is crucial, evoking the work of philosophers Helen Longino (1990: 2002) and Miriam Solomon (2001). In the context of a “practical realism,” the authors express their confidence that “the system of peer review, open referencing, public disputation, replicated experiments, and documented research – all aided by international communication and the extended freedom from censorship – makes objective knowledge possible” (Appleby et al. 1994: 281).

Consistent with their democratic and multicultural commitments, the authors associate disciplinary objectivity with negotiated and tentative agreements. While the *judgments* about historiographic questions may be ultimately irreconcilable, the different *perspectives* from which interpreters construe those questions need not be, but can, in the context of free and open contestation, be integrated. The negotiations within the discipline, they affirm, are “pragmatic,” i.e., they aim at resolutions of disputed questions in the light of, and in the measure that they address the interests that motivated them. Pragmatism needs and professes no absolutes. It is resolutely situational, committed to “a workable truth communicable within an improvable society.” The pragmatic, disciplinary quest for objectivity as negotiated judgment will be as strong or as weak as the commitment of the members of the discipline to democratic values both within and beyond the institution.

Telling the Truth about History has come in for a substantial amount of criticism, some of it justified, of the level of philosophical sophistication of their discussion (see *History and Theory* 34 (1995): Forum, 320–33). But some of it instances the very problem the book sought to address. Thus, with a staggering pomposity that tempts rejoinder in the same chauvinist coin, Arthur Marwick dismisses the book as an “appallingly American mix of grovelling and slang” (Marwick 2001: 300). As another of those “bumptious and overly insecure Americans” (Marwick 1993: 111), I find Marwick is transcendently confident in the “strict historical methods and principles” that make “history . . . an autonomous discipline with its own specialised methods” that cannot be “merged with any other approaches” (Marwick 2001: 17). Everything other than his conception of historical practice is, in his view, simply “metaphysical.” He associates this with an “extreme left-wing political agenda” and “one universal, holistic method for dealing with everything, actually Marxism melded with post-structuralism” (2001: 13).

Marwick opines that “historians among themselves know well enough how they go about their own business,” and “a discipline with such an outstanding record of achievement hardly needs the advice and criticism of outsiders (my battle cry!)” (Marwick 1993: 128). Philosophizing about historiography is for him a secondary pursuit, and in several senses: “writing books about other books is always easier than doing the research and solitary thinking necessary to produce a book of one’s own.” He only comes to this lesser labor because *other* “historians speaking about their subject go too far in giving the impression that history is not a body of knowledge at all” (1993: 112–13). On the contrary, Marwick informs us that “an astonishing amount of historical knowledge exists on a staggering range of periods, countries, and topics” (1993: 107).

Moreover, “historians, like scientists, build upon discoveries of their predecessors, bring new evidence, new techniques, and new approaches to bear in refining, correcting, and, sometimes, rejecting existing interpretations” (1993: 114). That is, “history, it is too often forgotten, is a cumulative subject: we advance where our predecessors have previously prepared some of the ground, stimulated by the questions they have left unanswered, or, sometimes, by their manifest errors.” To do this, “it is knowledge and method, above all, which are needed, not theory” (1993: 120–1). That is, “concepts, generalizations (that is well-trodden ground), and indeed theories (but not one over-arching macro-theory) are integral to historical study. But all must emerge from the evidence, be constantly tested against the evidence” (1993: 133). Theory is unwelcome if it is proposed from *outside* historiography, for such theorists “pay no heed to, or dismiss, what it is that historians are actually trying to do” (1993: 128). Thus, Marwick’s core commitment is to the idea of the disciplinary autonomy of historiography. Historians, in his view, “operate in the same *spirit* as natural scientists, always working from the evidence, always basing their generalisations, interpretations, or theses on the evidence (not on metaphysical speculation).” While Marwick has no patience for “muddled platitudes about history being ‘both art and science’” or for “silly metaphors about history being ‘a craft,’” he is aware that there are important differences between historiography and the natural sciences. Historiography entails no ambition for prediction, no mathematization, no ambition to general laws, no replicable experiments. Moreover, “the scope for value judgments, for subjectivity, is much greater in history” (Marwick 2001: 248–9). Nonetheless, he writes in 2002, “I am keener than ever to stress the basic similarities between history and the sciences” (Marwick 2002: 8, n. 10).

In perhaps the most flagrant of his essays, “Two Approaches to Historical Study: The Metaphysical (Including ‘Postmodernism’) and the Historical” (1995), Marwick launched an intemperate attack on Hayden White. White responded bluntly to what he termed Marwick’s “paranoia,” terming Marwick’s “characterization of ‘Postmodernism’ . . . bizarre and uninformed.” (White 1995: 242, 233). What is interesting in White’s irked response is that it restates, at Marwick’s expense, all the standard representations of the entrenched guild mentality of “naive” historians. Thus White notes that Marwick “appears to believe deeply in ‘professional history’ or better ‘professional historians’” faced with “threats to the historical profession’s claims to preside authoritatively over not only what shall count as history and the proper way for it to be studied but also the language that is to be used in any account of it” (1995: 235–6). White alleges that “history is rather a craftlike discipline . . . governed by convention and custom rather than by methodology and theory.” In a dubious sally, White continues: “The very notion of history-as-art has to be disconcerting to a profession which has cast its lot with the more masculinist ideal of history-as-science.” Thus, “historians have systematically built into their notion of their discipline hostility or at least a blindness to theory,” and maintain that “only ‘practising’ or, as the phrase has it, ‘working’ historians are competent to dilate on the question of how and to what purpose historians do the work they do.” But “historians as a discipline have not theorized the *form* of their own discourses in such a way as to be able to teach it other than by a trial and error method.” Historiography tends to “utilize ordinary or natural languages for the description of its objects of study and representation of the historian’s thought about those objects.” And: “most historians are not only incapable

of analysing the discursive dimensions of their writing, they positively repress the idea that there might be such a dimension." Of course, White's whole "metahistorical" endeavor has been to demonstrate that "any historian's account of his or her subject is constrained by *conventions* of language, genre, mode . . . argument, and a host of other cultural and social contextual considerations" (1995: 243–5).

The exchange between Marwick and White might, then, appear a classic dialogue of the deaf, but we can in fact extract a great deal from their respective vehement disputations. As two American historians recently observed, "there is no training and there are no rules for the process of constructing a story out of the disparate pieces of evidence . . . When it comes to creating a coherent account out of these evidential fragments, the historical method consists only of appealing to the muse" (Somekawa and Smith 1988: 150). That was White's essential point about the "craft" character of historiography, its theoretical naivety. But while the description of the "trial and error" character of historiography has a strong ring of truth, it does not follow that there are not – and more importantly that there cannot be – internal disciplinary structures of practice. There are both resources and grounds for seeking, in the welcome phrase of Jordanova, to "set standards of reliability from *within* the discipline" (Jordanova 2000: 93).

Jordanova points out that disciplines themselves are historical emergents, and of relatively recent origin at that. They were invented in order to "mark out territories that are 'theirs,' and the resulting division of labour is designed to produce different sorts of knowledge and to prevent boundary disputes" (2000: 60–3). But of course "disciplines, like those who practice them, cannot be seen as isolated." Thus, "there is no essence of the discipline . . . never clear boundaries around academic subjects." These are all conventions, human institutions, but this does not mean they are not effectual. With respect to historiography, Jordanova argues persuasively that it is "best understood as a set of practices" (2000: 1–2). That is, "to the extent that historians share anything, it is less an intellectual corpus than a commitment to the value of historical study and a shared sense of how relevant sources should be handled . . . [T]here is no body of historical knowledge that underpins the whole field" (2000: 27). Yet there are institutional matrices that govern historiographic practices. "Publications are never free-standing . . . but rather are parts of elaborate conversations with other historians, living and dead. They are also conversations with governments, political parties, interest groups and so on" (2000: 1). This is a crucial widening of the disciplinary context, because "ownership of the past" is a highly contested matter, "political at its core" (p. 7). And there is the further point that "history is a notoriously eclectic discipline methodologically speaking . . . constantly drawing on other disciplines" (p. 28). Thus, when Jordanova calls upon historians to seek standards of reliability from *within* the discipline, she is making a very complex and nuanced demand. Richard Evans objects to the notion of the disciplinary community as guarantor of objectivity: "what counts as evidence is not determined solely by one historian's perspective but is subject to a wide measure of agreement which transcends not only individuals but also communities of scholars" (Evans 1999: 110). It is certainly the case that the web of belief within which any knowledge claim must be negotiated extends beyond a single discipline and in fact encompasses the natural language shared and enforced in the practices of a whole community (Quine and Ullian 1970). But it does not follow that

the division of labor and specificity of inquiry which led to the constitution of disciplines fail to have a significant measure of efficacy.

In a truly admirable effort to present a defense of history from within disciplinary practice, Evans sets out the challenge as follows: "Documents can be read in a variety of ways, all of them, theoretically at least, equally valid. Moreover, it is obvious that our way of reading a source derives principally from our present-day concerns." He elaborates: "What is at issue, therefore, is how historians use documents not to establish discrete facts, but as evidence for establishing the larger patterns that connect them. Are these patterns, these connections already there waiting to be discovered by a neutral process of cognition, or do historians put them there themselves?" That is, does it follow, as postmodernism alleges, that "there is in effect no means of deciding between one translation and another, no means therefore of accurately reconstructing the past meaning of language and therefore the past to which it refers?" (Evans 1999: 69–78). Evans cites Hayden White's assertion: "We can tell equally plausible, alternative, and even contradictory stories . . . without violating rules of evidence or critical standards" (1999: 86, citing White 1973: 5–7). Ultimately, this is so not only because of the self-referentiality of language, according to White, but because there is *no* order in the past as such to be found. This would imply that "the historians' conventional concern with the past would be replaced in a postmodernist history with a focus on self-reflexivity and on problems of literary constitution" (Evans 1999: 84). This is an intellectual cul-de-sac.

A defense of historiography must claim, first, that it is *not* the case that all varieties of reading documents are even theoretically "equally valid." Evans agrees: "Not all sources are equally open to a variety of interpretations or uses, and some indeed can only be reasonably interpreted in a single way" (Evans 1999: 75). Second, the dichotomy that postulates patterns in historiographic interpretation must be either entirely invented or entirely discovered must be rejected. There is no *a priori* ground whereby one can know this in advance. It is an *empirical* question whether patterns are found or made. When White contends that they are "more *invented* than found," he is in fact making an empirical judgment which tacitly assigns some weight to *each*, and as an empirical assessment of a body of practices, his view can be controverted on equally empirical grounds. In fact, White and his disciples, most notably Hans Kellner, make a *metaphysical* claim that the past has no structure. Kellner (1989) famously used the quip of Stephan Bann to claim that all historians can attempt is to "get the story crooked," as there never was any "straight" story to be gotten. The radical claim that "historians must create meaning from meaningless debris," as Ernst Breisach aptly objects, "pre-supposed a true knowledge of the world prior to its linguistic staging" (Breisach 2003: 77, 82). That begs the question. To invoke a decisive rejoinder from a different philosophical engagement with the same problem, Larry Laudan and Jarrett Leplin have written:

We do not deny the possibility that the world is such that equally viable, incompatible theories of it are possible. We do not deny the possibility of the world's being unamenable to epistemic investigation and adjudication, beyond a certain level. But whether or not the world is like that is itself an empirical question open to investigation. The answer cannot be preordained by a transcendent, epistemic skepticism. (Laudan and Leplin 1991: 459)

And thus, Evans comes to the second crucial point of the defense: “In the end it simply isn’t true that two historical arguments which contradict each other are equally valid, that there is no means of deciding between them as history because they are necessarily based on different political or historical philosophies” (Evans 1999: 189). Invoking the arguments of Thomas Haskell (1998), Evans brings his work to a close with the affirmation that “the truth about patterns and linkages of facts in history is in the end discovered, not invented, found not made” (Evans 1999: 219). It is the task of a philosophical defense of historiography to buttress these two stances.

Mary Fulbrook’s *Historical Theory* is a very serious endeavor to work through these issues. Her objective is to “construct a theoretically tenable route out of the rather entrenched positions of an arguably increasingly sterile debate between empiricists and postmodernists” (Fulbrook 2002: ix). She finds two central issues of concern: first, the multiplicity of theoretical approaches, with the question how to choose among them; and, second, the question of objectivity, i.e., how to distinguish historiography from myth, from special-pleading. She proposes to defend “a view of historical investigation as a matter of collective and theoretically informed inquiries into selected aspects of the past” (2002: 6). At the outset, she expresses dissatisfaction with the stances taken by Evans, Jordanova, and Appleby et al. because she believes their accommodation with pluralism or multiculturalism evaded the ultimate question of adjudication: “There appears to be no rational – or at least mutually agreed – means of adjudicating between these approaches” (2002: 7). Thus, to believe that the “facts” will settle anything, or even that open debate will establish the best explanation, seems to her to fall short of the philosophical rigor the situation demands. It cannot suffice to maintain that all views are equally valid, and it will not do to reduce difference to the political preferences of individual historians. “Perspectives in history are not – or not only – about the personal or political viewpoints of the individual historian, but are filtered through a more complex theoretical net” (2002: 33). While, to be sure, there are “close links between particular theoretical approaches and political standpoints,” Fulbrook proposes to conceive historical-theoretical commitments in terms of the Kuhnian notion of *paradigms*, and to suggest that varying paradigms involve differing levels of commitment and exclusiveness, opening the prospect for limited adjudication among them.

“There is a remarkable degree of ‘craft’ agreement among most practicing historians” about how to handle sources, Fulbrook notes (2002: 101). It is here that a century of intense methodological attention has been directed, and “through the social processes of education, apprenticeship and professional critiques, [historiographic practice is] remarkably sophisticated in actively sustaining a set of assumptions about appropriate questions and procedures in the comprehensive and critical use of relevant sources. Systems of examination, the award of degrees, the professional peer review of books and articles, the building (or destruction) of reputations, ensure that there is a great deal more sophistication in the critical use of sources than might be assumed from those theorists who critique ‘archive positivism’” (2002: 103). Yet it will not do, she continues, to take “source criticism” as the basis of all “methodology.” “All historians operate in terms of categories and concepts which serve to shape and filter the ways in which they investigate and seek to represent the past” (2002: 74). They “have to make decisions in the context of wide debate about what sort of form will

be most appropriate for a particular argument, in a particular context of debate, for a particular audience" (2002: 159).

This is the decisive role that paradigms play, in her view. They constitute a "normal science" (a phrase from Kuhn that Fulbrook does not use, but which I think is crucial): "historians work within collective traditions of inquiry which set certain parameters for which they seek solutions. . . . What counts as an 'answer' to a particular puzzle . . . varies dramatically across different paradigms" (2002: 134). Thus: "Historical inquiry is, I would contend, more akin to participating in collective 'puzzle-solving' than to the individual production of a narrative, however much individual historians take responsibility for their own books, articles, exhibitions, lectures, or other forms of representation." Given the plurality of paradigms within historiography, the question that is central is not only how in each of these historians "actively construct, select and filter the traces of the past," but how the various paradigms can be at least to a degree related and compared (2002: 73). As she writes, "plurality does not preclude falsifiability," not all paradigms are equally valid (2002: 196). While some entrench themselves in ultimate commitments which make such commensurability intractable (she refers to Gallie's (1968) notion of "essentially contested concepts"), Fulbrook argues for the possibility of "cross-paradigm" concepts and degrees of translatability.

Fulbrook and Evans both look to the effort of historians within and across paradigms to work through the problems of overdetermination of events and underdetermination of theory by data. Thus Evans writes, "Almost all historians are used to the idea that historical events are frequently *overdetermined* . . . [T]hey see it as their duty to establish a *hierarchy* of causes and to explain, if relevant, the relationship of one cause to another" (Evans 1999: 135). And Fulbrook observes that historians seek "possible ways (however imperfect and incapable of definitive conclusion) of attributing degrees of relative causal weighting to different antecedents; ways, within certain paradigms, of trying to 'disconfirm' certain accounts on the basis of new evidence or the reconsideration of existing evidence in new ways, and with the limits of collectively agreed means of proceeding in these ways, because of wildly different assumptions across paradigms of inquiry" (Fulbrook 2002: 67). This seems to me a very valuable framework within which to work out the concrete problems of historiographic objectivity.

Concluding Comment

The challenge to build an effective philosophy of historiography in the service of historiographic practice – and not as the imposition of an external disciplinary regimen (literary or philosophical) – remains open. Historians have made serious efforts to work toward that interdisciplinary negotiation. In philosophical works like Bunzl's *Real History* and McCullagh's *The Truth of History* (1998) there is a reciprocal effort to build a coherent grasp of how historiography works. In my view, the postmodern episode contributed little to this endeavor. It is time to get on with both historiographic practice and the philosophical project of its elucidation. We have had enough of pronouncements of their impossibility.

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Part II

Basic Problems

Historiographic Evidence and Confirmation

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It is not only students of human history who reason about the past from evidence. The paleontologist trying to reconstruct an extinct animal from fossil fragments; the engineer charged with determining why a bridge collapsed; the teacher who suspects that a student may have cheated on an exam: all of these form ideas about what happened and seek to evaluate those ideas in the light of the evidence. Accordingly, philosophers interested in understanding historiographic evidence have tended to align their analyses with more general models of evidential reasoning. After a brief survey of different kinds of historiographic evidence and different approaches to understanding it, we provide an overview of the two models of evidential reasoning invoked most frequently by philosophers of historiography, the Bayesian and the explanationist models. We conclude with suggestions as to how these models can aid the formulation and understanding of two central issues in the philosophy of historiography, skepticism, and underdetermination.

What Is Historiographic Evidence?

Historiographic evidence is extraordinarily varied. Bones, pots, and foundations are unearthed. Field boundaries, path routes, and architectural features are examined. The living are questioned orally. Data on economic activity are accumulated and exhibited in the form of lists or tables. At the heart of most historians' reconstructive efforts are written works: memoirs, chronicles, letters, orders, minutes, legal records and more. The class of historiographic evidence is larger still, extending to past facts, no longer open to direct inspection. If yesterday's murder was committed in the kitchen, then the fact that the professor spent the whole of yesterday in the library is evidence of the professor's innocence. Similarly, it is plausible that the German reaction to the Archduke Ferdinand's 1914 assassination is evidence that, well before that event, the Germans harbored bellicose intentions.

For all this diversity, when it comes to understanding what makes historiographic evidence *evidential*, in the most general and abstract sense, there are two major options. An *epistemically internal* understanding of evidence directs us to connections internal to a set of beliefs. Those beliefs whose empirical support is under scrutiny are

typically called “hypotheses.” Those beliefs that provide or fail to provide empirical support to hypotheses – that “confirm” the hypotheses or “disconfirm” them – are the evidence. The position can be defined more precisely as follows:

Internalism = E is evidence for H if and only if H is more confirmed given E & B than given B alone

where E, H, and B all stand for beliefs: E is the evidential belief, H the hypothesis, and B the set of background beliefs relevant to judging the probability of H. Some terminological variety is to be expected. “Justified” can be used in place of “confirmed.” One may “infer” H from E to the extent that E is evidence for H. For the limiting cases of perfectly good and perfectly bad confirmation, there are special terms: “verification” or “proof” for the former, “falsification” for the latter. We develop the internal understanding of evidence below, with specific reference to its dominant variant, Bayesianism, and to the main competitor, explanationism.

An *epistemically external* understanding of evidence is couched not in terms of beliefs and their relations, but in terms of the world of effects and causes about which those beliefs are formed. On the externalist view, a lit match is evidence for the presence of oxygen, the microwave background radiation is evidence for the Big Bang, and a certain quantity of carbon-14 in unearthed bones is evidence for a process of radioactive decay over a certain period (and hence for those bones having been part of an organism that died a certain number of years ago). Such connections obtain whether or not any beliefs are formed about them. More precisely:

Externalism = E is evidence for C if and only if there is a reliable connection between C and E

where E and C are properties or events, with E the effect of the cause C. One must understand “reliable connection” here in the right way. The claim is not that C always leads to E, but that E only follows from C. Other things being equal, a lit match is evidence for oxygen, even though placing a match in oxygen does not always lead to the match’s being lit.

Although the internalist and externalist accounts point in opposite directions, and the internalist picture has attracted far more philosophical attention of late, there are clear relations between the two pictures. It is precisely the connections between the internal and external pictures of historical evidence that supply the underlying rationale for our evidential practices: correct historiographic reasoning leads us to true knowledge about the past. Where the set of background beliefs B referred to by the internalist is correct and complete, then all of the relevant reliable connections referred to by the externalist can be objects of belief and so available to confirm or disconfirm any given H. In this imaginary omniscient state, any true hypothesis could be confirmed in many different ways.

Of course, in our epistemic predicament, we are very far from omniscience, especially about the past. Much of the background information is unknown, including both relevant historical facts and knowledge of the general regularities that would enable correct inference. Much of the evidence is likewise unavailable – evidence that may

one day be discovered, or may remain always hidden. Finally, there is a suspicion that certain beliefs in the historians' background sets will not find a place in the external picture of the world. In the jargon, these beliefs will turn out not to be "truth-apt." We return to these concerns in the final section.

Bayesianism

On the confirmation theory most widely accepted today, to confirm is to render and be rendered more probable. This is the essence of "Bayesianism," a family of doctrines taking its name from the eighteenth-century British clergyman and mathematician Thomas Bayes. Specifically, Bayesian confirmation theory is founded upon two claims. First, the degree to which the belief E is evidence for the hypothesis H depends upon the initial plausibility of H and the degree to which H makes E more likely than it would otherwise have been. Second, to the extent that one accepts that E is evidence for H , one should as a rational inquirer accordingly update one's strength of belief in H .

It is uncontroversial that the Bayesian position captures important elements of our inferential practices, historiographic reasoning included. What is more controversial is the claim that Bayesianism provides a full description and justification of those practices. We shall argue that Bayesianism provides a useful framework for understanding some, though not all, instances and aspects of historiographic reasoning; and that, while it permits a precise statement of core problems for the philosophy of historiography, it does not necessarily provide the solutions.

Bayesianism tends to be formulated in terms of equations linking beliefs with a certain quantitative probability, with 1 the value of a verified belief and 0 the value of a falsified belief. In order to understand these equations, some brief definitions of typical Bayesian terminology are required. The "prior" is the value representing the probability of the hypothesis H before the evidence is in, and is written $(H|B)$, where the symbol " $|$ " stands for "conditional upon," and " B ," as before, is the set of background beliefs. The "posterior" represents the probability of the hypothesis once the evidence has been considered, and is written $(H|E\&B)$. The "likelihood" represents the probability of the evidence, given the assumption that the hypothesis is true, and is written $(E|H\&B)$. Likelihood can be regarded as a measure of how well the hypothesis predicts the evidence. Finally, "expectancy" is the prior probability of the evidence itself, $(E|B)$.

Combining these definitions with the basic axioms of probability theory, one can derive the following two equations. The first is applicable to the assessment of a single hypothesis, and says that the hypothesis posterior equals the product of the hypothesis prior and the evidential likelihood, divided by the expectancy. In symbolic terms,

$$B1: (H|E\&B) = (H\&B) \cdot (E|H\&B) / (E|B)$$

The second equation applies to the comparison of two hypotheses, H and H^*

$$B2: \frac{(H|E\&B)}{(H^*|E\&B)} = \frac{(E|H\&B)}{(E|H^*\&B)}$$

B2 has certain advantages, and so will provide the focus for the remaining discussion. For one thing, it dispenses with the expectancy ($E|B$), and so relieves us of having to estimate the prior probability of the evidence. More importantly, the focus on contrastive confirmation – H versus H^* – brings Bayesianism into line with certain intuitions about evidential reasoning. Consider that, although the death of the former dictator Slobodan Milosevic (the evidence) raises the probability that he committed suicide (the hypothesis), the belief that Milosevic died does not seem in itself to do anything to confirm the suicide hypothesis. The Bayesian who takes hypothesis competition into account can respond by holding that confirmation requires not simply an increase in the probability of a hypothesis, but a comparative raise in probability with respect to contextually appropriate competing hypotheses. In the Milosevic case, we would do well to consider such competitors as that Milosevic died by another's hand, or from natural causes. Given that the probability of the suicide hypothesis is not raised relative to these competitors, the suicide hypothesis is not confirmed, and Bayesianism falls into line with the original intuition.

Bayesianism as a Model of Historiographic Reasoning

Given its quantitative thrust, Bayesianism applies most straightforwardly to historiographic reasoning in which the evidence is explicitly probabilistic, as when a jury weighs the evidence provided by a match between a suspect's DNA and the DNA extracted from the blood on a murder weapon (example taken from Dawid 2002). Such clear quantitative examples are common in natural historiography and comparative historical linguistics, but are unusual in historiographies of the human past. But even where reasoning about the past is entirely qualitative, it often involves judgments of comparative probability; and the norms governing such reasoning can often be modeled well using Bayesian resources.

Some of these norms are obvious and hence usually implicit, for instance, the doctrine of "evidence against interest": the notion that the probability of the truth of the propositional content of testimonies is higher if those claims do not serve the testifiers' interests. But whether the norms are implicit or explicit, intuitive or technical, general or specific to a particular area of study, the Bayesian understands them in a common way: as guides for the judging of probabilities of hypotheses and evidence in the light of each other.

A good example of a technical norm which Bayesianism captures well is discussed by Aviezer Tucker (2004: 113). Those Europeans who, from the sixteenth century, puzzled over the origins of the Central American pyramids tended to contemplate two hypotheses. One hypothesis supposed that the pyramids were basically copies of the Egyptian pyramids – in which case the American and Mediterranean civilizations must have had contact before Columbus. The other hypothesis supposed that there was no pre-Columbian contact, but that the Americans had nevertheless built pyramids like the Egyptian ones because pyramids are a unique solution to the problem of building large structures without advanced engineering techniques. Here, the relevant norm in play amounts to the view that the possibility of two innovations being the result of a generic solution to a general problem undermines the claim that those innovations are

evidence of particular influence. Tucker (2004: ch. 3) goes on to note that this first stage – the comparison of common cause hypotheses to separate causes hypotheses – precedes the detailed specification of information bearing causal chains. Finally, if the evidence is significantly more likely given a common cause, then the historian can compare competing common cause hypotheses; if more likely given separate causes, then competing separate causes hypotheses can be compared.

For all its successes, Bayesianism as a model of historiographic reasoning has its limitations. Consider the large gap between the way that the Bayesian represents belief, as a state continuously varying between 0 and 1, and our usual experience of belief as a discrete state, something held or not. Typically, we halt the weighing of evidence when we have made up our minds on whether to accept or reject a hypothesis. Continuing to weigh evidence thereafter implies not having truly decided on the matter one way or the other. Can the Bayesian account for such definitive, even closed-minded attitudes? Any attempt on the Bayesian's part to provide numerical thresholds beyond which hypotheses should be conclusively accepted or rejected is implausible. There are other responses that look more promising, however. A concessionary response would be to permit a pragmatic element to belief acceptance, such that whether one holds a belief depends upon both Bayesian confirmation and on the value of being committed or remaining agnostic on this particular issue. But taking that option would be to admit that Bayesianism does not provide sufficient resources for hypothesis acceptance and rejection. An alternative, non-concessionary response would be to deny that the discrete-state view of belief offers a correct description of our epistemic experience, and that closer scrutiny of that experience will yield a description more closely conforming to the Bayesian model (see, for example, Jeffrey 2004).

Whether Bayesianism can provide a sufficient model for understanding historiographic reasoning depends on what kind of understanding we want, and what precisely we want to understand. Perhaps no Bayesian has claimed that all aspects of historiographic reasoning are comprehensible in those terms, although Tucker comes close: "Bayesian analysis can explain most of what historians do and how they reach a . . . consensus on determined historiography" (2004: 139). Our claim is that we must supplement Bayesianism with explanatory considerations.

Explanationism

As we have seen, evidence for the Bayesian is what renders hypotheses more or less probable and what is rendered more or less probable by hypotheses. For the explanationist evidence is what hypotheses explain more or less well. Specifically, the explanationist position – or at least the modest version to be examined here – holds that explanation of evidence E by hypothesis H is a guide to inference of H from E. To confirm on this view is to be explained.

It is worth noting at the outset that a good deal of twentieth-century debate in the philosophy of historiography proceeded on the view that inference comes first and explanation follows. First historians discover the facts, then they weave these facts into a narrative, with some of the facts turning out to explain the rest. It is a widely

acknowledged advantage of the explanationist approach that this naïve picture is undermined. On the explanationist view, historians are on the lookout for explanatory facts throughout their inquiries, and attempt to infer those facts according to their power to explain.

For any number of instances of historiographic reasoning, the explanationist model is as good a fit as the Bayesian model; and this very applicability might be a source of concern, for it makes it hard to see what explanationism adds to the understanding provided by Bayesianism. Consider, for instance, the example cited earlier in support of the Bayesian approach, involving competing hypotheses and the Central American pyramids. One might, in a Bayesian spirit, ask: on which hypothesis is the existence of the pyramids more probable? But one might equally, in an explanationist spirit, ask: on which hypothesis is the existence of the pyramids explained? Both questions lead to the same answer. Bayesians like Salmon and Sober and philosophers of historiography like Dray noted that likelihood and explanation and complementary symmetrical moves in the same type of inference.

There are two important replies to the concern that explanationism adds nothing to Bayesianism. First, even if Bayesianism and explanationism fit examples of historiographic reasoning equally well, they may not be equally appropriate as accounts of *how* historians actually and typically reason. Bayesianism is not illuminating if we wish to understand, in particular, how certain hypotheses and not others come to be tested, how new hypotheses are arrived at, and why certain evidence but not other evidence come to be considered. To use the distinction familiar to philosophers of science, Bayesianism tells us much about the context of justification, but not much about the context of discovery. Those philosophers who believe philosophical theories are concerned with rational justification would consider this situation quite satisfactory. We, however, take it to be an advantage of explanationism that it permits a general understanding of the process of historiographic reasoning, including discovery. The central explanationist claim will be that those hypotheses which *would* explain the evidence are those which are formulated and tested.

Explanationism also yields understanding of the structure of historiographic controversies. Historians often challenge reigning hypotheses by emphasizing, re-interpreting, or searching out evidence which those hypotheses appear powerless to explain. In the well-known debate between Gananath Obeyesekere (1992) and Marshall Sahlins (1995) over the alleged apotheosis of Captain James Cook, the arguments turn on requests for the explanation of certain pieces of evidence. If, as Sahlins advocated, the Hawaiians thought of Captain Cook as a god, then why, asks the skeptical Obeyesekere, did the Hawaiian priests not prostrate themselves before him? To take another, more politically urgent, example, consider the controversies surrounding "Holocaust denial." One of the reflections that keeps Holocaust denial a fringe position is that it so conspicuously fails to explain so much – such as why so many Jews and members of other groups such as homosexuals and Roma from Germany and occupied Europe were missing after the war, or how the testimony of those who survived came to be so consistent, and durably so, about German atrocities.

A second point in favor of taking the explanationist approach into account is that explanatory and probabilistic considerations can come apart. To see how this can happen, we need first to clarify the central notion of a "best explanation." Peter

Lipton's work on this point is invaluable, in particular his distinction between the "likeliest explanation" and the "loveliest explanation" (Lipton 1991: ch. 4). The likeliest explanation of E is that which is most probably true, the loveliest explanation of E is that which would, if true, provide the most understanding of E. If the explanationist jumps straight to the former notion, it will indeed be difficult to claim any substantial addition to straightforward Bayesianism. Instead, the explanationist needs to show that loveliness is a guide to, and justification of, likelihood.

Towards an Explanationist Bayesianism

The value of explanatory considerations is clearest in connection with difficulties that arise when judging the Bayesian likelihood value – the probability of the evidence on the assumption that the hypothesis is true. Consider two ways in which such judgments may be reached. One may, in certain cases, reason about evidential likelihood in a straightforwardly deductive manner. If, for example, one's background knowledge contains the law "all corrupt regimes lead to revolutions," one can deduce the likelihood of revolution given the hypothesis of there having been a corrupt regime at 1. And, more generally, where there is a statistical law stating that E follows H in x percent of cases, one may assign the likelihood $E|H$ as $x/100$.

Yet this approach to likelihood is inadequate in two ways. First, deduction is not an infallible guide to inference. The simplest way to appreciate this point is by way of a standard lesson from logic, regarding hypotheses formed of arbitrary conjunctions; say, "Henry XIII had six wives and Louis XVI was beheaded." It is a trivial entailment of this hypothesis that "Louis XVI was beheaded." On the Bayesian scheme, Louis' beheading given the Henry–Louis hypothesis has a likelihood value of 1, making the beheading a verification of the hypothesis – an absurd result. On the explanationist view, by contrast, no such absurdities lurk, since the Henry–Louis conjunction is not remotely explanatory of the Louis part of the conjunction. The second reason why a deductive approach to likelihood is inadequate, particularly with respect to historiographic judgments, is that we are usually in possession neither of clear and exceptionless regularities, nor of well-founded statistical relationships. Again, our suggestion is that one applies the more readily available criterion that the value $E|H$ depends upon how well H would explain E.

Explanationism does not assume any particularly contentious or restrictive notion of explanation. We need only focus on two elements which would feature in any decent theory of explanation: First, explanations of E trace the causal history of E, using a combination of specific knowledge of that history, general regularities, and appropriate analogies. The historian Marc Bloch once described the tracing of the history of how the evidence under scrutiny came into being as "unwinding the film spool": "in the film which he is examining, only the last picture remains quite clear. In order to reconstruct the faded features of the others, it behoves him first to unwind the spool in the opposite direction from that in which the pictures were taken" (2004: 46). Much historiographic reasoning is directed towards this end, including the set of practices taught to every history student under the name "source criticism." Should the pious self-portrayals in volumes of testimony now held in the Vatican be accepted at face value,

as true accounts of how it was? Facts about the history of these testimonials – in particular, that they were collected during the Inquisition, from people who had a lot to lose by being thought impious – might well lead the judicious historian to doubt their veracity (see Boyle 1981). Far from being add-ons in historiographic reasoning, such explanatory considerations are ubiquitous.

Just as theories of explanation are bound to acknowledge our epistemic stake in knowing the causal history of the evidence we contemplate, they are also bound to identify the things that make for better and worse explanations – the explanatory virtues and vices. A second way to demonstrate the relevance of explanatory considerations for evaluating evidential likelihood involves these explanatory virtues, one such being accuracy. Compare, for example, the more specific and accurate hypothesis “there were four Cathars in Montautou in 1310” with the more general “there were a few Cathars in Montautou in 1310.” In terms purely of statistical likelihood, there is no evidence that is more likely given the former than the latter, but plenty of potential evidence that is more likely given the latter than the former. Yet, intuitively, we could imagine evidence which would make us prefer the former over the latter. To account for that preference, we must recognize the necessity of the virtue of accurate explanation – and a Bayesianism unaffected by considerations of explanatory virtue would be unable to account for this preference.

Applications: Skepticism

We conclude by applying the Bayesian and explanationist models of historiographic reasoning to two central issues in the philosophy of historiography, skepticism and under-determination. The following suggestions are intended to demonstrate how these issues can be formulated in a perspicuous manner and to suggest fruitful avenues for further thinking.

In historiography as in other domains of epistemic life, skepticism is a good thing that one can nevertheless have too much of. As R. G. Collingwood insisted as part of his broadside against “scissors-and-paste history” (Collingwood 1994), the historian’s attitude to testimony should be skeptical not only in terms of accepting some elements of testimony and rejecting others, but also in being ready to use that testimonial evidence in ways unrelated to the testifiers’ purposes. The same attitude should be evident when dealing with other historical accounts: the historian goes beyond the dichotomy of “accept or reject,” instead deploying innovative explanations that better account for the evidence than does previous historiography.

In today’s debates, the phenomenon of Holocaust denial, already discussed briefly, has become the paradigmatic example of the vice of unreasonable, even wicked skepticism masquerading as the virtue of reasonable skepticism. For nearly two centuries, however, another, less emotionally wrenching case has been the standard one for thinking through the relevant conceptual issues: an 1819 pamphlet by the English logician and theologian Richard Whately casting doubt on the existence of the (then living) Napoleon Bonaparte. After all, Whately pointed out, very few of those who talked so confidently of Napoleon had actually *seen* him. And when one considered as

well the extraordinary feats with which he was credited, and the vested interest of the tax-dependent British government in promoting the idea of him . . . The rationale for Whatley's polemic was complex; but the upshot was a devastating demonstration of how consensual knowledge of even the recent past can come to seem fragile when subjected to intense skeptical pressure.

Global skepticism has proved as hard to banish in the epistemology of historiography as in other domains of knowledge. One variety of skeptical argument maintains that since historiographic knowledge is of something necessarily absent, genuine knowledge of history is impossible. One purpose of developing inferential accounts of historiographical evidence, in particular Bayesianism and explanationism, is to lessen the grip of that concern. Other varieties of global historiographical skepticism have proved more stimulating, one such being the claim that intuitively bizarre hypotheses are as justified by the evidence as are the hypotheses we usually take to be true. This variant of Cartesian skepticism finds expression in Bertrand Russell's "five minute hypothesis" (Russell 2004: 160). One could logically maintain the hypothesis that the world came into existence five minutes ago, complete with "fossils" under the ground, gray hairs on the "aged," and "memories" seeming to point to the distant past. Bayesianism by itself serves as an inadequate tool to rebut the five minute hypothesis: given that that hypothesis makes the evidence just as likely as our usual hypotheses about the past, any difference will depend upon subjective evaluation of the respective priors or the addition of other epistemic virtues to likelihood. But explanationism may fare somewhat better. For while both the bizarre hypothesis and our usual hypotheses can offer some sort of an explanation for present evidence, there is good reason to suppose that our usual hypotheses offer a *better* explanation. Not only is the five minute hypothesis itself unexplained, but it offers only an ad hoc accuracy. In short, the bizarre hypothesis is logically consistent with the evidence, but does not best explain the evidence.

Applications: Underdetermination

Whether or not the evidence determines only one correct historiography is a central question in contemporary philosophy of historiography, just as the corresponding question has for some time been central in the philosophy of science. A benefit of the Bayesian model is that it permits clear specification of the different varieties of underdetermination that historiographic reasoning may suffer. What Bayesians call the "problem of the priors" – the difficulty of assessing the probability of hypotheses in advance of the relevant evidence being consulted – suggests one way that historiographic hypotheses might be underdetermined by the evidence. The historian's background provides a second potential type of underdetermination. Underdeterminationist critique could emphasize the role of a historian's culturally specific "world view" or "conceptual framework" in that historian's interpretations of the evidence. The clearest and best known analysis along these lines can be found in the work of Hayden White (1973), who emphasizes the underdetermination by evidence of historians' rhetorical strategies, explanatory strategies and modes of "emplotment."

Concerns about underdetermination also cluster around likelihood judgments. We have argued that such judgments typically rest upon explanatory considerations. But to argue for this view is not to argue that those judgments are *determined* by explanatory considerations. Indeed, in some cases there seem to be interesting and persuasive reasons to suppose the opposite – reasons stemming from the application of background knowledge about historical regularities to the explanatory assessment of evidential likelihood. Three considerations in particular deserve mention here. First, historiographic regularities are typically vague in application to particular cases, so that concerns about whether the case under examination instances the regularity – and so whether the regularity is explanatory – are ever present. Second, historiographic regularities can conflict with one another, in ways that can be resolved only on a case-by-case basis (a memoir might be self-serving, and yet the memoirist was truthful in similar past cases). Finally, there may be no regularities available in this case, so that irreducibly case-specific judgment must suffice. Even a complete and correct understanding of the rational historian’s way with evidence will not preclude there being many cases for which an accurate assessment is that “there is no substitute for the brewmaster’s nose, adapted to the art of producing historical brew” (Martin 1989: 105).

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Causation in Historiography

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Historiography is made of myriad causal links, connecting descriptions of historical events or processes. Arguably, causal links are one of the “glues” that keep historiographic narratives together. Philosophers have been asking a number of questions about causation in general: Metaphysicians have inquired what causation is. Do causes exist objectively in the world, or are they just a category our mind uses to order sensory inputs, in which case causation is subjective. Causation could even be a culturally obsolete anthropomorphic projection of human agency on the world to be eliminated by the scientific description of the world as structured of symmetries. Epistemologists have attempted to find out how we gain and justify our beliefs about and knowledge of causal relations. Philosophies of the special sciences such as physics, biology, or historiography have examined these metaphysical and epistemic questions in the particular domains and contexts of these sciences, against the background of philosophic discussions of causation in general.

It is possible to distinguish two basic approaches to causation in the philosophy of historiography: *Unificationist* approaches advocate a universal philosophical analysis of causation; a cause is a cause whether in history or in nature, historiography, or science. Causal assertions follow the same logical and semantic rules in historiography, physics, or everyday life. Historians bring revolutions and collusions as causes, while physicists use revulsions and collisions, but on an abstract level, their philosophical analysis may be quite similar. Unificationist positions in the philosophy of historiography reflect alternative universal theories of causation such as regularity, conditional, counterfactual, probabilistic, and process theories.

Alternatively, *exceptionalists* argue for the *exceptional* nature of causation in historiography in comparison with other sciences. An extreme version of exceptionalism is *eliminativism*, the normative claim for eliminating all causal statements from historiography. Some philosophers went as far as claiming that the special properties of action, which they favor as the subject matters of historiography, such as free will and intentionality, preclude the possibility of making causal claims in historiography. The alleged unique capacity of historians to empathize with their subject matters, other intentional beings who lived in the past, understand them from “within,” allows “understanding,” or “reenactment” to fulfill the heuristic or explanatory functions that causation fulfills in science or everyday life. Other epistemic *exceptionalist* arguments

suggest that it is impossible to justify causal claims in historiography as in other sciences. Historical events may be unique and so historiographic statements about causal relations in history are singular causal statements that cannot be supported by general rules or laws.

Exceptionalist arguments rarely isolate historiography from all the other sciences as a science entirely unto itself. Rather, they divide the sciences into several realms. Most commonly, they group historiography together with other “human” sciences like anthropology or non-behaviorist psychology that share a human or intentional subject matter. Others group historiography with geology as sciences of unique events. Some philosophers consider the complexity of history to be the deciding factor and group historiography with sciences of complex systems, in “the open air,” outside of the laboratory, such as meteorology.

The quality of the debate between advocates of unificationism and exceptionalism has sometimes been constrained. Some philosophers who have been making general claims about the metaphysics and epistemology of causes have had little or no interest in gaining knowledge of historiography, how historians use causes and justify their assignment. Presumably, these authors considered examining historiography redundant because it would either confirm the unificationist account, or be a bad example. Conversely, some of those who argue for the special nature of causation in historiography have not quite kept up to date with unificationist theories of causation, and so argue against versions of unificationism that are either obsolete or caricatures of actual theories. Further difficulties in the philosophical literature on causation, especially in the philosophy of historiography, follow confusions between the analyses of historiographic explanation and causation. Some historiographic causes explain and some explanations are causal, but some historiographic explanations, e.g., colligation, are not causal, and some historiographic causes, e.g., trivial causes such as the existence of the human race or breathable atmosphere, do not explain. Nevertheless, discussions of causation and explanation sometimes overflow into each other, treating all explanations as causal, and all causes as explanatory.

There are two alternatives to the unificationist and exceptionalist approaches: *Primitivism* argues that causation is a primitive concept that cannot be reduced or analyzed. The *evidentialist* approach rejects the metaphysics of causation or its conceptual analysis as philosophically insignificant, favoring the epistemic question of justification of causal propositions through their relations with their evidence.

Unificationist Accounts of Causation

Regularity accounts

The eighteenth-century philosopher and historian, David Hume, claimed that the relationship of cause to effect is that of *constant conjunction*: causes always precede their effects, they are contiguous with them, and the same type of effect always follows the same type of cause. This tripartite definition has been criticized because some causes are contemporaneous with their effects and non-mechanistic science, such as Newton’s, allows action at a distance, without contiguity. Historiography has been most

relevant for doubting the constant conjunction or *regularity* clause. Simple cause-effect regularities are rare in history. Often, the historiographic sciences use singular causal propositions that connect unique descriptions of events, and attempt to describe complex systems where multiple single causes are involved in bringing about an outcome (Tucker 1998). It is one thing to say that a ball broke the window, missiles thrown with sufficient force usually break fragile materials; quite another to say that Mikhail Gorbachev caused the collapse of the Soviet Empire as there has been only one Gorbachev and the Soviet Empire fell only once.

As a historian, Hume avoided the problem of historical uniqueness by assuming a universal human nature that generates the required regularities in history. Hume thought that the same set of motives and emotions, greed, lust for power, envy, and so on, have been causing all historical events: the same set of causes affected Greek and Roman as well as English and French histories. More recent historians have found the reduction of all historiographic causation to this kind of universal folk psychology unsatisfactory. Human motivations have evolved historically. Historiography often describes events as unique configurations of properties that cause other unique events.

A particularly rigorous constant conjunction or regularity is a scientific law. When following Hempel, logical positivists reduced explanation to cause, effect, and a law that connects them, the philosophy of historiography was again useful in casting doubt on the relation between causation and regularity as the alleged yet elusive laws of history.

Conditional Theories

An alternative theory of causation has advocated its reduction to conditional statements, indicative or subjunctive (counterfactual). The original positivist dream was simply to reduce all causal statements to statements of sufficient and necessary conditions. Conditional statements can be translated into formal symbolic logic and then benefit from its clarity and established rules. Still, it became evident almost immediately that a simple reduction to sufficient and necessary conditions is too narrow and too wide: Too narrow because most causes historians mention are neither sufficient nor necessary, and too wide because some sufficient and necessary conditions are not causes. The quest then was for a sophisticated reduction of causation to conditionals that would fit all uses of causation. The philosophy of historiography joined this search, especially during the third quarter of the twentieth century. Philosophers of historiography like William Dray have conducted philosophical case studies of historiographic causal explanations of events such as the American Civil War and the Second World War. These case studies were particularly influenced by Collingwood's analysis of causation (1998) and Hart and Honoré's (1985) study of causation in the law.

When people, including historians, propose causes, they often assume background conditions, and ask which causes were added to them to produce the effect. For example, when historians ask what caused Gutenberg's invention of printing, they may assume as background conditions the conditions in contemporary fifteenth-century Italy, or the conditions in Germany before Gutenberg's invention during the fourteenth century. They ask then, respectively, which conditions were present in Germany, but not Italy, and caused the invention of printing and which conditions appeared for the first

time during the fifteenth century and were relevant for the invention of printing. Obviously, the invention of printing will have different causes according to different background conditions. A related though separate issue has been the analysis of causal significance: What do historians mean when they claim that one cause was more important than another, or when they compare the significance of several causes. One approach simply reused the above analysis of conditions vs. causes to distinguish among causes between less important causes that are conditions given some comparison situation and those that are outside of it and are therefore more important. For example if we ask why did the Berlin Wall fall *in 1989*, rather than prior to that year, the slow decline and growing inefficiency of the command economies of the Soviet bloc are less important causes than the particular actions and inactions of the Soviet leadership in 1989. The question philosophers pursued next was whether the distinction between causes and conditions depends on pragmatic contexts of inquiry or on some universal criteria.

Collingwood (1998) suggested that the concept of causation is anthropomorphic, related to practical human concerns and interests and as varied as they are. In everyday life, we choose as causes those conditions that are humanly *manipulable*. Obviously, manipulability is hardly relevant for historiography, unless it is attached to the perspective of the historical agents themselves, what they could have done differently. Collingwood's other criterion, which Dray expounded on (1989), was *responsibility* or *culpability*. Similarly, Hart and Honoré (1985) demonstrated that the choice of cause as distinct of conditions in legal contexts is influenced by two universal criteria: Cause is either abnormal in a given context, acknowledging that what is considered normal may differ from one social context to another. Second, human voluntary agency is preferred as cause to other conditions, for example the cause of the forest fire is the discarded cigarette rather than the wind. Put together, in legal contexts, the cause is abnormal voluntary act that causes abnormal result.

Though conditional dependency may be an objective relation in the world, the choice of which conditionals we consider causes (or as some philosophers of historiography put it "the cause") is value laden and dependent on human interest. William Dray (1980, 1989) developed Hart and Honoré's (1985) theory of causation to argue that historians typically use value laden normative judgments to consider whose behavior was abnormal, condemnable, and therefore responsible for historical events such as wars. Dray demonstrated using historiographic case studies that historians distinguished differently conditions from causes of identical historical events according to their normative, politically motivated, values.

The interpretation of causation by means of conditional statements reached its most sophisticated version in the work of Mackie, who famously defined causes as *Insufficient but Non-redundant parts of Unnecessary but Sufficient conditions of an outcome* (INUS for short) (Mackie 1974). A cause then is insufficient on its own to bring about the effect, but it is indispensable for the set of conditions that suffice to produce the outcome. This allows us to say, for example, that the economic depression of the 1930s caused Nazism without saying that economic depressions invariably cause totalitarian regimes as the regularity account of causation would affirm. Instead, it is a part of the set of conditions that sufficed for the rise of Nazism, and a part that could not have been replaced by another condition.

The history of Mackie's ideas about causation is indicative of the direction of the discussion of causation as conditionality. In 1965, Mackie (1965) argued that INUS is better than regularity theories in fitting singular causal statements without assuming covering laws. Mackie then applied this theory to the debate about the nature of explanation in historiography. Conversely, many of the contributors to Dray's 1966 anthology on analytic philosophy of historiography discussed the conditional account of causation. However, by 1974, when Mackie published his *magnum opus* on causation, there was no attempt to apply the theory to solve problems in the philosophy of historiography. Conversely, there have been only faint echoes of later developments in the philosophical analysis of causation in the philosophy of historiography. To use a genetic metaphor, the populations of philosophers of historiography and philosophers of causation drifted apart during the 1970s and ceased interbreeding.

The standard objection to the INUS analysis of causation was that it relies implicitly on unjustifiable vague regularities, involving sets of conditions, and so is not a real alternative to regularity accounts. Other counterexamples involved the kind of complex causality that seem to pervade history, yet cannot be disentangled by relying on this concept of causation. Epistemically, if we accept that INUS captures the metaphysical essence of causation, we are left with the absurd conclusion that most people including historians assign causes to events without being able to justify this assignment.

Later debates considered whether causation assumes, at least implicitly, certain background conditions. Davidson believed that causation is exclusively a relationship between cause and effect. Van Fraassen (1980) like Hart and Honoré and Dray proposed that a causal relation asserts not just that the cause caused the effect, but also a comparison class of conditions that did not *cause* the effect. Hitchcock (1996) argued that causal statement includes in addition to causes and effects also a class of *effects* that did not happen. For example, when we say that Gutenberg caused the invention of printing in Europe we also assume implicitly that it was not invented in the Middle East. Schaffer (2005) synthesized all the above to claim that causal statements have four components with comparison classes for both causes and effects. This debate, however, does not quite distinguish explanation from causation. The distinction between causes and conditions may be between causes that explain in a given context and causes that do not, and so the debate about causation is actually about explanation.

Counterfactuals

David Lewis' (1973) theory of causation reduces it to counterfactual dependence: if the distinct cause event was, or had been, the case, the distinct effect event would be, or would have been, the case, and vice-versa, had the cause event not been the case, nor would have the effect event. Later (1986), Lewis introduced a variation on his theory to accommodate "chancy," probabilistic, causation: the effect depends on the cause in the sense that had the cause not occurred, the chance of the occurrence of the effect would have been far lower than actually was the case.

The first problem with these reductions of deterministic and probabilistic causation to counterfactuals is in discovering the truth conditions for the counterfactuals, and how they can be satisfied. Lewis suggested that when alternative counterfactuals

compete, we choose the ones that are more similar to the actual world, or in the context of philosophy of historiography, to actual history. Lewis' notion of similarity, as many of his critics noted, is quite vague. Competing counterfactuals may claim greater similarity to the actual world in some fashion or another; since what we consider similar depends very much on contexts and pragmatic considerations.

Though historians have not demonstrated much footnoted awareness of Lewis' theory, they have been debating whether they use or could use counterfactuals in their work and how they could possibly be confirmed. Mainstream historians often believe that they study what actually happened, not what could have, may have, or would have happened. The minority of historians who use counterfactuals explicitly support them by considering empirical evidence. For example, in considering the effects of individuals like Hitler or Stalin on history, historians consider the evidence for the policies of alternative leaders who would have filled their place: How different would Germany under Göring or Himmler, and Russia under Bukharin or Trotsky have been to actual history. The evidential aspect of counterfactual justification is broader than Lewis' account. Still, there seems to be a gap between the ability of historians to justify their causal assertions, and their ability to justify counterfactuals, in favor of the first. This gap may well indicate that the first is epistemically irreducible to the second. Further, particularly problematic from the perspective of philosophy of historiography is Lewis' disregard for the background conditions of causes. The complexity of most historical situations and the multiplicity of causes and conditions are major problems for the indicative conditional reduction of causation, it may well be even more so for the subjunctive conditional reduction.

Causation as a Process

The process theory of causation, proposed in the philosophy of science by Wesley Salmon (1984, 1998), in the philosophy of the social sciences by Jon Elster (1989) and in the philosophy of historiography by Maurice Mandelbaum (1977) considers causes and effects to be parts of a continuous process. Processes are objective. Their continuity explains discrete relations between causes and effects without having to assume unknown covering laws, conditionals, counterfactuals, or mysterious causal powers to link cause with effect. A process is not made up of causal chains, quite the opposite; causes and effects are abstracted from objective conceptually and epistemically prior processes. Mandelbaum took historical processes to be the prime objects of historiographic research. Much of Salmon's efforts and the debates that followed concentrated on distinguishing genuine processes, such as the Industrial Revolution, from pseudo-processes such as the "wave" of democratization during the 1980s in Latin America and Eastern Europe. Salmon suggested that genuine processes involve the transmission of a *mark* along the process. However, Salmon's characterization of the "mark" that should be transmitted in processes remained excessively vague and open to conflicting interpretations. One possible interpretation of a process is as the development of an object in time, a "worm" in space-time. The problem then for historians is that there is often insufficient evidence for all the phases in the development of some historical objects. Historians must infer some missing links. This is not a problem as far as the epistemic

underpinnings of historiographic practice are concerned, but then historians must connect causes with effects without being able to rely on prior knowledge of a process. The process is then made up of chains of causes and effects rather than the other way around. Further, historians like social scientists, infer causal links sometimes on the basis of probabilistic correlations. For example, in discovering a strong association between a certain distinct voting pattern and a certain sub-section of the general population, they conclude that the second caused the first without understanding the process that connected the two. Such causal inferences again are at odds with the process theory of causation.

Probability

Probabilities are particularly useful for precise descriptions of systems that are complex or for which there is scarcity of evidence and consequently of which there is a degree of ignorance. In the context of probabilistic regularities or conditionals including counterfactual ones, causes increase *ceteris paribus* the likelihoods of their effects.

Science, and accordingly the philosophy of science, is interested mostly in *types* of causes. For example, smoking in general, not merely this one puff, increases eightfold the probability of contracting lung cancer. Prospectively, we can apply the probability rules that deal with types to their tokens, we can tell that habitual smokers increase their chances of contracting lung cancer eightfold. But historiography is interested exclusively in token events that had a definite time and space, it is interested exclusively in sequences of causes and effects that occurred in the past. After the fact, we know that heavy smoking did not cause Winston Churchill to contract lung cancer. We know that Humphrey Bogart died of lung cancer, and so can assess that the hypothesis that Bogart's death was caused by his smoking has an 89 percent probability. Ex-post-facto, we also know that Nietzsche's syphilis caused his dementia, though prospectively syphilis is a necessary but insufficient cause of dementia with a certain probability attached to the development of dementia as a result of syphilis.

The question about the likelihood of a type of effect given a type of cause is of little relevance in historiography because we usually know if the effect took place, and so its probability is usually 1. Historians are interested in the likelihoods of the evidence given alternative causal hypotheses. For example, the likelihood of the evidence we have for Churchill's health at the time of his death is zero given that his smoking caused him to develop lung cancer. The likelihood of the evidence we have about Nietzsche's health at the last years of his life has a likelihood of one given the causal hypothesis that Nietzsche's syphilis caused his dementia. The evidence about Humphrey Bogart's state of health at the end of his life has an approximately 0.89 likelihood given that Humphrey Bogart's smoking caused his cancer; there is an 11 percent chance that his cancer had other causes.

Shifting the focus of philosophic research about causation from conceptual or metaphysical investigation of the universal properties of causes to the probabilistic relation between causal hypothesis about history and the evidence can do more than model how historians justify causal statements, it can also clear confusions in the general analysis of causation. For example, cases of prospective over-determination and

preemption have received much attention recently as challenges to probabilistic theories of causation (Collins et al. 2004; Dowe and Noordhof 2004). Such problems involve and emanate from mixing prospective and retrospective approaches to causation while ignoring ex-post-facto justification of causal hypotheses by the evidence. Unfortunately, philosophers of historiography hardly reacted to probabilistic approaches to causation; alas, too many of them cannot understand elementary probability theory, while philosophers who specialize in probabilistic interpretations of causation have hardly attempted to apply their models to historiography; alas also, too many of them do not understand historiography or consider its philosophy. Yet, synergies between the philosophy of historiography and the probabilistic interpretation of causation are likely to be fruitful.

Exceptionalism

If the justification of historiographic causal claims rests on the corroboration of historiographic claims about historical regularities, or conditional dependencies, or counterfactuals, historians may not be warranted in making any causal claims, because they may not possess knowledge of historical regularities, conditional relations or counterfactuals. This is the exceptionalist case.

The classical exceptionalist position in the philosophy of history was that history is a realm of backward causation: historical events are caused by their ends, purposes, or *telos*. Historical events then may be caused, not by events that precede them or are simultaneous with them, but by events that succeed them. Such backward causation, like causation in general, need not be deterministic and so can allow free will. This view of historical causation was popularized by philosophers like Hegel and historiographic schools like the Whig one, both considering liberty (in different senses) to be the final cause of history. Different philosophies of history proposed inconsistent ends to history without offering evidence that may decide which teleology is better justified. Moreover, the metaphysical barriers to accepting backward causation seem insurmountable.

Some philosophers of historiography, at least since the Neo-Kantians, have argued that one of the distinctive features of historiography as a *human* rather than *natural* science is the type of causes it uses. Since historiography describes largely, or some would even claim exclusively, human actions, historians are interested in motives, reasons, interests, passions, beliefs, and so on. Since historians and their readers, just like the agents of historical action, are humans moved by the same types of causes, this allows an understanding historical causes in a way that natural causes can never be understood, from within or emphatically. The argument is not so much that it is impossible to give reasoned causal explanations in historiography as in the natural sciences; rather, such causal assertion are or would be uninteresting in comparison with the kind of understanding that historians are, or at least should be, after.

For causes of action to be effective, so goes the argument, agents must choose to recognize them as such, as reasons for action. Unlike some of the causes inferred in the natural sciences, such causes are not *necessary* since free willing agents may choose not to act on them. Therefore, causes in historiography are only in the sense of reasons, never deterministic or parts of law-like cause-effect regularities, never sufficient

or even necessary. This argument does not recognize unconscious motives as causes of action: for example, rapid economic change and geographical dislocation may cause people to develop radical political or religious opinions and movements, though they would not put their religious or political conversions in such terms. Since historians often make such causal claims, using unconscious motivations, this version of exceptionalism needs to explain this discrepancy between philosophy and historiographic practice.

Collingwood (1993, 1998), and Oackshott pushed this argument even further to argue that causes have no role at all in historiography to the extent that it deals with human action. Human action and choice, so they argued, are free, rather than *determined* by causes, motives, persuasions and pressures influence them. The association of causation in historiography with determinism, necessary and sufficient conditions as in turn of the twentieth-century positivism eventually led these philosophers to attempt to eliminate causation altogether from historiography.

Eliminativism

Eliminativism recommends doing away with the concept of causation in historiography. Bevir (1999) argued that historians of ideas should explain by adducing reasons instead of causes as in science. Reasons in Bevir's sense show the coherence of the webs of beliefs of individuals and how they develop them in relation to the traditions from which they emerge. Understanding belief, then, is locating it in a coherent net, in relation to traditions. Rational explanations in the historiography of ideas may reveal conditional relations between beliefs, not relations of cause and effect. "Crucially, conditional connections are neither necessary nor arbitrary. It is because they are not necessary that we cannot express them as physical or logical laws; and it is because they are not arbitrary that we can, nonetheless, use them to explain the beliefs they connect to one another" (Bevir 1999: 252). As we have seen above, non-necessary and non-arbitrary conditionality fits well the INUS reduction of causation to conditionality. Bevir's rejection of "causation" in historiography identified causation with necessity or regularity: "Natural scientists explain things using law-like generalizations which postulate physical connections. Historians of ideas explain things primarily in rational terms by highlighting conditional connections" (Bevir 1999: 254). Bevir's eliminativism is in fact a rejection of one approach to causation, the old non-probabilistic regularity theory in favor of another, the conditional one.

Distinguishing human sciences like historiography from natural sciences, especially physics, as non-causal vs. causal, is particularly problematic because contemporary fundamental physics does not mention causes (or natural laws) so extensively but describes natural symmetries with mathematical rigor. The case for descriptive causal eliminativism, the claim that contemporary science does not use or require causes is much stronger for physics than for historiography. Bertrand Russell, for example, considered causation an animist projection of agency onto the natural world. Differential equations do not require causation. By contrast, doing away with causation in historiography would imply rejecting much of existing historiography and the rationality of the practices of most historians.

Historians express opinions that may be interpreted in philosophic terms as eliminativist in two very different contexts: First, in response to echoes of the obsolete positivist interpretation of cause as sufficient and necessary condition, they actually reject positivism, not causation. Second, facing paucity of evidence, some historians despair of ever being able to determine the causes of some events, let alone disentangle them to discover the relations between the causes and the effect and assign them relative degrees of importance. However, this does not imply that in all cases historians cannot and do not attribute causes to events for good evidential reasons.

Primitivism

The apparent difficulties of all the reductive theories of causation, and the arguably question begging reliance of many of the concepts that should do the reducing, such as probability, counterfactual, and process on a preconceived notion of causation, led philosophers to attempt to pull the rug from underneath the project of analyzing causation by designating it a primitive, irreducible, and undefined, fundamental notion. Causation is at the center of our conceptual network, we can neither eliminate it, nor reduce it to something else. The philosophical research program then is to classify types of causes and causal relations rather than analyze the concept of causation itself. In the philosophy of historiography, Miller (1987) attempted to construct a philosophy of historiography and the social sciences following such a primitive notion of causation.

According to primitivism, whether or not causes and causal relations happen in the objective universe, they are part of how we perceive the world and require no further analysis. The philosophic effort to attempt to analyze the concept of causation in its various contexts has been misspent, much like the project of philosophical conceptual analysis in general, the project of analytical philosophy that Quine demolished in his "Two Dogmas of Empiricism." If, after Quine, we accept that the difference between the atom and the Greek gods, is in their relation with the evidence, the project of philosophy of historiography should not be the analysis of historiographic statements that make causal assertions, but the analysis of their relations to the evidence. The relevant philosophic question is not "what are historiographic causal statements?" but "how do historians justify causal statements?" Likewise, questions about the distinction between causes and conditions, and the degrees of importance ascribed to different causes should be resolved by the examination of the relations between such claims and the evidence historians bring to support them. An intrinsic analysis of historiographic texts without considering their evidence can never tell the whole story. Such a reorientation of philosophical analysis of historiographic causation is yet to be developed.

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Historiographic Counterfactuals

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A counterfactual is a subjunctive conditional that presupposes the falsity of its antecedent. A fully explicit historiographic counterfactual would be typically of the form: “If *e* had not occurred, then *f* would not have occurred,” where *e* and *f* stand for events known to have occurred.

Historiography is replete with counterfactuals. For example, when discussing reasons for the decline of the Roman Empire, Cary and Scullard examine various (at least four) counterfactual suggestions to conclude that “the Roman frontiers would never have been seriously imperiled but for the preoccupation of the Roman garrisons in the third century with the game of emperor-making” (Cary and Scullard 1975: 556). H. Pirenne (1956: 27) expresses his famous thesis about the significance of the spread of Islam for the making of medieval Europe with a couple of counterfactuals: “Without Islam, the Frankish Empire would probably never have existed, and Charlemagne, without Mahomet would be inconceivable.”

Notwithstanding the ubiquity of counterfactuals in historiography, traditionally counterfactual thinking has been quite unpopular among historians, at least if their methodological reflections are taken seriously. Thus, A. J. P. Taylor (1976: 9–10) wrote: “Some historians like to play at the game ‘if it happened otherwise . . .’ This only goes to show that they would be better employed writing romantic novels where dreams come true.” And notoriously, according to E. H. Carr (1961: 97), counterfactual thinking is nothing but playing “a parlour game with might-have-beens.”

Recently, however, we have witnessed a few publications, particularly about military and political history, collections of articles dealing with “What If?” questions (see Tetlock and Belkin 1996; Ferguson 1997; Cowley 1999; Cowley 2001; Roberts 2004; Tetlock et al. 2006). Many historians have set aside their reservations and counterfactual historiography seems now to be flourishing. In the 2006 annual meeting of the American Historical Association a round table titled “Not just a ‘Parlour Game’ Anymore: Counterfactual History in the Historical Mainstream” was held.

Some general explanations of the present popularity of counterfactuals may be apposite (Rosenfeld 2005). To begin with, “What If?” discussions seem to attract a wide readership. Second, this kind of historiography seems to be more appropriate at present, as longstanding ideologies are being rejected and a deep sense of “incredulity towards meta-narratives” (to use Lyotard’s expression) prevails. Third, in the spirit of

post-colonialism and the general valorization of the “Other,” attempts are made at resurrecting the voices of the oppressed, the subalterns and losers in history. Finally, the computer revolution has accustomed us to imagine realms that lack the usual restrictions of time, place, and materiality, but nevertheless have some sort of reality. In this vein, counterfactual historiography is called “virtual” (Ferguson 1997).

The longstanding recoil at counterfactual historiography is somewhat understandable in view of the tendency to confuse it with the so-called “*alternate history*” – a branch of science-fiction, where one or more real past events are changed, and the subsequent narrated events are fictional, as in historical novels. The fact that counterfactuals typify both is not a good reason, however, for conflating and confusing them.

The Counterfactual Character of Historiography

I distinguish *historiographic* from *metahistoriographic* counterfactuals. The latter appear in philosophical analyses of historiography as well as in methodological tracts by historians. We are told sometimes that the historian has three tasks: description, explanation, and understanding. Here I shall discuss the use of metahistoriographic counterfactuals in explicating these activities.

Description

Since antiquity historians have had remarkable respect for the artistic ideal, according to which the work of art should imitate reality as accurately as possible in order to create in the reader (or observer) the illusory impression that she experiences reality proper. In *How to Write History* Lucian of Samosata (ca. AD 120–180) compared the good historian to a perfect mirror that reflects past reality so well that the observer may “think thereafter that he is actually seeing what is being described” (Lucian 1959: 64–5). The historian is expected to produce in the reader the counterfactual illusion that she observes past events as an eyewitness would have. Ranke’s famous claim that the historian’s task is to show what actually happened is sometimes interpreted in this manner. To show how the past really was is, one might say, to show how it appeared when it was present. It is possible however, that Lucian spoke with irony. The mimetic illusion is unattainable, even in art, though it cannot be denied that the historian, not unlike the writer of fiction, has in his tool box lots of techniques and devices for producing the impression of resemblance.

Explanation

The covering-law model was an attempt at constructing a theory of explanation for historiography. Following the much discussed logical and epistemological conditions of adequacy for explanation, Hempel insisted on a structural identity of explanation with prediction. He considered the task of explanation to resolve some puzzlement. An occurrence of an unexpected event is typically puzzling. But how can a past event surprise the historian at present? It is therefore tempting here to use a counterfactual: had the historian lived in the age under discussion, then the unexplained event would

have surprised her. Showing why the event would have been expected in the given circumstances removes the puzzlement. “Historical explanation . . . aims at showing that the event in question . . . was to be expected in view of certain antecedent or simultaneous conditions” (Hempel 1965: 235). Even if the explanation-prediction identity thesis is rejected, Hempel’s basic intuition need not be false. Similarly, Marc Bloch (1953: 125) alludes to his own attempts “to transport himself, by a bold exercise of the mind, to the time before the event itself, in order to gauge its chances, as they appeared upon the eve of its realization.”

Understanding

Some philosophers maintain that in historiography the right answers to why-questions involve understanding rather than explanation. To put it differently, explanations of action demand a special kind of understanding (*Verstehen* in German; see ch. 11, this volume). According to Collingwood (1946: 283), the historian “must see for himself, just as if the [agent’s] situation were his own, how such situation might be dealt with.” Collingwood’s re-enactment theory has thus an unmistakable counterfactual flavour.

Counterfactual formulations of this sort are popular among historians. In Butterfield’s (1931: 146) words, “[t]he story cannot be told unless we see the personalities from the inside, feeling with them as an actor might feel the part he is playing.” Behind every explanation of historical action lurks a counterfactual of the form: “If I (the historian) had been the historical agent, then my reasons for performing the action would have been such and such.” On another version (see Apel 1980), the form is: “If the historical agent had been asked “What are you doing?” (or “Why are you doing it?”), then such and such would have been her answer.” In this vein, perhaps, Gardiner (1952: 136) considers the agent’s real reasons to be those she “would be prepared to give.”

The Hempelian and the Collingwoodian have been frequently considered as offering alternative approaches to the philosophy of historiography. But both raise counterfactual questions, both involve some sort of “return” to the past, and both suggest a close parallel between “looking backward” and “looking forward.” They actually have much in common (Weinryb 1989).

Metaphysical Preliminaries

The discussion of historiographic counterfactuals should be guided by general metaphysical considerations. Here I refer almost exclusively to the work of David Lewis, whose contribution in this area is most significant (for other contributions see Goodman 1954; Bennett 2003).

In Lewis’s (1973b: 160–1) words, “[w]e think of a cause as something that makes a difference, and the difference it makes must be a difference from what would have happened without it.” He then explicates causation as follows (*e* and *f* stand for events):

e causes f if, and only if, e and f both occurred, and if e had not occurred, then f would not have occurred.

An analysis in terms of probability is also available, according to which if e had not occurred, then the chance of f 's occurrence would have been much lower than in fact it was. Causation is defined then in terms of *counterfactual dependence* between distinct events.

But how can we determine the truth value of counterfactuals? According to Lewis's (1973a) theory, a counterfactual is true when its consequent is true in the closest possible world to the actual world where the antecedent is true. A world is more or less close to another to the extent that it resembles that world in matters of laws and particular facts. For instance, the glass fell and shattered. Had it not fallen, it would not have shattered. A possible world where the glass does not fall and does not shatter is closer (more similar in terms of the laws of physics and particular circumstances) to our world than a possible world where the glass does not fall but shatters. The relevant similarity relation requires that the nearest possible world in question agrees with the actual one throughout the past until shortly before the time of the occurrence of the cause. We imagine a small "miracle" that cancels the cause, and see what follows according to the laws of nature.

Counterfactuals of this kind are the product of thought experiments. In the experimental sciences, eliminating a cause is in many cases feasible and can be reproduced in a laboratory. In other sciences, including geology, cosmology, and historiography, experiments are impossible, and thought experiments in the form of counterfactuals may be necessary.

The analysis of causation in terms of counterfactual dependence seems plausible. But there are inherent difficulties, as Lewis himself was quite ready to admit (see Collins et al. 2004). The difficulties arise from discrepancies between counterfactual dependence and some features we find intuitively essential for causal ascription. Here are some prominent difficulties.

- 1 *Causal asymmetry*: Effects do not cause their causes, but counterfactual analysis makes causation symmetrical. If the nearest world where the cause does not occur is a world where the effect does not occur, why is the nearest world where the effect does not occur not a world where the cause does not occur? To avoid this, the condition that causes must temporally precede their effects is added. This should rule out backward causation.
- 2 *Transitivity*: Causation is generally held to be a transitive relation, which means that if e is a cause of f , and f is a cause of g , then e is a cause of g . Counterfactual dependence, however, lacks such transitivity. To mend this situation, causation is defined in terms of a chain of counterfactual dependences. That is to say, the above-mentioned g depends counterfactually on f , and f depends counterfactually on e ; g might not depend counterfactually on e but still be its effect.
- 3 *Pre-emption*: Apart from symmetrical cases involving over-determination there are asymmetrical cases involving pre-emption by two or more potential causes, only one of which is genuine. A and B, for instance, are hired gunmen whose mission is to assassinate C; B shoots C only if A does not or misses the target; A shoots and

kills C. But even if A had not assassinated C, B would still have. Intuitively, the causal relation holds here without the appropriate counterfactual dependence.

- 4 *Omissions and preventions*: Counterfactual dependence applies to omissions and preventions in the following manner. In the case of omission, *e* did not occur but *f* did, and if *e* had occurred, then *f* would not have occurred. In the case of prevention, *e* did but *f* did not occur, and if *e* had not occurred, then *f* would have occurred. This accords with commonsense, which accepts that failures of events to occur can both cause and be caused. But two kinds of doubt creep into this picture. First, omissions and preventions involve absences. But can an absence be either a cause or an effect of something? Second, as there does not seem to be any particular event that we are describing, there will typically be a vast multitude of ways by which something could, in principle, have been omitted or prevented. Thus, either there are no cases of causation by omission and prevention, or there are far more than commonsense says there are.
- 5 *The cause/condition distinction*: Counterfactual analysis is insensitive to the various context-relative ways by which commonsense distinguishes between causes and background conditions. There are many necessary conditions for the occurrence of an event, but we typically select only one of them as a cause. To put it differently, we sometimes promote one of the causes to the status of *the* cause. Counterfactual analysis, however, cannot help us in this selection, which is guided, perhaps, by normative considerations (see Dray 1964: 56).

In metaphysics these problems are an area of continuing research. Historians, of course, should be on the alert for these difficulties.

Causal Counterfactual Analysis in Historiography

Many years ago Max Weber (1949 [1906]) recommended the use of counterfactuals in historiography. Of Robert William Fogel's book *Railroads and American Economic Growth* (1964) it has been said, however, that it was "certainly the most ambitious attempt at wholesale counterfactualization ever undertaken by a serious historian" (Elster 1978: 204). Fogel was a prominent member of the "New Economic History" school that aspired to found economic historiography on scientific methods. An explicit use of counterfactuals was one of the conspicuous features of the efforts in this direction.

Fogel questioned the accepted wisdom, according to which the existence of railways in the second half of the nineteenth century contributed significantly to the growth of the US economy. The cause (railways) is taken here to be a necessary condition of the effect (growth). As Fogel saw it, the only way to either justify or refute this causal claim is to examine the truth of the appropriate counterfactual. The question then is: is there a significant difference between the historical GNP (gross national product) and the GNP that would have been achieved, had the economy adopted itself as efficiently as possible to a situation without railways? For various reasons, Fogel asks the question for the year 1890 and for the transportation of agricultural products only. In order to calculate the costs of transportation by waggons and waterways he must show how the non-existence of railroads would have changed the map of cultivated land and

waterways. For this he uses commonsense reasoning as well as the available economic theories. In particular, economic theories are necessary for the derivation of the counterfactual consequent (a hypothetical GNP) from a possible world antecedent (US without railroads). Thus, “the natural limit to asking causal questions in history is imposed by the adequacy of the theory describing the alternative world” (Fishlow and Fogel 1971: 15). Fogel’s final conclusion was that the difference between the actual 1890 GNP and the non-railroads GNP is insignificant. The original causal claim is thus proved to be false.

Among historians, this conclusion aroused a serious controversy. Of course, not every sub-field of historiography has the tools available in economic historiography. But we can see, at least in principle, the need for counterfactual thinking in causal contexts.

Fogel’s procedure can be interpreted in two different ways:

- 1 The historian starts with an ascription of causation in the sense of necessary condition assuming as *ceteris paribus* conditions the actual historical context. But when she moves to the counterfactual world she switches to a covering law explanation by means of a sufficient condition. To narrate a sequence of events in the actual world there is no need for a theory. But theory is indispensable when we step into the hypothetical world for which we lack evidence. Thus, the possible world’s description has two contrasting features. It aspires toward fullness and “thickness” on the one hand, but is theory-laden on the other.
- 2 Alternatively, the historian may consider causes as necessary conditions in both actual and possible worlds, so a second possible world has to be postulated, and a third, and so on.

It would be only natural to understand Fogel’s counterfactualization in terms of Lewis’s analysis. That is, “If the American 1890 economy had been without railroads, then the GNP would not have been much smaller than it actually was,” would mean “If you take the actual economy of 1890 and remove from it all features directly linked to railroads, then the GNP would not be much smaller than it actually was.” Elster (1978: 190–2, 207–8; see also Tucker 2004: 227–30) argued that there can be no *ceteris paribus* historiographic counterfactuals. Unless the historian assumes a kind of “miracle,” history would need to be revised to allow the counterfactual precedent in such a way that its conditions would have to be revised as well. Fogel should have asked how US history prior to 1890 had had to be different in order to allow the absence of railroads in 1890. The historian should look for a *branching point* “from which a permitted trajectory leads to a state where both the antecedent and the consequent obtain” (Elster 1978: 191; see also Bennett 2003: 214–21). Fogel should have examined the possible world from ca. 1830, when railroads were actually introduced in the US until 1890; a huge task.

In Lebow’s (2000: 565–7) view, however, both kinds of counterfactuals – plausible world counterfactuals and miracle world counterfactuals, as he calls them – are legitimate. *Miracle world counterfactuals* are appropriate when the question is whether some longstanding factor significantly influences a given system. The so-called miracle functions here as an analytical tool. For example, the counterfactual, attributed to Churchill, about the likelihood of a third world war if the nuclear bomb had not been available (see Gaddis et al. 1999).

In recent “What If?” writings, however, *plausible counterfactuals* are often the rule. The possible should fork from the actual almost unseemingly. This approach is popular in military history, where split-second decisions, missing bullets and unforeseen weather problems may change the course of a battle, even a war. Historical critical turning points coincide with branching points.

Some historians are eager to figure out how things would have been different many years (sometimes 2,500 years) after the branching took place (see Cowley 1999). Such an approach is risky. As Lebow (2000) explains, difficulties in long-term counterfactual thinking result from three sources: the statistical improbability of multi-step counterfactuals, the interconnectedness of events, and the unpredictable effects of second-order counterfactuals. To these, the above mentioned metaphysical difficulties should be added (see Elster 1978: 187–90). History is replete with omissions, preventions, symmetrical over-determination, and pre-emption. Inattention to these nuances is deplorable.

Counterfactuals and Practical Reasoning

In an oft quoted passage, Hugh Trevor-Roper (1982: 365) explains why counterfactuals are indispensable in historiography:

It is only if we place ourselves before the alternatives of the past, as of the present, only if we live for a moment, as the men of the time lived, in its still fluid context and among its still unresolved problems, if we see these problems coming upon us, as well as look back on them after they have gone away, that we can draw useful lessons from history.

Counterfactuals form a part of our understanding of the past because they play an essential role in explanations of action that try to take the historical agents’ point of view. In considering what to do, the agent has to compare the possible outcomes of the alternative courses of action open to her in the given circumstances, in order to choose that course of action that would best serve her aims. An explanation of historical action must therefore show why the historical agent chose the action she did rather than some other alternative. It has to describe the alternative courses of action and show that the chosen action was preferable, from the agent’s point of view, to all other reasonably available options. This may be typically done by a series of counterfactuals, the general form of which is this: “If the agent had chosen to perform *e*, then the foreseeable outcome would have been *f*; if she had chosen to perform *g*, then the foreseeable outcome would have been *h*, etc.” In many cases, however, historians mention only a single alternative to the action performed, and even that only implicitly. Erich Gruen (1974: 494), for instance, reconstructs Caesar’s reasoning before the fateful crossing of the Rubicon as follows: “Caesar could not afford to return to Rome as a *privatus*, for [had he done so] he would be immediately prosecuted and eliminated from political life.” Gruen’s intention is clear – to answer the question “What would have happened, if Caesar had returned to Rome as a private citizen, and not at the head of his army, as he eventually chose to do?”

At least in some cases a counterfactual scenario seems to the historian to be preferable to the actual course of action implemented by the agent. Counterfactuals, in such

cases, serve to evaluate and criticize policies. Ferguson (1997: 279), for instance, claims that “it would have been infinitely preferable if Germany could have achieved its hegemonic position on the continent without two world wars.” His answer to the question “What if Britain had ‘stood aside’ in August 1914?” refers to various discussions within the British government as well as to messages transferred through diplomatic channels. We thus can see how relevant possibilities for contemporaries, formulable in principle in indicative conditionals, become, when observed retrospectively, historiographic counterfactuals. Ferguson (1997: 269) cites, for instance, the British foreign secretary’s proposal to the Germans “not to attack France if France remained neutral in the event of war between Russia and Germany.” In simple cases the translation of indicative conditionals into historiographic counterfactuals is quite direct. But of course, the evidential basis is likely to change, and what seems probable now need not have been so then. But logically speaking, a counterfactual *now* stands or falls with an indicative conditional *back then* (see Edgington 1995: 318).

Various models for explanations of action have been suggested. In view of the role of counterfactual alternatives in such explanations, it is rather surprising to find that almost no such model does them the appropriate justice. A remarkable exception is Rex Martin’s (1977: 158–9) model, which includes, among other things, the condition that the agent “does not prefer any of alternative courses of action – B, C, or D – to doing A.”

Of course, not every possible alternative should be considered. But where are the limits to be drawn? On Ferguson’s (1997: 86) view, historians should consider only those alternatives that can be drawn on the basis of contemporary written evidence. But this restriction seems to be too stringent. Imagining herself to be in the agent’s situation, the historian might consider scenarios not explicitly referred to by the agent. But the hypothesized possible world should lie within the conceptual, epistemic and practical horizons of the agent. Otherwise, the historian would commit the fallacy of anachronism. But there is no simple answer to the question when the historian is showing (anachronistically) how she would have acted in the agent’s situation as opposed to showing (correctly) how the agent would have acted then and there.

The two kinds of historiographic counterfactuals are interrelated. Causal ascriptions have an essential role in practical inferences, as possible courses of action are viewed in the light of probable outcomes. On the other hand, when causation (i.e., counterfactual dependence) is in question, the decisions and the actions of historical agents must be taken into account.

Science and Counterfactuals

Counterfactual thinking can be found in sciences such as geology, paleontology, evolutionary biology and cosmology. Like historiography, they deal with the past on the basis of its present remnants and cannot be experimental, at least in some of their important aspects (for an early comparison, see Carr 1961: 56–62). In the context of such comparisons chaos theory has caught attention in particular. Ferguson (1997) speaks of “Virtual history: towards a ‘chaotic’ theory of the past,” and calls historiography “chaostory” (see also Gaddis 2002: 76–88). This analogy with chaos theory

seems understandable, as at least in its popular version, this theory supplies a series of appealing and easy to be absorbed notions, such as sensitivity to initial conditions, the butterfly effect, complexity, loop processes (or recursivity), strange attractors, punctuated equilibrium (adopted from paleontology), and criticality. The mathematical apparatus of the theory is, however, generally ignored. Short explanations of these concepts and their relationship with historiographic counterfactuals are due here.

The butterfly effect

Sensitivity to initial conditions of a system is exemplified by noting that the fluttering of a butterfly's wings in China can cause a hurricane in Texas a fortnight later. This means that a small, by itself insignificant, hardly noticeable, or even immeasurable event, may result in a big and even catastrophic change at the end of the process. In historiography, the idea was notoriously expressed in Pascal's (1947 [1670]: 51) remark about Cleopatra's nose: "Had it been shorter, the whole aspect of the world would have been altered." "What if?" discussions are captivated by this idea of small factors having big consequences. Thus, by analogy with the butterfly effect history has its "nose effect."

Complexity

Historiography deals with open, multi-variables systems, the initial conditions of which cannot be specified precisely. Moreover, as human beings have consciousness and memory, there are in history plenty of recursive loop processes that can increase chaos.

Hidden order

Observers have found in history nothing but disorder, "only one emergency following upon another as wave follows upon wave" (Fisher 1936: v). But chaos theory provides historians with a kind of consolatory confirmation: the chaotic mess conceals hidden patterns of regularity. Chaos theory also hints why discovery of these patterns is so difficult. Even where there is an orderly recurrence, it is not exact and cannot be easily figured out, since presumably it is created by "strange attractors." These, hopefully, might be discovered in the future (perhaps with the help of better computers).

Criticality

Counterfactual thinking looks for turning points, where all of a sudden history takes a wholly different direction. As chaos theory explains, this might happen in critical conditions, when a small shock starts a sequence of reactions which leads eventually to an avalanche. Gaddis (2002: 84–8) borrows from paleontology the notion of punctuated equilibrium: long periods of stability are "punctuated" by abrupt, destabilizing changes. Historians focus on "points of no return," moments at which an equilibrium that once existed ceases to do so.

In sum, "[w]ithout our having had to do anything different [. . .] we [historians] find ourselves, at least in metaphorical terms, practicing the new sciences of chaos,

complexity and even criticality” (Gaddis 2002: 88). The relevance of chaos theory to history has been, however, disputed. Roth and Ryckman (1995; see also Tucker 2004: 222–6) view all this chatter with a deep suspicion. Since there are no quantifiable models of history where the mathematical apparatus of chaos theory is applicable, the latter might at most enrich historians’ metaphorical language which is anyway rich with metaphors, such as the last straw, the trigger, the gunpowder barrel (but see Reisch 1991; Shermer 1995). All in all, the analogy with chaos theory might remind us how difficult is the historian’s task of making thought experiments involving the isolation and removal of one factor, such as Cleopatra’s nose, to ascertain “What if?”

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Historical Necessity and Contingency

YEMIMA BEN-MENACHEM

Introduction

Historians and non-historians alike are often occupied with questions concerning the evitability or inevitability of certain events: was the war (inflation, urbanization, divorce, medical procedure, etc.) inescapable, or could it have been avoided? And though historians are often admonished to eschew counterfactuals and speculation as to scenarios contrary to the actual course of events, they rarely desist altogether. We invoke possibilities, for instance, when distinguishing between the causes and triggering incidents of historical events, a distinction implying that whereas the event in question would not have occurred had the cause been absent, had the trigger been absent, some other incident would in all likelihood have precipitated the event. Thinking about alternatives to the actual course of events is thus as integral to thinking about history as it is to quotidian discourse. Not only do we in fact have opinions about the necessity (contingency) of certain events and processes, but these opinions are highly significant for decision-making and the evaluation of actions. Examples are plentiful in the personal sphere. John is haunted by the thought that on the day Jane committed suicide, she was upset about his having canceled a planned visit. He would probably not blame himself as much for having canceled if he believed Jane's suicide to have been inevitable. The public/political sphere is equally replete with such assessments of historical contingency and inevitability. Take, for example, the different explanations put forward for the unequal status of men and women in western culture: some consider it a product of natural selection, a natural necessity, so to speak (note the juxtaposition of the notions of "nature" and "necessity" here), others consider it a mere social contingency. This divergence of opinion with regard to the etiology of the gender status gap in turn informs our views on specific gender-related sociopolitical issues. The more contingent we take the inequality to be, the greater our confidence in our ability to correct it.

In addition to this commonplace recourse to notions of necessity and contingency, the role of necessity or contingency in history is often systemically invoked by historians and philosophers of history. Hegel, Marx, and some of the Enlightenment *philosophes*, were, perhaps, the quintessential advocates of historical inevitability, while Nietzsche, Foucault, Berlin, and Rorty have vigorously championed contingency.

The notions of necessity and contingency require careful analysis, for although they figure quite prominently in both academic and everyday discourse, their precise meaning, I will argue in this chapter, is frequently misconstrued. On the received view, necessity is associated with causation, contingency with chance or randomness. By contrast, I want to suggest that contingency and necessity be understood in terms of stability, that is, sensitivity or insensitivity to initial conditions and intervening factors. As we will see, this shift affects our understanding of other notions, such as fate and teleology and has far-reaching implications for the question of the individual's influence on the course of history.

Necessity and Contingency as Degrees of Stability

Some states are more stable than others: a ball inside a (not too large) spherical bowl is in stable equilibrium, whereas a ball atop the same bowl overturned is in a far less stable state. Stability is characterized in terms of the effect of small changes, a stable state being one to which a system returns after having been subjected to a small change. Upon being moved, the stable ball will always reach the bottom of the bowl no matter what the trajectory of its movement inside the bowl, whereas the unstable ball, upon being dislodged from the top of the overturned bowl, will reach very different rest-positions depending on the initial conditions of its motion. Yet both the stable and the unstable ball move in accordance with the same causal (and deterministic!) laws of Newtonian mechanics, indicating that *causality and stability are independent notions* (see figure 9.1).

Before further exploring the relation between these notions, consider some familiar events: a car accident, a meeting, a quarrel, a defeat. One question that can be asked about such events has to do with causality. Was the event in question brought about by a causal process, or was it a random occurrence? Another question has to do with *alternatives* to the actual course of events. Could the accident have been prevented? Would they have met had she not missed her flight? Would they still have quarreled had there been no guests at dinner? Would the battle have been lost had the weather been different, or had the commander managed to get some sleep that night? These questions are not about the events that actually transpired, but are, rather, about sets of

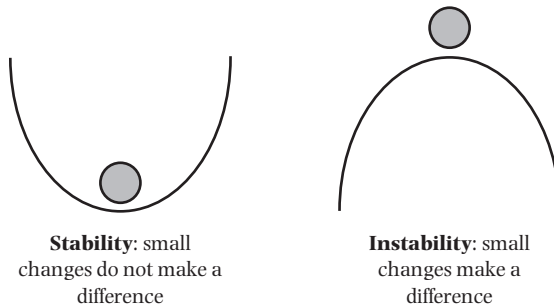


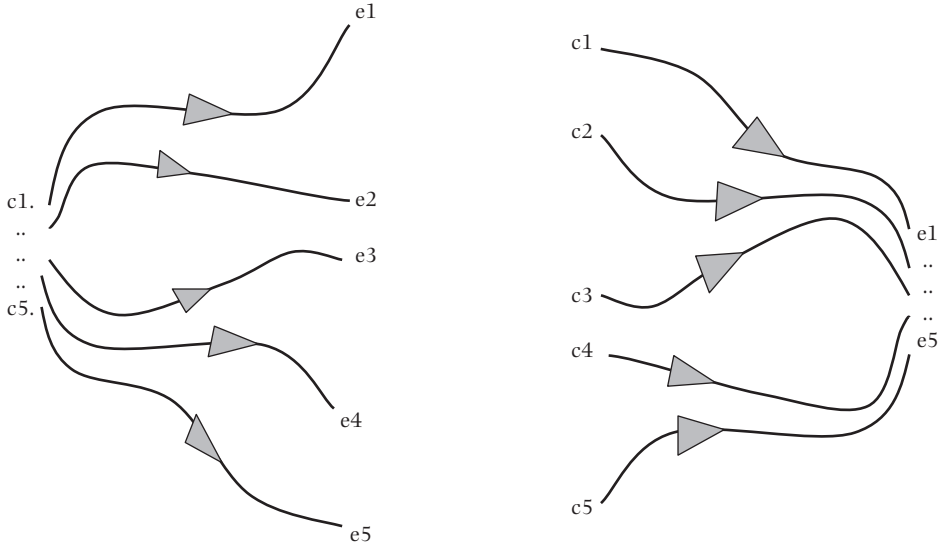
Figure 9.1 Stability and instability

possible events more or less similar to the events that took place. For obviously, the accident, the meeting, the quarrel and the defeat that would have occurred had the initial conditions or intervening factors been different, would not have been the *same* events, but events of a similar – or more or less similar – kind. Even when causes are characterized as *sine qua non* conditions (that is, the effect-event in question would not have occurred had the cause-event not taken place), it does not follow that an event similar to the actual effect-event would not have taken place under different circumstances. The stormy weather was indeed part of the causal chain leading to the actual defeat, but if we consider the defeat to have been necessary, we reckon that the battle would have been lost anyway, regardless of the weather. We must thus clearly distinguish questions regarding causation – what was the cause of a particular effect? – from questions regarding stability – would a small change have made a difference to the end result? Whereas the causal connection is traditionally construed as entailing uniformity – the *same cause* is followed by the *same effect*, in ascertaining whether stability obtains, we determine whether *similar causes* are followed by *similar effects*. The independence of the same cause–same effect question from the similar cause–similar effect question is crucial; an affirmative answer to the former does not imply an affirmative answer to the latter!

Let us reserve the notions of necessity and contingency for questions of the second type, namely, questions about the variation in end results that ensues from a difference in initial conditions or intervening factors. An event will be more contingent the more sensitive it is to initial conditions and intervening factors, and more necessary the less contingent it is, that is, the less sensitive it is to initial conditions and intervening factors. Thus, the defeat is more necessary if a similar defeat would have occurred in the absence of a number of conditions that in fact obtained – the storm, the sleepless night, the tactics chosen by the commander – and more contingent if changing these conditions would have altered the result significantly (see figure 9.2).

Analogous examples of necessity and contingency as degrees of stability are found in the natural sciences. The so-called butterfly effect (a butterfly waving its wings in Hawaii causes a hurricane in Florida) is a well-known example of a process that is extremely sensitive to slight variations in the initial conditions. Furthermore, such examples of deterministic chaos demonstrate that a contingent process need not be random and can be perfectly deterministic. By the same token, a causal chain may be interrupted by a random event without any change to the final outcome in cases where the process is indifferent to such interruption, for instance, when the system is approaching a stable equilibrium. The two distinctions – that between causation and necessity, and that between chance and contingency – must thus be respected. In general, all four modalities – chance, causation, contingency, and necessity – have their place in the analysis of historical processes, but the two pairs cannot be reduced to the others.

This analysis also affords a better understanding of the notion of *fate*, for fatalism seems appealing precisely when we have reason to believe that the “fated” event such as Oedipus’ murdering his father would have occurred regardless of any intervening events, regardless of any action Oedipus might have taken. Fatalism is frequently identified with determinism, but if I am correct in distinguishing necessity from causation, this is a category mistake. Fatalism, like necessity, is not implied by



Contingency: similar causes lead to different types of effects, manifesting high sensitivity to initial conditions.

Necessity: different types of causes lead to similar effects, manifesting low sensitivity to initial conditions.

Figure 9.2 Contingency vs. necessity

determinism. The determinist is entitled to believe that *if* he carelessly raises his head, he will be hit by a bullet, whereas if he protects himself he will be safe. The fatalist, on the other hand, typically holds that a bullet will hit him (or that he will be safe) in any event, regardless of what he does. In other words, rational determinists take steps to avoid predictable harm from a causal process already in motion, whereas fatalists do not, inasmuch as they maintain that the fated result is too stable to be thwarted by their intervention.

The notions of necessity and contingency as defined here are different from, but not unrelated to, their counterparts in metaphysics and modal logic. In the latter context, a proposition is considered necessary if true in all possible worlds, viz., true regardless of the features of a particular world, or simply, true no matter what; propositions that are not necessary are contingent. The historian has little use for this distinction – the truths she is interested in are clearly not true in all possible worlds, that is, they are all contingent in the logical sense of the term. And yet, it is quite common to refer to historical events as necessary or inevitable. Relaxing the notions of necessity and contingency by construing them not as binary, but as a matter of degree, allows us to make significant distinctions within the range of events that from the logical point of view are all indiscriminately classified as contingent. According to the above definition, an event will be considered (more or less) necessary, not if it takes place *under all circumstances*, but if it is *relatively* insensitive to small changes in the circumstances under which it takes place. Note that on the standard account of necessity, what one considers in contemplating the modal status of an event is whether *this very event* would

take place in all, some, or no possible worlds. By contrast, I consider such ascriptions of modal status to implicitly refer to *sets of more or less similar events*. This point is crucial, as we are about to see.

Necessity (Contingency) and Description

Since the notions of necessity and contingency assume *sets* of more or less similar events, their application is *inherently sensitive to the descriptions we use in referring to events*. To assess degree of necessity, we need to know whether the same *type* of event would have occurred given a certain change or intervention. Our assessment hinges, therefore, on modes of sorting and individuation, on what we consider a type, or the same type. “The war was necessary” means “a similar kind of war would have occurred in any event,” hence a judgment about the degree of necessity that should be ascribed to an event will depend on how broadly or narrowly we construe the type in question. Knowing early twentieth-century European history, we may believe a war would have started sooner or later. But if the historian uses finer distinctions – a war in 1914, a war triggered by an assassination, she might lower the level of necessity she ascribes to the war, attributing increased significance to the assassination in Sarajevo.

This develops a point made in Davidson’s celebrated “Causal Relations.” Davidson distinguishes between causal and explanatory contexts: whereas the truth of singular causal statements is independent of the description of the events in question, explanatory contexts, much like other intentional contexts, are description-sensitive. This sensitivity is due to the fact that explanations contain laws that connect *types* of events rather than individual events. To explain a particular event we must, therefore, refer to it by means of the right description, namely, the description matching the type specified in the relevant law. Similarly, as the notions of necessity and contingency have to do with types, they too are implicitly description-sensitive.

Description-sensitivity, I should stress, does not render claims of necessity or contingency subjective. Although assessments of stability in both science and historiography refer to categories and types *we* have decided to employ, our claims about these categories and types are objective. There are, of course, natural and unnatural, familiar and unfamiliar, useful and useless, modes of description, and finding the useful ones is not a trivial enterprise, but the discretion we enjoy in choosing a mode of description does not detract from the objectivity of the claims made about the described events.

Assertions of necessity and contingency are *justifiable*. This fact has sometimes been questioned, for at first glance it might seem difficult to come up with evidence justifying assertions pertaining to *alternatives* to the actual course of events rather than actualities. Basically, though, the epistemic problem we face here is not qualitatively different from those we encounter when attempting to justify claims about causal connections. Of course, both causal claims and claims of stability/instability are easier to justify in the physical sciences than in historiography, for the mathematical equations expressing natural laws may also contain information about stability. This is so, for instance, in the case of the aforementioned chaotic processes. But just as historians do not despair of establishing causal connections in history, they need not

despair of establishing necessity and contingency. The claim that the First World War would have broken out even without the assassination of Franz Ferdinand and his wife Sophie could be corroborated by the usual kinds of historical evidence. Documentation of the relevant enmities, alliances, mobilization plans, etc. could be adduced to establish that the outbreak of hostilities was (more or less) inevitable. And so too with regard to establishing contingency: claims to this effect would have to be based on examination of the specific conditions that produce a particular kind of end-result, sanctioning the conclusion that, in all likelihood, this kind of result would have failed to occur in the absence of these conditions. Ultimately, then, claims of historical necessity and contingency need not be more speculative than causal claims.

Cleopatra's Nose and Other Category Mistakes

In the literature, the tendency to identify chance with contingency is as pronounced as the tendency to conflate causation and necessity. E. H. Carr, for example, attacks what he calls "the crux of Cleopatra's nose" (Carr 1961: ch. 4), namely, the idea that chance plays a significant role in history. He maintains, correctly in my view, that Cleopatra's nose, Trotsky's cold, and other such oft-invoked intervening factors provide no support whatever for the claim that history is a random string of events. Neither Antony's falling in love with Cleopatra, Carr argues, nor the results of the subsequent battle of Actium, were random occurrences. And indeed, on the analysis offered here, these events exemplify neither chance, nor a causal void. Illustrating the causal significance of "small" interventions, they reflect causal connections characterized by a high degree of contingency. A more interesting instance of erroneous identification of various causal categories can be found in Isaiah Berlin's seminal "Historical Inevitability." Seeking to create space for human freedom and responsibility, Berlin targeted a whole cluster of philosophies of history, characterizing all of them as committed to historical inevitability. He maintained that the appeal of inevitability is rooted in the desire to emulate the natural sciences so as to endow historiography with the elevated status of these sciences. Berlin identified necessity with causation and lawfulness. He assumed that paradigmatic scientific explanations exemplify necessity. We have seen, however, that not every law-governed process manifests a high degree of necessity; in general, the laws found in scientific theories do not confer on the events they cover the kind of stability we characterized as necessity. One of Berlin's chief concerns is to repudiate teleological conceptions of history such as that put forward by Hegel. But as teleology has been scrupulously expunged from the physical sciences, whatever necessity may be involved in teleology cannot be identical with that typical of the physical sciences. Setting aside for the moment the relevance of the question of human freedom to historical research, it seems to me that Berlin's critique of historical inevitability would have been far more effective had he distinguished between the various causal connections he lumps together.

Having mentioned Berlin's take on teleology, this is a good place to examine the relationship between teleology and necessity. Undoubtedly, a necessary process such as a mechanical system approaching a stable equilibrium is very different from purposeful action. Nevertheless, there is a structural analogy between at least one aspect

of goal-directed action and processes characterized by a high degree of necessity. Goal-directed behavior is typically flexible with respect to the means employed to achieve a desired goal. I usually take the shortest route to the campus, but if the road is jammed I will choose an alternative route. Since rational beings can often achieve the same goal by various means, the outcome of their purposeful actions is less sensitive to intervening factors than, say, instinctive behavior that is not self-adjusting in this way. In terms of structure, then, what our paradigmatic cases of necessity (the inevitable defeat, for example) and the actions of a rational being have in common is that the outcome is relatively impervious to minor changes in the initial conditions and intervening factors. Indeed, the very fact that flexibility and adjustability are familiar to us from the sphere of intentional action enhances our tendency to project this template onto cases where no conscious action seems to be at play. For instance, our fatalist believes, as we saw, that certain results are inevitable regardless of what he does. From here it is just a small step to the personification of fate – not only is the result inevitable, but there is a power that ensures that no matter how hard we try to elude our destiny, it will eventually catch up with us.

The imperviousness to disruption that is shared by necessary processes, goal-directed actions, and what are alleged to be the dictates of fate, may give rise to the misguided injection of a *telos* into processes that exemplify a high degree of necessity but are not demonstrably teleological. Surprisingly, such teleological thinking is still quite common in biology, where the flexibility manifested by a living system in achieving the same kind of effect via different causal processes is known as *canalization*. For example, suppression of phenotypic variation can be achieved by genetic canalization – insensitivity of a trait to mutations, and by environmental canalization – insensitivity of a trait to environmental variation. It may seem as if the trait is a goal the organism is seeking to achieve regardless of genetic and environmental interference. Structurally, then, canalization is so similar to goal-directed action that the temptation to think of an organism (cell, tissue, genome, species) as working toward a goal is understandable. Yet the common structure is misleading: even mechanical systems, we saw, occasionally manifest patterns associated with necessity. Clearly, then, the temptation to resort to teleological thinking whenever such patterns are identified is best resisted. Similar considerations apply to historiography. Though tempting, the ascription of purposefulness to processes involving a high degree of necessity has no justification. Fortunately, contemporary philosophers of history seem considerably less inclined to such ascription than their predecessors, whose views so riled Berlin.

Making a Difference

Should the classic problem of human freedom be a major concern to the philosophy of history? I mentioned that Berlin's attack on historical inevitability is motivated by his commitment to human freedom. On his view, since history is at least to some extent the product of human action, and since human action is free, there is an irreducible element of freedom in history. Although the problem of freedom has always been

one of the most pressing in ethics and metaphysics, I would deny that it is a pressing problem in the philosophy of history. Indeed, evidence marshaled by the historian can hardly be expected to settle the philosophical question of freedom. What the historian must find out is not whether the historical agent acted freely and was thus morally responsible, but whether his or her actions *made a difference* to the course of history.

For example, in 2000, in the wake of a visit to the Temple Mount by Ariel Sharon, the sequence of events that has come to be known as the Second Intifada (Palestinian uprising) was set in motion. Was Sharon responsible for the uprising? When a historian raises this question, the last thing she is likely to be interested in ascertaining is whether Sharon was exercising free will when he went ahead with that notorious visit. Rather, she wants to find out whether Sharon's visit made a difference to the ensuing events. One aspect of making a difference is causality: our historian will inquire into whether there was a causal relation between the visit and the uprising. She will seek to determine whether the sequence of events that in fact unfolded, including, among other things, worshipers throwing rocks at Sharon and his guards, and the latter responding with gunfire, establishes a causal relation between the visit and the uprising. Will this suffice to demonstrate that Sharon's visit made a difference to the history of the uprising? To arrive at *that* conclusion, the historian must exclude the possibility that an uprising would have occurred whether Sharon visited the mosque or not. For it might be the case that an incipient uprising was underway at the time, and that a very similar chain of events, a similar uprising, would have been set in motion around then even had Sharon never entered the mosque. If indeed our historian determines that this was the case by finding relevant evidence such as official Palestinian documents ordering preparation for war prior to the visit, then despite the causal relation between the visit and the actual events, she will conclude that Sharon's visit merely triggered the events that followed, and did not make a real difference to history. Making a difference, I suggest, is directly linked to the degree of necessity or contingency of the events in question. Human action is one intervention among others in the course of history. Whereas highly contingent processes can be radically affected by such intervention, processes involving a high degree of necessity are far less amenable to its impact. Thus, necessity is distinct from determinism, as a predetermined action may make a difference in a contingent system, and a free action may make no difference at all in a highly necessary system. Still, in acknowledging the role played by contingency we make space for human beings to make a difference in history. Philosophers who seek to harness history as a critical tool in the service of political change are thus more likely to direct their efforts at uncovering the contingent facets of history than in pursuit of its necessities.

Emplotment

Thus far I have discussed the notions of necessity and contingency with reference to historical events and processes, but they are just as useful when construed as modes of *narrating* such events, along the lines suggested by Hayden White. According to

White, historiography involves emplotment – organization of the historical material into narrative genres familiar to us from literature. Different configurations of the historical material may endow the same facts with different meanings. Although White does not address the perspectives of necessity and contingency, the literary patterns he identifies can be distinguished from each other by the degree of necessity they ascribe to the events in question. Tragedy is the paradigmatic manifestation of necessity. Writing the story of Napoleon as a tragic hero entails conveying to the reader the sense that rather than being the master of his fate, Napoleon, like Oedipus, was in fact dominated by powers beyond his control. It will seek to convey the sense that despite appearances, the history of the era was not the fruit of Napoleon's plans and actions, but the product of overwhelming historical processes whose degree of necessity made them quite insensitive to human intervention. By contrast, writing the history of Napoleon from the perspective of a different genre, say the epic or the contemporary novel, will communicate a strong sense of contingency, leading to the conclusion that the historical outcome was shaped by even the smallest moves Napoleon made, that European history would have been entirely different had he not acted as he did. No reader of a history written in this mode could see the future as fated, as impervious to individual human endeavors. Greater complexity of presentation can be achieved by juxtaposing several genres, representing the perspectives of different players or observers. For example, the hero may adopt a highly contingent perspective while the narrator undermines that contingency by superimposing on the protagonist's perspective an overall tragic emplotment. The contrast between these perspectives then creates irony, the distance between the knowledge available to the protagonist and that available to the narrator or reader. Traditionally, it was often the divine perspective that was invoked to expose the dimension of necessity behind the apparent contingencies of events – the story of Joseph and his brothers in the book of Genesis is a classic example of this kind. But philosophers of history are just as fond of the complexity and irony created by the superposition of different perspectives, Hegel's "cunning of reason" being a case in point.

Let us review the characteristic features of the notions of necessity and contingency as here defined.

- 1 Unlike the logical notions, historical necessity and contingency are not binary concepts, but span a spectrum of possibilities.
- 2 Necessity and contingency defined in terms of stability are irreducible to the notions of causation and chance.
- 3 The notions of necessity and contingency are inherently sensitive to modes of description and individuation. This sensitivity does not undermine their objectivity and justifiability.
- 4 The structural analogy between necessity and teleology should not mislead us into reading teleology into necessary processes.
- 5 The notions of necessity and contingency provide us with an understanding of historical agency – making a difference – that is unburdened with the metaphysical baggage associated with the notions of freedom and determinism.
- 6 While necessity and contingency are characteristic modalities of historical events, they also typify literary genres employed in historiography.

Prophets of Contingency

Armed with this new understanding of necessity and contingency, let us end by considering its utility beyond the purely academic sphere. In addition to its intellectual and scientific aims, history has long been recognized as pregnant with critical insights. The critique of religion, morality, politics, and even science has benefited from historiographic research that has challenged the received myths and stereotypes associated with these institutions. This has made History a threatening discipline from the vantage point of those who wish to maintain the existing order or preserve the privileged status of a particular body of "truths," and a discipline pulsating with the promise of liberation for those who seek to engage in ideological housecleaning. Historiography as critique has been advocated by Nietzsche and practiced by thinkers as diverse as Spinoza, Marx, Feuerbach, and Foucault. And indeed, a critical effect is frequently generated by offering a change of perspective on the necessity or contingency of historical events, social structures, political arrangements, and so on. Wars, for example, are all too often represented to the public as inevitable, the gloss of necessity serving obvious political ends. Reframing the events leading up to a war as highly contingent, suggesting that the war was escapable after all, will stimulate a more critical attitude to the war in question and perhaps other wars as well. Similarly, the bourgeois lifestyle is frequently lauded by those who embrace it as not merely morally commendable, but also *natural*, appropriate for human beings as such. The history of the emergence of this particular lifestyle may defuse this sense of naturalness, replacing it with a more sober perception that it is but one mode of living among many. In these examples, the change of perspective points in the direction of a greater degree of contingency, but changes of perspective in the opposite direction may also have socio-political implications. Sociobiology, for instance, strives to anchor our moral values and social practices in the evolutionary history of the species. Certain practices, habits of gift giving, say, that are usually considered social constructs, and thus relatively contingent, emerge in sociobiological studies as "evolutionary stable strategies," and bear the attendant imprint of necessity. The controversy over feminism has involved a struggle between the two kinds of perspective, with the friends of necessity typically taking the more conservative stand, and advocates of contingency leading the campaign for change.

Over the generations, both necessity and contingency have been championed by historians and philosophers of history as central to the dynamics of historical change. In the twentieth century, however, the framework of contingency has clearly superseded its alternative. The perspective of contingency has become not only a lever for social critique and political change, but also a manifesto for greater human independence in every realm. Further, eschewing attempts, Kant's in particular, to identify the necessary elements of human thought and reason, the implications of contingency are increasingly appreciated in metaphysics and epistemology. While Richard Rorty is widely considered the foremost proponent of the metaphysical plea for contingency, it is Michel Foucault who has contributed more than any other historian to dissolving the perspective of necessity with regard to categories hitherto regarded as "natural," such as madness and sexuality. Foucault is quite explicit about the role of contingency in

his vision: unlike critique in the Kantian sense of the term, the historical critique he is after

will not deduce from the form of what we are what it is impossible for us to do and to know; but it will separate out, from the contingency that has made us what we are, the possibility of no longer being, doing, or thinking what we are, do, or think. (Foucault 1984: 46)

For Foucault, reframing the past as contingent is an ethical-political mission that enables us to forever change ourselves and forever change the world.

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Explanation in Historiography

GRAHAM MACDONALD AND CYNTHIA MACDONALD

Explanations may have some essential common core, or the notion of explanation may resemble what Wittgenstein called a “family-resemblance” concept, whose members need not share the same set of features, but may be more loosely connected by various similarities. One can also attend to different philosophical aspects of explanations, such as their logical structure, their epistemic import, or their pragmatic virtues. These aspects may apply to particular putative explanations in various contexts; for example, causal explanations in mechanics, teleological explanations in biology, or intentional explanations of actions.

Explanation in historiography concerns past events. Events come in smaller and larger clumps; the killing of a soldier may be part of a battle within a war, defeat in which causes a decline in the fortunes of an empire. Other events, large and small, will also be contributory factors to the decline; for example, the failure of the harvest due to the drought will affect the economy and so render the empire more vulnerable to attack. Different historians will be interested in different aspects of this large-scale event, so it would be unsurprising if their explanations of the facets they find fascinating did not yield explanations with differing styles and structures. Historically, the main cleavage has been between subjective understanding of the meaning of the action from an agent’s perspective and the objective explanation of the action (Wright 1971; Martin 1977: ch. 5).

The underdetermination of historiographic facts by the evidence is another reason for different types of explanation: Fernand Braudel (1981: 47–51), for example, had to exert some considerable effort to establish the pattern of population growth in the world during 1500–1800. Having satisfied himself that there is a general pattern of growth in the eighteenth century, he searched for an explanation. Was it caused, perhaps, by technological advances, particularly in agriculture and health care, these in turn producing a decline in mortality rates? But the phenomenon of demographic change was too general; it included territories where there was no significant technological advance. Perhaps, then, it was caused by more space being available through a process of “inward colonization,” whereby previously unused territory within a country was becoming increasingly inhabited. But this looks more like effect than cause, the population growth causing the territorial expansion, and not the reverse. Braudel eventually settles on one important factor: climate change. This explanation is fleshed

out by connecting climate and harvests and then noting the importance of harvests for a mainly peasant population: “The rhythm, quality and deficiency of harvests ordered all material life” (Braudel 1981: 49).

This type of explanation of a large-scale historical event does not, at least not immediately, raise particular philosophic problems relating to historiographic explanation. The explanation is straightforwardly causal. The hypothesis that variations in the climate produced the fluctuation in population is testable by looking for significant correlations between the two variables; our understanding of the effect is further increased by noting the causal connection between the quality and quantity of a harvest and the climactic conditions under which the harvest was grown. This is an example of what we may call “macro-historical explanation,” and it resembles a pattern of explanation that some philosophers of science and scientists would find familiar, explanation via the subsumption of particular events under causal laws. Arguably, though the causal laws doing the work in the above explanation cannot be formulated with the precision, nor tested with the rigour, of those found in physics and chemistry, it would be niggardly to refuse them explanatory status on that account. They are *ceteris paribus* laws, familiar enough from other contexts such as biology, ecology, economics, and so on.

This model of explanation has been articulated and defended most notably by Hempel and Oppenheim (1953), then applied to explanation in historiography in a number of papers by Hempel (1965). This analysis of explanation initially provided a two-fold classification of scientific explanations. An explanation is legitimate *qua* explanation if and only if it provides the means to deductively derive a description of the event being explained (the explanandum) from a statement of a law and a description of some initial conditions, *or* if the law-statement and description of initial conditions makes the explanandum highly probable. This account clearly makes the presence of a law essential to all scientific explanation and has been dubbed the “nomological” or “covering law” model of explanation.

Amendments to this model might make its application to historiography less controversial, allowing for *elliptical* explanations, *partial* explanations, and *explanation sketches*. An elliptical explanation does not make explicit mention of the laws presumed in the explanation or does so allusively, more or less vaguely. In the example given above, Braudel does not explicitly state any laws connecting climactic conditions to harvest-quality; he simply assumes common knowledge that the causally relevant climactic factors impact on the quantity and quality of certain harvests. For Hempel, though, such a “thin” explanation requires filling out if it is to be a full-fledged explanation. As it stands, he claims, the explanation is “*elliptically formulated*,” omitting “certain laws or particular facts which it tacitly takes for granted, and whose explicit citation would yield a complete deductive-nomological argument” (Hempel 2001a: 282). It is here that the historian might demur, claiming that the explanation is fully satisfactory as it stands.

An elliptical explanation, on Hempel’s account, is elliptical because the *explanans* requires completion; a partial explanation is partial because the *explanandum* is derivable from the *explanans* only if it is described in terms more general than those initially given. This style of explanation works by explaining the occurrence of an action of generic type *P* where the target action is of a more specific type *Q*. For

example, explaining the French Revolution as a case of revolutions in general. An explanation *sketch* is neither specific enough, nor complete enough, to qualify as either partial or elliptical. Such an explanation “may suggest, perhaps quite vividly and persuasively, the general outlines of what, it is hoped, can eventually be supplemented so as to yield a more closely reasoned argument based on explanatory hypotheses which are indicated more fully, and which more readily permit of critical appraisal by reference to empirical evidence” (Hempel 2001a: 284).

Many issues arise from the above typology of explanations. First, in the discussion of partial explanation it is clear that the partiality of an explanation is a function of the description of the event or action being explained. In the example given, if one were to ask: “Why did a revolution happen in France?” then the explanation given would not be partial. It is only because the event has been given a more precise specification that one sees the explanation as being partial. The classification of explanations as partial is context-dependent and conceptually laden.

Second, Hempel admits that even an explanation sketch can be persuasive. This would lead us to inquire into the psychological, some would say pragmatic, features of explanations. If an explanation, even though it fails to meet the criteria of ideal explanation as adumbrated above, leads to enhanced understanding, and those seeking understanding are epistemically content, why does it not qualify as a fully satisfactory explanation? Would it add anything to our understanding to be given a precise law-like formulation of the link between certain climates and crop growths? And does it detract from the explanation that it doesn’t explain the exact percentage of population growth during the relevant period? What is at issue here is whether a causal explanation can be complete in the absence of a law relating the cause(s) to the effect. Pragmatic accounts (e.g., van Fraassen 1980; Achinstein 1983; Smart 1990; Tucker 2004) answer positively and develop models of explanation that do not include laws as necessary components, most notably by separating issues concerning the structure of explanation from issues relating to its justification and validity.

1

The standard objection leveled at Hempel is that he has unjustifiably extrapolated from a highly idealized explication of explanation in one setting, of Newtonian physics, and made this the normative requirement on all explanation. This, however, overlooks the flexibility of Hempel’s analysis of explanation. Consider functional explanation that explains the existence of a feature or activity by citing the effects it brings about. Functional explanations may be:

- 1 *Purposive*: Increasing scale yields economies; managers are aware of that so, desiring economies, increased scale results.
- 2 *Darwinian*: In a competitive environment, increase in scale both yields economies and helps survival. Managers don’t know this, but scale (of some firms) is increased “by chance.” Survival of only those firms with this chance variation produces increase in scale of production.
- 3 *Lamarckian*: Species adapt to their environment; an organ not fully adapted becomes fully adapted via a struggle to use it in its environment.

- 4 *Self-deception*: Here the functional fact operates through the minds of agents without the agents' full awareness.

Take the supposed fact of the ever-increasing productive power of technology. One explanation is straightforwardly causal: knowledge grows, and one aspect of that growth (technical knowhow) effects a transformation in the way a society produces goods. Alternatively, some Marxist historians reversed the order of explanation: they cite changes in the nature of productive forces in explaining social change. The institutions that provide the new knowledge (schools, universities, research departments), and the social patterns allowing for the re-organization of labor, are the way they are or exist with the features they have *because* they facilitate enhanced productive power. They have the *function* of facilitating the optimal use of the available technology, and of adjusting to improved technologies when they become available.

G. A. Cohen invites us to consider the difference between:

- (a) Event f brought about event e .
- (b) Event f preceded event e .

(a), he claims, ventures a causal explanation of e , and (b), though explanatory in some circumstances, is not explanatory in all. It is only in virtue of a true generalization (what Hempel would call a covering law) that both are explanatory.

Likewise, consider:

- (c) The function of x is to \emptyset .
- (d) The beneficial effect of x is to \emptyset .

Again, (c) ventures a functional explanation, and (d), though explanatory in some circumstances, is not explanatory in all. What makes (c) and (d) explanatory (when they are) is a true generalization of a special form, a *consequence law*, which is "a universal conditional statement whose antecedent is a hypothetical causal statement" (Cohen 1979: 259). For example, if it were the case that, if an event of type E were to occur at t , then it would bring about an event of type F at t_2 , then an event of type E occurs at t_3 .

Roughly, the idea is that E's disposition to cause tokens of F explains the occurrence of a token of E. e.g., if E = performance of rain dance of type R; F = rise in social cohesion, then the law says:

Whenever performance of rain dance R would bring about, shortly thereafter, a rise in social cohesion, rain dance R is performed.

The rain dance (its performance) is not explained by the rise in social cohesion (causes cannot post-date their effects); the performance of the rain dance is explained by this dispositional fact.

We are not suggesting that historians must use functional explanation (or explanation by "consequence laws"), though those historians who are interested in the explanation of changing patterns in cultural evolution should be aware of this style of

explanation (see some of the papers in Boyd and Richerson 2005, for interesting material on cultural evolution). Lewens (2007: 50–5; cf. Gat 2007) suggests that all evolutionary explanations that rely on processes of selection employ the type of consequence law discussed by Cohen. Functional explanation accords with the Hempelian model: it is because there are consequence *laws* that consequence style explanation is genuine, even though “we may have a good reason for thinking that a functional explanation is true even when we are at a loss to conjecture by what means or mechanism the functional fact achieves an explanatory role” (Cohen 1978: 266).

Functional explanations (using consequence laws) are partial explanations because they underdetermine the explanandum: alternatives to the rain dance may produce the same result (social cohesion). It may be that in these conditions, given the history of the tribe etc., only a rain dance will be functional in the required way. Otherwise one may retreat to the less specific: if an (unspecified) ceremonial will reinforce group identity, then some such ceremonial will be performed. No explanation explains all the properties of the explanandum. So, the performance of a ritual may be functionally explained, even though which ritual it is may not be. Whether the explanations using this consequence-law style are otherwise satisfactory is another question.

The Hempel–Oppenheim model, suitably modified to deal with some of the objections raised within the philosophy of science literature (see Godfrey-Smith 2003: ch. 13, for a discussion of these objections) may adequately cover macro-historical explanations, despite the unpopularity of this model in the philosophy of historiography for the last few decades. This does not mean that we think that there is a set of necessary and sufficient conditions whose satisfaction would ensure the explanatory status of any set of sentences; there may always be the potential for counter-examples to the analysis. The history of attempts to provide just such a set of necessary and sufficient conditions on knowledge claims support the thought that any analysis of explanation will itself be partial.

Another form of historiographic inquiry, associated with the “explanation” (often termed “interpretation”) of the kind of macro-historical events we have been considering is classification. Was the English Revolution really a revolution? Often that is a matter of debating the correct interpretation of what has occurred, this being conducted alongside increasingly detailed research into the “entrails” of the appropriate period. Sometimes this interpretative process has been construed as essentially synthetic, the integrating into a satisfying whole otherwise disconnected features of the era. Subsumption under a concept, *colligation* (see ch. 12 on colligation, this volume), may be illuminating in the way an explanation ought to be.

Even if the above remarks in defence of generalism are true, there remains a question of the *purport* of the putative explanations. Consider another example, that of the severe consequences of the potato famine in Ireland during 1846–51 (English 2006). The population of Ireland decreased from an estimated 8.3 million in the early 1840s to around 6.4 million in 1851, with about a million emigrating and the rest dying of famine-related diseases. How could the failure of a single crop lead to such devastation? The explanation, unsurprisingly, is multi-factorial: relevant features of the situation were the structure of agriculture, the dependence of the peasantry on a diet of potatoes, the lack of political power of those most affected, the length of the famine,

the failure of the peasantry to include a potential source of food (fish) in their diet, and insufficient help provided by the central (English) government agencies.

It may well be true that there are laws of some sort underwriting the connections between these various factors and the devastation wrought by the protracted famine, but the person seeking an understanding of what happened may not be in the business of trying to assess any such law-like connections. The purpose of this historical inquiry is to shed some light on a *particular* set of events, the presumption being that the relevant factors would be causally influential in the bringing about of those events. We are not claiming; like Graham (1983: 46–51), Elliott Sober, Carol Cleland (ch. 4, this volume), and Tucker (2004) that the interest of historians in explaining events is different from that of theoretical scientists in being directed to the particular token as opposed to the type. Our claim is rather just that this is a legitimate form of historiographic explanation. It is, we contend, the notion of cause that is doing the explanatory work here, not the (background) presence of laws. Knowing the causes implicated in some historical event or process allows us to satisfy what Hempel calls a “necessary condition of adequacy for explanations,” which is that any explanation of X must supply information providing good grounds for believing that X did in fact occur (Hempel 2001b: 299). Lipton (2004) also requires of any explanation that it provide grounds for believing the explanandum occurred. This epistemological requirement will be satisfied if there is a non-contingent connection between causes and their effects. On some Humean conceptions of the causal relation, this non-contingency is secured by the existence of a law “underwriting” the putative relation, and it is, we believe, this conception of causality that induced Hempel to embrace the covering law model of explanation. This leads us to discuss the use of singular causal explanation in the explanation of micro-historical events, particularly the actions of historical agents.

2

Historiographic explanation of particular events has sometimes been categorized as *narrative* explanation. Such explanations are said to set events in a context and to place them within a series of events which can be seen to have a connecting thread. It is the connecting thread that supplies the explanatory illumination: it is because of the relevance of the connections that one sees the explained event as being the plausible (or even inevitable) outcome of what has gone before. The event has been made sense of, and so rendered unsurprising, when such a narrative account is provided. (Alternatively, W. B. Gallie (1964) found emphasis on contingency and unpredictability essential to narrative understanding, but we think that *explanation* must reduce (but not necessarily eliminate) contingency.)

The challenge to the particularist narrativist is to say more about the connecting thread between explanans and explanandum without recourse to law-like generality. Historiographic narration is sometimes explicated along the lines of supplying the plot to a story, the plot being said to collect together the otherwise unrelated events into an intelligible whole (see White 1973, for a typology of plots); or it is rendered, more

controversially, along the lines of an overarching intention which supplies the meaning linking the subsumed events. The difficulty is in being more non-metaphorically explicit about how plot or intention can illuminate historical deeds, as distinct of historiographic texts, in such a way as to make certain outcomes follow “by necessity or probability” (Aristotle, quoted in Velleman 2003: 1). In what follows we suggest ways in which we think this can be done, concentrating on issues relating to explanation while ignoring complications due to the dubious applicability of notions such as “plot” and “story” to historical events, as distinct from their depictions in historiography. We concentrate on the explanations of the actions of historical agents as we think that it is to actions that narrative explanation is most usefully applied. The “making sense” that narrative supplies is seen most plainly in an explanation of a series of events that consist of actions performed in pursuit of a goal; we see *why* some action was performed when we know the motivational and cognitive states of the agent, and the context (social and physical) within which she was acting. These psychological states may be exceedingly complex, consisting of a number of conscious, sub-conscious (sub-doxastic), and unconscious cognitive and non-cognitive factors. Nevertheless, when we have the requisite information concerning the agent’s psychological profile we can see an action as being necessitated by those psychological states, and so are in a position to explain that resulting action.

Reports of significant events are vastly complicated, so explanations attributing only psychological states to historical actors will not be sufficient to account for the way those events turned out. The eventual demise of Napoleon had many causes: the decrease in France’s population, the effect of disease coupled with logistical problems of food supply during the Russian excursion, the ability of England to escape the worst of an economic blockade, the necessity for Russia to trade with England, the obstinacy of his “defeated” enemies together with the overcoming of their mutual distrust of one another, and so on. Nevertheless, one relevant factor is Napoleon’s insatiable appetite for glory, which led him to overstep the bounds of what was militarily and politically feasible (Blanning 2007: 669). This psychological defect explains the initiation of actions that had disastrous consequences.

One task of the historian is to find and supply such psychological information in as plausible a manner as possible, one that takes into account differences in values, upbringing, and social environment between different times and places. Although these differences make the explanatory task that much harder, the resultant explanation is not different in kind from that employed in ordinary accounts of why agents act in the way they do. The question of whether these accounts rely essentially on generalist premises has been much disputed, so we now turn to evaluate that controversy.

In a well-known passage Collingwood says: “For history, the object to be discovered is not the mere event, but the thought expressed in it. To discover that thought is already to understand it. After the historian has ascertained the facts, there is no further process of inquiring into their causes. When he knows what happened, he already knows why it happened” (Collingwood 1956: 214).

Collingwood here gives expression to the view that once one has discerned the thought in the action one has no need to inquire further into the causes of that action. One could put the idea this way: the thought in the action both individuates and

illuminates, providing an understanding of the action that makes further (causal) explanation at least redundant. In Collingwood's case this idea is accompanied by a distrust of the use of laws in historiographic explanation; he thought that there was not enough uniformity in the psychologies of historical agents to justify any appeal to general truths about human nature in the explanation of actions. But this contingent fact (of historical psychological heterogeneity) is not the main reason for Collingwood's rejection of any appeal to law-based explanation; he was convinced that the *logic* of action-explanation was different from that of the explanation of (merely) natural events. (For a sensitive discussion of Collingwood's philosophy of historiography see Martin 1977.)

One way of interpreting Collingwood's distinction between the explanation of natural events and that of actions is to deny what has hitherto been presumed, that actions have causes. If this claim is true it would clearly render causal explanation inadmissible – and at the same time remove one argument supporting the essential appeal to laws in historiographic explanation. This argument has its basis in the influential model of causation first proposed by David Hume, that where there are true causal statements linking cause and effect, there is a law covering those events, relating them as cause and effect. As Hume put it,

There must be a constant union betwixt the cause and effect. 'Tis chiefly this quality, that constitutes the relation. (Hume 1978: 173)

There are two theses of varying strengths that one can associate with the above. The stronger is that causation is nothing but “constant union,” the weaker is that causation requires such constancy to be present. We find the stronger thesis implausible, and so view what constancy there is as being explained by the presence of causation, rather than vice versa. As it happens, the case we are going to present for non-law based historiographic explanation compels us to reject the stronger version of the Humean model, so those committed to it will find our argument wanting. The choice they face is either to accept the generalist account of historiographic explanation or to deny that it works by citing causes.

A related Humean proviso on causally related events is that they be “distinct existences.” That is, the events have to be logically independent of each other, and the law governing them must be an empirical law, which requires that the antecedent and consequent of the law be conceptually independent. It is this requirement that has produced disquiet amongst those wishing to deny that actions have causes: the putative causes (the intentional states of the agent) do not seem to have the required independence. In order to explain actions, the supposed mental causes are required to make it intelligible that somebody with that set of mental attitudes would act in that way, and this intelligibility is achieved by making the action a rational outcome of those attitudes. The “rational connection” between the attitudes and the resulting action conceptually connects the two (so the action of *eating ice-cream* is explained by positing the *desire to eat ice-cream*), and it is precisely this conceptual connection that has been thought to be inconsistent with both the independence characteristic of causes and their effects and the conceptual independence of the antecedent and consequent of the empirical law. The conclusion drawn is that reasons are not causes.

Yet, this conclusion is mistaken, or at least it does not follow from these premises alone. Donald Davidson (Davidson 2001a) points out that the two requirements need to be formulated carefully: events related as cause and effect must be “distinct existences,” and must have *some descriptions* under which they instantiate laws (not that they do under every description). On this understanding, if the attitudes are distinct from the actions, then there could well be descriptions of the two (attitudes and action) that figure in laws, *but not by virtue of their reason-giving descriptions*. That is, the events of my coming to possess a relevant attitude (a belief, say) and of my acting may well fall under laws, but the descriptions that compose the laws need not be descriptions using the vocabulary of “belief,” or of “reason”; there may well be physical descriptions of these events that do figure in laws. If there are such law-apt descriptions employing physical vocabulary, then the events so described can be seen to be independent in the sense required for them to qualify as causally related. So the conceptual connection between reason-giving explanations and what they explain (subsequent actions) is not a ground for denying that the attitudes constituting the (motivating) reason cause the action for which they are a reason.

We take it that there is a good case to be made for regarding reason-giving explanations as citing causes of what they explain, and that this can be reconciled with the Humean requirements on cause–effect relations, those of independence and of subsumption by law. And so we take it that much narrative explanation will cite appropriate causes of the actions taken by historical agents, admitting that some such explanations work by citing facilitating (rather than sufficient) causes, thus showing how an action was possible, though not necessitated (see Dray 2000: 225–7, for a discussion of “how-possible” explanation).

This will only be a victory for the generalist, though, if the reason-giving *explanation* works by essentially averting to the presumed (empirical) law, and nothing in the above supports this view. Davidson claimed that there are no laws in which mental descriptions figure, so not all explanations citing causes need to be seen as conforming to the Hempelian model. He says:

The most primitive explanation of an event gives its cause; more elaborate explanations may tell more of the story, or defend the singular causal claim by producing a relevant law or by giving reasons for believing such exists. But it is an error to think no explanation has been given until a law has been produced. Linked with these errors is the idea that singular causal statements necessarily indicate, by the concepts they employ, the concepts that will occur in the entailed law. Suppose a hurricane, which is reported on page 5 of Tuesday’s *Times*, causes a catastrophe, which is reported on page 13 of Wednesday’s *Tribune*. Should we look for a law relating events of these kinds? It is only slightly less ridiculous to look for a law relating hurricanes and catastrophes. (Davidson 2001a: 17; see also Davidson 2001b, for further elaboration)

The upshot is that narrative explanation can be distinctively particular; it can pay due attention to a particular context in all its complexity, and provide satisfactory explanations of actions arising from those contexts without resorting to fanciful or trumped-up laws. It can do this while remaining faithful to the spirit of the generalist position: all explanation must reduce the sheer contingency of what is explained. It does

this because the factors cited in narrative explanations are causally related (in various ways) to the events they explain.

3

We have argued that historiographic explanation comes in many forms. Sometimes general claims form an essential component, whereas at other times only the particular circumstances and psychological states of the agent(s) need to be mentioned. In the former case the generalizations are likely to be laws only in a loose sense, one allowing for the law to be imprecisely formulated and limited in scope. We have suggested that Hempel's fairly broad characterization of those different types of explanation using generality will cover these cases. We have argued in addition that there is room for what has been called narrative explanation, a form of causal explanation that does not make essential use of laws, even if the causal relation requires there to be a law covering that relation. Singular causal explanation works because there is a cause in play, not because there is a law in the background.

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Historiographic Understanding

GIUSEPPINA D'ORO

Introduction

An important debate in the philosophy of historiography concerns the question of the unity of the sciences and the implications of the claim to methodological unity for the aspiration of historiography to affirm itself as an autonomous discipline, with a method and subject matter that are clearly distinct from those of other sciences. There are two classical *prima facie* reasons as to why historiography should be regarded as autonomous. First, historiography, unlike theoretical science, is concerned with unique events. Thus, for example, the historian, unlike the social scientist, is not concerned with the character of revolutions as such but with what is unique to the French or American revolutions (Tucker 1998). Secondly, historiography is concerned with the actions of human beings. Since human beings are rational, and historiography is concerned with rational processes, it is a science of the mind and the methods appropriate to the investigation of the mind are very different from those which are appropriate to the investigation of natural phenomena. This entry concerns the latter argument against methodological unity. In so far as the debate for and against methodological unity is based on the view that historiography is a science of the mind, it touches upon issues concerning the place of normativity in the natural world and the relationship between truth and meaning, issues which are normally discussed in the context of the philosophy of mind and language, rather than in the philosophy of historiography.

The distinction between the sciences of nature and mind goes back to Kant's distinction between theoretical and practical reason and was appropriated by neo-Kantians such as W. Dilthey, W. Windelband, and H. J. Rickert to develop an argument against methodological unity. In fact, the very terms which are often employed to distinguish between the goals of "explanation" (in natural science) and of "understanding" (in historiography and the social sciences) are translations of the German terms *Erklären* and *Verstehen* and frequently, the German distinction between the *Geisteswissenschaften* and the *Naturwissenschaften*, is used in lieu of the distinction between the sciences of mind and those of nature. I proceed historically, outlining the main lines of argument and debate since the 1940s, culminating with contemporary philosophy of mind that has so far been rarely discussed in the context of debates about understanding in the philosophies of historiography and the social sciences.

The Argument for Methodological Unity

In the mid-twentieth century the debate for and against methodological unity focused on whether the differences between the objects typically studied by the *Naturwissenschaften* and historiography are merely superficial differences that conceal similarities in the method employed by historians and natural scientists, or whether there are genuine disparities in the logical structure of scientific and historiographic explanations, disparities that are strong enough to support an argument for methodological pluralism.

The debate was sparked by the publication of an article by C. Hempel, "The Function of General Laws in History" (Hempel 1942; see also Popper 1935, 1963). In his 1942 paper Hempel claimed that explanations in historiography are subsumptive explanations which share the same logical structure as explanations in natural science. The reason why appear to be different is that historians often omit to make explicit reference to general laws, which are, nonetheless, presupposed by their explanations. Thus, when historians claim that a certain population migrated because weather conditions rendered survival difficult, they presuppose the general law "populations will tend to migrate to regions which offer favorable living conditions." Historiographic explanations are simply syllogisms with a suppressed premise, or as Hempel puts it, mere "explanation sketches." Hempel concludes from this that although historians are concerned with human beings rather than with natural phenomena, they employ essentially the same kind of explanation that is at work in the natural sciences. Even the fact that natural scientists are concerned with predicting future events, whereas historians are concerned exclusively with understanding past ones, makes no difference to the logical structure of their respective explanations.

The argument for methodological unity as articulated by Hempel revived a claim already made by Mill in his *System of Logic* (Mill 1843). As a supporter of the unity of the sciences, Mill denied that there are methodological differences between the sciences which investigate natural events and those which study the feelings, desires, and actions of human beings. Mill claimed that the genuine distinction is not between the sciences of nature and mind, but between the exact and inexact sciences. The sciences of the mind are inexact sciences because the complexity of their subject matter renders precise predictions hard to obtain; the natural sciences tend to be exact sciences, but there are also a number of natural sciences, such as meteorology and tidology, in which strict predictions cannot be attained either. The important distinction is therefore not between the sciences of mind and nature, as supporters of methodological pluralism claim, but between the exact and inexact sciences. For Mill, the classification of the sciences into the sciences of mind and nature cuts across the distinction between exact and inexact sciences and is a classification across categories analogous to the one that we would make in inappropriately distinguishing between diesel cars and red cars, rather than between diesel and petrol cars. In the same way as some, but not all, petrol cars are red, so too some, but not all natural sciences are exact sciences, as the case of meteorology and tidology demonstrate. For Mill all sciences, whether exact or inexact, have a single goal (prediction) and a single method (they subsume the event to be explained under general empirical laws). The reason why inexact sciences such as tidology, meteorology and the human sciences in general are unable to provide

precise predictions merely reflects the complexity of the initial conditions with which they have to deal, not the fact that they employ a different method or that they have a different goal. The distinction between explanations employed in the natural and human sciences is thus a distinction in degree, but not in kind.

The Argument against Methodological Unity

The major complaint of the supporters of methodological autonomy of the historiography against the claim for methodological unity was that the distinction between the methodology of the natural sciences and historiography is, contrary to Mill and Hempel, a distinction in kind rather than merely in degree. The goal of humanistic explanation is not to predict but to make sense and this goal is best served by a different form of explanation, one in which the link between the *explanans* and the *explanandum* is conceptual rather than empirical. One of the key contributors to the argument for methodological pluralism was W. H. Dray (Dray 1957a, 1957b, 1963). Dray was one of the most perceptive interpreters of R. G. Collingwood. He responded to Hempel's argument for methodological unity by interpreting Collingwood's philosophy of historiography (Collingwood 1946) in the idiom of contemporary analytic philosophy, thereby making the latter known to a wider audience. Following Collingwood, Dray argued that explanation in the sciences of the mind differ from explanation in the natural sciences not because the former focus on human behavior rather than natural events (this is indeed a superficial difference) but because they are concerned with the behavior of human beings in so far as it counts as action or as an expression of rational thought. To explain human behavior as an action, rather than a bodily movement, entails rationalizing it. Historiographic explanations are thus "rational explanations" and they differ from explanation in natural science not because they study different things (human beings rather than natural phenomena), but because they study them in different ways. To explain rationally requires constructing a practical argument and to establish a logical fit between an agent's beliefs and motives and his actions. We understand the resolution to act in a particular way not by referring to antecedent conditions and general laws but by referring to an agent's beliefs (including his perception of the antecedent conditions) and his motives. To explain an action is not to predict the occurrence of a bodily movement on the basis of previous empirical observations, but to *understand* why it is rational to perform it. In rational explanations the connection between the *explanans* and the *explanandum* is thus not an empirical connection established through observation and inductive generalization, but a logical or conceptual connection established through reflection. Since in rational explanations the connection between the *explanans* and the *explanandum* is logical, not empirical, such explanations are at the same time justifications for the action: they are thus normative explanations. Normativity is the mark of the mental and of mentalistic explanations.

The principal merit of Dray's philosophy was that of focusing the debate on the distinction between normative and naturalistic explanations and thus on that aspect of the philosophy of historiography that clearly made it into a *Geisteswissenschaft*, or science of the mind. He clearly demonstrated that the explanation of action has a logical structure that differs from the explanation of events because action explanations

are a species of justification and, as such, have a normative element that is not found in naturalistic explanations.

Dray was not the only one to express such views. Analogous positions were developed by Wittgenstein (1953, 1958), Anscombe (1957), Winch (1958, 1964), Melden (1961), and Von Wright (1971). Differences apart, such thinkers were united in the view that actions cannot be explained causally or nomologically by subsuming them under empirical laws. One common perspective that united the defenders of methodological pluralism of this generation may thus be captured by the slogan "reasons are not causes."

The methodological difference between explanation in natural science and historiography is dictated by their different goals. The goal of natural science is the prediction and mastery of nature. This goal is well served by the method of inductive generalization. The goal of the sciences of mind is to understand, and understanding requires "making sense." We understand human behavior as an action not when we try to predict a bodily movement, but when we grasp the rationale for acting in a given way. This is not to say that rational explanations may not be used to make predictions. One might legitimately claim that someone with certain beliefs and motives is likely to act in a certain way. But predictions of this kind are very different from the ones found in natural science because they anticipate what might happen by showing how it would be rational for a given agent to act, rather than merely by projecting what will happen on the basis of repeated observations of constant conjunctions because, in a historiographic explanation, the beliefs and motives of the agents serve as the logical ground of the action, not its antecedent conditions.

Another key view that united defenders of the thesis for methodological autonomy in the mid-twentieth century was the view that whereas the natural sciences are guided by the epistemological search for truth, truth does not play any role in the explanation of action. In fact, the pursuit of truth positively hinders the hermeneutic search for understanding that guides the *Geisteswissenschaften*. This becomes particularly evident when one considers attempts to cast light on cultures that are radically different from those of the historian. Consider an example from R. G. Collingwood's *The Idea of History*: the case of a historian who approaches a text which states that a certain people would not cross a mountain chain because of the existence of evil demons in the mountain. One approach to the text would be to ask whether the statements it contains are true or false. Hume (1777) for example suggested that when a historian comes across statements which attest the occurrence of mesmeric events, the historian should consult his own experience. If the historian can find nothing of that sort in his experience, he is justified in treating the statement as false and thus in discarding it as unreliable evidence. A similar view of the role of the historian was put forward by Bradley (Bradley 1874). Bradley claimed that when confronted by unbelievable statements, the historian should put them to the test of his own experience and that if the statements failed such a test they would have to be discarded as false.

Defenders of the methodological autonomy of historiography would claim that the pursuit of truth positively hinders the hermeneutic search for understanding, since the task of the historian is to uncover the rationale underlying the actions of agents and to uncover such rationale requires endorsing, even if just for argument's sake, the epistemic and motivational premises of the agent. Whilst conducting his inquiries the

natural scientist must assume nature to be uniform, because the uniformity of nature is a presupposition for carrying out inductive generalizations, no such assumption can be made by the historian who must, on pain of failing to understand, assume that the agents whose thoughts he is trying to understand may not share his epistemic, moral, and aesthetic beliefs. The historian is concerned with these beliefs in so far as they are logically efficacious, that is, in so far as they provide grounds for acting, rather than with their truth or falsity. The issue for the historian is not whether the belief that there are devils in the mountains amounts to knowledge, but the role it plays in the agent's deliberation to act. Thus, while the natural scientist must presuppose nature to be uniform, the historian cannot presuppose the same with regard to human nature. It is possibly because the inadequacy of the method of natural science in understanding the meaning of actions becomes particularly evident when it is deployed to understand the actions of agents whose beliefs are radically different from those of the interpreter that the philosophy of historiography and cultural anthropology have provided a fertile ground for the argument against methodological unity to grow. It is thus no coincidence that one of the key contributors to the claim for methodological pluralism, Peter Winch, developed the argument for the autonomy of explanation in social science through a reflection on what it takes to understand alien cultures (Winch 1964).

Understanding Others

The debate for and against methodological unity that transpired in the immediate post-war years was primarily a conceptual debate. The questions at stake were questions such as: "what is the nature of event/action explanations?; what does it mean to explain something as an action as opposed to an event?" The supporters of methodological autonomy argued that if one is to do justice to the concept of action, one cannot explain what occurs by following the experimental method of observation and inductive generalization because such method cannot recover the rationale for performing a particular bodily movement. If we are to capture the rationale that underpins the action of, say, "opening a window," as opposed to simply predicting the bodily movement of "raising an arm," we need to ascribe the agent a motive that logically entails a certain description of that bodily movement.

Although the debate for and against methodological unity was primarily a conceptual debate, it did have a number of epistemological implications, particularly with regard to our ability to interpret and understand other minds. As we have seen, an important corollary of the claim that action explanations are a species of justification is that such explanations have a normative character. To explain what someone does is to impute to them a valid practical argument. Understanding others consists of making sense of what others do by finding a fit between the premises and conclusions of a practical syllogism. This view of what it takes to understand others entails the explicit denial of a claim that is often associated with the defence of the methodological autonomy of the *Geisteswissenschaften*, namely that understanding others requires "empathizing" with them. If action explanations are normative explanations then they are such that they could be endorsed by anyone who takes on board the epistemic and motivational premises of the action because in a normative explanation the action is the conclusion of a valid

and thus publicly re-enactable argument. It cannot be the case that a rational explanation is valid for you but it is not valid for me. If the explanation is rational (and we agree on the premises), then it is valid for both of us and indeed for any one who is capable of reasoning validly from premises to conclusions. To the extent that action explanations are rational explanations, the ability to understand requires not identifying with the agent but rationalizing the action. The view that action explanations require the reconstruction of a publicly re-enactable practical syllogism rather than an act of empathetic identification was argued by W. H. Dray and has been restated by Von Wright (Von Wright 1997). Some defences of the methodological autonomy of the *Geisteswissenschaften*, such as Dilthey's, do indeed identify understanding others with an empathetic projection, but this is not a feature that characterizes the account of the methodological autonomy of the *Geisteswissenschaften* as articulated by the likes of Collingwood (1946, 1999), Dray (1963), Gadamer (1975) and others, for whom the recovery of meaning is essentially a hermeneutic process that does not require a hotline to the inner workings of either the agent's or the author's mind.

Recent discussions in cognitive psychology have gone back to empathy to formulate an explanation of how we represent the mental activities and processes of others that dispenses with an appeal to the public and intersubjectively shared nature of reason. For example, mental simulation theorists (Gordon 1986; Heal 1986) believe a simulative strategy that requires putting oneself imaginatively in the other's place to be more parsimonious, and for this very reason more attractive, than an account of interpersonal understanding that requires appeal to rational arguments and the practical syllogism. Yet if, as it has been argued, the method of *Geisteswissenschaften* is hermeneutic and the task which confronts the interpreter is primarily a task of rational reconstruction, there is no essential link between contemporary simulation theory and attempts to defend the autonomy of folk psychological explanations in the philosophy of historiography. (For a contrasting view on this issue, see Stueber 2006.)

The Ontological Turn and the New Causalist Consensus

Whilst in the immediate post-war years the main battle ground for the argument for and against methodological unity was the philosophy of historiography and the philosophy of social science, more recent debates about the nature of action explanation have tended to take place primarily in the philosophy of action. The question concerning the relationship between the sciences of mind and nature comes to be framed in a very different way. The second half of the twentieth century saw the publication of a number of works (Kripke 1972; Lowe 1998) which sought to rehabilitate hard core metaphysics, against the previous philosophical focus on conceptual analysis. Wittgenstein and ordinary language philosophy went out of fashion and the conception of philosophy as a form of conceptual analysis receded into the background. In the context of this revival of metaphysics the fundamental questions addressed by the philosophy of action are no longer considered to be of a purely semantic nature. The problem is no longer a conceptual one: "what does it mean to explain something as an action rather than as an event?," but a metaphysical one: "how is mental

causation possible?" The fundamental problem to be faced by the philosophy of action thus becomes the so-called problem of causal over-determination.

The turning point in the debate concerning the relationship between explanations in the sciences of nature and mind was the publication of Davidson's influential article "Actions, Reasons and Causes" (Davidson 1963). In this paper Davidson denies one of the central tenets of the classical argument for methodological autonomy, namely the thesis that "reasons are not causes." Davidson occupies a somewhat hybrid position in the debate concerning the relation between explanation in the sciences of nature and mind. On the one hand he agrees with the claim that reasons are not causes in so far as the concept of action is not reducible to the concept of an event and action explanations have an irreducibly normative element that is not found in causal explanations. On the other hand, Davidson holds that reasons *are* causes in a metaphysical sense, because there is only one ontology, an ontology of events. Given this endorsement of the reasons-causes distinction in one sense and denial of the reason-causes distinction in another, for Davidson the problem becomes that of explaining the place of actions in the context of an ontology of mere events. Whereas for the earlier supporters of the methodological autonomy of the sciences of mind the task of the philosopher was to clarify conceptual muddles, and to recognize that once the conceptual questions were answered, there remained no residual metaphysical issues to be solved, for defenders of the autonomy of the mental in the second half of the century the real challenge was to address the metaphysical questions and to explain how the mind can fit into the natural world. Davidson's anomalous monism (Davidson 1970) was developed precisely to resolve this metaphysical challenge. Anomalous monism seeks to defend the methodological autonomy of the sciences of mind within the confines of a naturalistic metaphysics by arguing that mentalistic explanations supervene upon physicalist ones and thus that mental properties stand to subvening physical properties in much the same way in which, for Locke, secondary qualities such as colors, odors, and sounds stand to the primary properties of objects.

Davidson's anomalous monism answers the question "how is mental causation possible?" by showing that mental events are causally efficacious because they are ontologically identical with subvenient physical events even if conceptually irreducible to them. Thus, Davidson's solution to the problem of mental causation is to argue that mental events are causally efficacious in so far as they are ontologically identical with physical events, and the latter are, by assumption, causally efficacious. Davidson's solution to the problem of mental causation entails rejecting the bifurcated conception of the relationship between the sciences which governed the defence of the autonomy of historiography in the immediate post-war years in favor of a layered view of science which grants ontological or metaphysical priority to the explanatory framework of the sciences of nature and turns the philosopher into the mere under-laborer of science. Davidson's 1963 essay thus brings about a paradigm shift in the reasons/causes debates for it alters the very terms in which the case for the autonomy of the sciences of nature is to be conducted. The layered view of science, according to which the sciences of the mind supervene upon a physical base, implies a much reduced role for philosophy. The task of the philosopher is no longer that of arbitrating the disputes between the sciences of nature and mind a priori, by reflecting on the concepts of action and event and determining what kind of explanation they require, nomological

or rational. The task of the philosopher becomes that of explaining how it is possible to hold on to folk-psychological descriptions of human behavior in the light of a prior acceptance of the ontology of natural science. The success of Davidson's essay in revolutionizing the nature of the reasons-causes debate and of the issues at stake in the discussion concerning the relationship between the sciences of nature and mind is evident by the fact that some kind of causalism has become the standard view in contemporary philosophy of mind and action. Causalism informs what was possibly the most successful form of non-reductivism in the second half of the twentieth century, multiple realization functionalism (Putnam 1975; Fodor 1989).

Multiple realization functionalism identifies mental states with causal roles and denies the existence of lawful connections linking mental phenomena with their physical realizers. Since there are no lawful connections linking mental phenomena with their physical realizers, the empirical generalizations developed in the special sciences, such as psychology, are not reducible to the generalizations employed in lower levels explanations. This particular line of argument has been developed by Fodor (1989) who has sought to defend the causal efficacy of psychological generalizations by arguing that the explanations employed in the special sciences are not mere generalizations that are useful for pragmatic purposes, but have the status of genuine causal laws and thus that psychological explanations, far from being epiphenomenal qua psychological explanations, have causal efficacy. Fodor admits that the generalizations employed in higher level sciences such as psychology differ from those of physics in that they are hedged by *ceteris paribus* clauses but denies that such a difference is of metaphysical relevance and affects the causal status of psychological explanations. Fodor's account of special science laws was developed partly in an attempt to remedy a problem that besieged Davidson's defence of the autonomy of the mental; namely, that in Davidson's anomalous monism mental events remain ontologically inert and thus epiphenomenal qua mental. Yet Fodor's attempt to defend the autonomy of psychology by granting the generalizations employed in the special sciences the status of genuine causal laws raises even more worrying concerns as it entails that there are as many layers of reality as there are levels of explanation and ultimately flounders against the problem of causal over-determination (for a criticism of multiple realization functionalism, see Heil 2003a, 2003b).

No matter how successful Davidson's 1963 essay has been in shaping the contemporary debate, it is unclear whether the kind of non-reductive physicalism that Davidson advocates is compatible with a fully fledged commitment to naturalism (see Kim 1993, 1995) and thus whether a defence of the methodological autonomy of the *Geisteswissenschaften* can be accommodated within a conception of philosophy as the mere under-laborer of science. If both Davidson's and Fodor's causalist accounts of reasons fail to provide an adequate defence of the autonomy of the mental the best strategy for defenders of the autonomy of the sciences of the mind might be that of advocating a reconsideration of the debate in its classical formulation, as it took place in the heyday of the Dray vs. Hempel controversy (D'Oro 2005). Today defences of the autonomy of action explanation informed by the view that the debate is essentially a conceptual one and that there are no residual metaphysical questions to be answered once the conceptual issues are settled, are few and far between, but have not completely disappeared (Tanney 1995; Hutto 1999). Yet it is unlikely that such voices will find

further resonance as long as a return to metaphysics conceived as the study of things in themselves, coupled with a commitment to a naturalistic picture of reality, continues to dominate the philosophical scene. More widespread endorsement of the autonomous, non-causal nature of action explanation may have to wait until the tide turns and a conception of philosophy as a second-order discipline which provides a meta-level reflection upon the first order sciences, including the sciences of nature, is seriously reconsidered. Be that as it may, these debates within the philosophy of mind are yet to significantly affect the discussion of similar topics in the philosophy of historiography. Conversely, the relevance of the philosophy of historiography to discussions in the philosophy of mind is yet to be recognized.

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Colligation

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Historians often write with a question in mind: What did a person achieve? How did an event unfold? Why did people behave in a certain way? But sometimes historians write, not to answer a question, but to display a pattern they have discerned among events in the past, a pattern which gives each of those events meaning and significance, and which sometimes helps to explain them. The practice of producing patterns like this is called “colligation.”

David Hackett Fischer’s book *Liberty and Freedom* (2005) provides an impressive example of colligation. It is a monumental record of symbols, writing, and events related to liberty and freedom in the history of the United States of America. He sees them as all related to a theme or pattern of change in the history of the USA. “The central theme of American history,” he writes, “is about the growth of liberty and freedom . . . Through the span of four centuries, every American generation without exception has become more free and has enlarged the meaning of liberty and freedom in one way or another” (pp. 721, 722). He adds: “In short, the history of liberty and freedom clearly fits a Whig model of American history” (p. 722; see Butterfield 1931). By relating his collection of symbols, ideas and events to the theme of developing liberty and freedom, he gives each of them significance as contributing to a change of great importance.

The Concept of “Colligation”

The practice of colligation was first described by William Whewell in the mid-nineteenth century. He was fascinated by the way in which scientists relate apparently independent facts by means of concepts. His example is of Johannes Kepler, who noted various positions of the planet Mars and then described it as following the path of an ellipse. Such conceptions, Whewell said, provide “some true bond of unity by which the phenomena are held together.” This he referred to as “the colligation of facts,” adding that “we may apply this term to every case in which, by an act of the intellect, we establish a precise connexion among the phenomena which are presented to our senses” (Whewell 1967: vol. 2, 36). The word “colligation” comes from the Latin word *colligere*, meaning to bring things together.

W. H. Walsh noticed that historians sometimes relate events in the manner Whewell had described. He said that “different historical events can be regarded as going together to constitute a single process, a whole of which they are all parts and in which they belong together in a specially intimate way” (Walsh 1958: 23). The “intimate” connections he mentioned are what he calls “intrinsic relations,” by which he meant thoughts or ideas which find expression in human actions. Often a series of actions can be related to a purpose or policy that directs them. For instance, Walsh said, many of Hitler’s decisions can be related “to the general policy of German self-assertion and expansion which Hitler pursued from the time of his accession to power” (p. 59). Sometimes the ideas by which historians unify events are not purposes or policies, but are sets of values, such as are found in “the Enlightenment, the Romantic movement, the age of reform in nineteenth-century England, [and] the rise of monopoly capitalism” (p. 60).

There is no reason why the concepts by which historians unify events should be of a purpose or a value. After all, the idea by which Kepler unified observations of the position of Mars was an idea of the shape of its trajectory, an ellipse. Similarly historical events can often be colligated as constituting a change of a certain general kind, such as an instance of economic growth, or political decline, or of a scientific revolution (McCullagh 2004: 127–8). Such phrases describe the kind of change brought about by a collection of historical events, admittedly in very general, and therefore rather vague, terms.

For example in his book *The Great Wave* (1996), D. H. Fischer described the history of price inflation in the west as a series of statistical “waves,” at their peak in the thirteenth, sixteenth, eighteenth, and twentieth centuries. He argues that these were not “cycles,” as is sometimes claimed: “Cyclical rhythms are fixed and regular. Their periods are highly predictable. Great waves are more variable and less predictable. They differ in duration, magnitude, velocity, and momentum” (p. 9; he discusses cyclical theories in appendix E, 273–7). The concept of a wave captures the shape of the changes in prices that Fischer identified, and went on to explain.

Notice that sometimes the colligatory concept is a metaphor. Waves in the ocean not only have the shape and frequency Fischer describes, but are wet as well. Clearly it is their shape, not their wetness that Fischer is alluding to when he says changes in prices came in waves.

Quite often a collection of historical events is colligated in both ways, by referring to the ideas they expressed and the shape of the change they produced. For instance the events that constituted the French Revolution were driven by an ideology of republican freedom (valuing liberty, equality, and fraternity), and effected a revolutionary change in the constitution, from government from above (by a feudal king) to government from below (by an assembly of elected deputies).

Some Common Hazards in Colligation

When colligating historical events, there are some mistakes which it is easy for historians to make. Revealing these gives greater insight into what the process of colligation in historiography involves.

(1) While many Americans wrote and spoke about liberty and freedom over the last four centuries, they did not always mean the same thing by those words. Fischer is very careful to distinguish the different senses in which people used them. The danger, which he avoids, is of assuming that because they were all committed to liberty and freedom they were all supporting the same thing. Often they were not. For instance, when Lincoln issued the Preliminary Emancipation Proclamation to free the slaves in 1862, he intended it to advance liberty in the land. But supporters of the Confederate cause in the South saw him as attacking their liberty to keep slaves, reducing rather than extending liberty (Fischer 2005: 335–7). Fischer noted that “Northern ideas of freedom, union and equality triumphed in 1865 [with the North’s victory over the South], but southern ideas of liberty, independence, states’ rights, and racial hierarchy endured” (p. 355). Northerners viewed liberty as freedom from oppression. Southerners understood it as freedom to own and use one’s property, including slaves.

To defend his thesis that liberty and freedom expanded throughout the history of America, Fischer has to provide his own definition of those words, and then display the extension of liberty in those senses. This he attempts to do. In the Introduction to the book he defines “liberty” as freedom from restraint to live as one wishes, and “freedom” as belonging to a society of free people with equal rights. In the course of the book he describes particular events, like the Emancipation Proclamation, that extended liberty and freedom in these senses.

(2) Another danger is that historians will assume a certain pattern of events existed, and find some evidence to support their assumption, but fail to check just how consistently the pattern was maintained. This can be called “top-down” historiography, which begins with a general interpretation and then fits a class of events to that grand model. Much more reliable is “bottom-up” historiography, which examines members of a class of events in detail and then, having considered them carefully, decides whether there was a more or less consistent pattern to be found among them.

Fischer seems to have committed the error of writing “top-down” historiography. There is no doubt he would like to find more or less continuous growth in the liberty and freedom of people in America throughout its history. But the facts that he himself records describe a decline in liberty from the mid-twentieth century. It seems to begin with the enthusiastic reception given to Friedrich Hayek’s book *The Road to Serfdom*, published in 1944, which argued against the welfare-state and big government (Fischer 2005: 623–4). Hayek would confine liberty to freedom from coercion. Since then, “libertarian conservatism,” as Fischer calls it, has flourished. Those leaders who tried to extend liberty and freedom to the oppressed were assassinated: John Kennedy in November 1963, Martin Luther King and Robert Kennedy in April and June 1968. Bill Clinton attempted some social reforms, but “his idea of freedom as full membership for all people in a free society was seen from the right as profoundly hostile to liberty as individual independence and responsibility” (p. 701). President George W. Bush, Fischer remarks, “was not much for civil liberties or civil rights” (p. 705). The “war on terror” has seen further erosion of human rights (p. 712). On Fischer’s own evidence there has not been a steady growth in liberty and freedom in America over the last half century. Quite the reverse.

Fischer admits there have been periods of reaction, constricting people's liberty, but affirms a "central trend" of expansion (p. 722). How many and how great can exceptions be for a trend to exist? All one can say is that the more numerous and extensive the exceptions, the less accurate the general colligatory claim is.

(3) Sometimes historians colligate a number of features of a historical subject – for example the character of a person, the motives for an action, or the policy of a government – in order to provide a neat interpretation of it. The danger in this case is that the features of the subject that the historian colligates might give a misleading idea of the subject as a whole. For instance Fischer described American reaction to the Japanese attack on Pearl Harbor as motivated by concern that the Japanese were bent on the destruction of their free society, as well as by anger at their treachery (p. 512). In his comments on Fischer's book, Brian J. Glenn objects that "Fischer's account does not appear to leave much room for other feelings, such as anger and revenge, fear of being conquered, repulsion at genocide, or racial animosity" (Glenn 2005: 21). Clearly Glenn is worried that Fischer's account of American motives for retaliation was partial and misleading.

The most common examples of misleading colligatory interpretations are those of people's character. When describing people they admire, historians will sometimes select and emphasize admirable aspects of their character, but people they dislike will be studied for evidence of wickedness. Getting the balance right can be difficult.

It is important to realize that while some colligatory descriptions are intended to describe an independently identifiable historical subject, such as a person's character, not all are. Some just identify a pattern among a class of events, which describes nothing but itself. The growth of liberty in the United States, if it had occurred, would be such a pattern, as are the waves of price inflation in the history of the west. There is no question of patterns like these being fair or misleading accounts of something else. The only question is whether they accurately describe a relation between the individual actions or events to which they refer. As was remarked before, the more exceptions there are to the pattern, the less accurate an account of those individual events it is.

Philosophical Issues

Two philosophical questions have been raised about colligatory patterns: (1) do such patterns exist in reality? and (2) can descriptions of such patterns be true?

F. R. Ankersmit once argued that colligatory patterns can be detected in the narratives historians write, constituting the interpretations they provide of the events they describe, but they do not really exist in the past itself. Ankersmit called these interpretations "narrative substances," or "narratios," which can be named by colligatory concepts. These concepts, he said, "do *not* refer to things in or aspects of the past" (Ankersmit 1983: 100). Individual sentences in a historiography can refer to things that really existed, but the total picture they provide, "the narrative substance," does not (p. 101). If the patterns named by colligatory concepts do not really exist, then clearly no statements about them can be true or false as descriptions of things in the world.

In a subsequent publication Ankersmit has changed his position, declaring that historians' interpretations of past events are "far from being exclusively a question of viewpoint, of seeing the past from an angle that as such is independent of past reality itself." Rather, "a certain quality of the represented reality actually corresponds to the 'representing as' . . . in the writing of history" (Ankersmit 1995: 230).

In defense of his original skepticism, Ankersmit produced two lines of argument. One simply denies that the patterns historians discern in events really existed in the past. They are not discovered in the past, he says, but rather they are constructed by historians. "For the 'historical landscape' is not *given* to the historian; he has to *construct* it . . . The structure of the narratio is a structure *lent* to or *pressed* on the past and not the reflection of a kindred structure objectively present in the past itself . . . All this means that the past as such has no narrative structure – narrative structures occur only in the narratio" (Ankersmit 1983: 86).

Despite all the italics, I do not think this statement is true. Good historians, namely those who are not dominated by preconceptions, do discover patterns in past events, just as Kepler discovered the path of Mars by carefully considering observations of its position over time. Certainly the scientist or the historian makes the discovery: the pattern they discover is not stated in the initial data they consider. But only historians wishing to shape their narratives to suit their interests will press an interpretation upon the data. Conscientious historians will carefully consider which patterns best fit the data available to them.

Doubts about the reality of patterns among events can arise if one is a metaphysical physicalist, and so assumes that the only things that exist are individual physical objects and changes in such objects (events). For instance, one could observe the planet Mars in various positions, but one could not observe the elliptical path it followed. As Whewell said, the path is something contributed by the mind of the scientist, and in fact it took Kepler a lot of work to discover it. Turning to history, the particular physical events that constituted the French Revolution, or at least some historian's version of the French Revolution, were observable and real. But the constitutional revolution that they achieved, from a feudal to a democratic form of government, is something that historians discover by a process of reflection.

In fact people constantly refer to strictly unobservable things or attributes of things in the world, such as people's liberty and freedom. Statements about unobservable things are judged to be true of the world if they have more or less definite truth conditions, ultimately observable truth conditions, and there is good reason to believe these conditions to be satisfied. That is why the definition of such terms is so important in establishing their truth. Fischer recognized this in providing carefully considered definitions of liberty and freedom. He wrote:

In analytic passages throughout this book we shall use the two words liberty and freedom in their original and literal meanings. Liberty will refer to ideas of independence, separation, and autonomy for individuals or groups. Freedom will mean rights of belonging and full membership in a community of free people (whether a tribe, a nation, or humanity itself). The phrase "liberty and freedom" will also appear. It refers to the entire range of modern ideas that combine elements of these ancient ideas in many different forms. (Fischer 2005: 10)

What is a bit unsettling about this analysis is that liberty in fact is not an idea but a state of personal independence and autonomy. He acknowledges the distinction in his concluding remarks, quoted above: “every American generation without exception has become more free and has enlarged the meaning of liberty and freedom in one way or another” (p. 722).

The important philosophical point is that when the truth conditions of a description of the world are satisfied, one is entitled by the rules of language to believe the description to be true as a description of what the world itself was like. When Lincoln outlawed slavery, he increased the freedom of slaves under the law. When Congress outlawed certain forms of racial segregation, and gave African-Americans the right to vote, thanks largely to the work of Martin Luther King and his followers, it further increased their social and political freedom. Thus one can justify the claim that freedom of African-Americans has increased over the last hundred and fifty years or so, assuming that nothing has happened in that period to reduce their liberty. It is wrong to suppose that the patterns historians describe when interpreting events did not really exist.

The second reason for Ankersmit anti-realism about colligatory statements was his conviction that since each particular interpretation of historical events is unique, it cannot be the subject of a general description. As he puts it: “A narratio cannot be used to characterize a particular class of historical states of affairs . . . Narratios always account for one specific historical situation only” (Ankersmit 1983: 69). He goes on to say that every historian who describes the French Revolution, for example, does so in a different way. So the phrase “French Revolution” has no fixed meaning, nor is there an agreed collection of events that constitutes the French Revolution to which the phrase could refer (pp. 69–73).

This objection raises the question of whether colligatory descriptions should be regarded as general classificatory terms or proper names of unique patterns of events found in history. It is fairly easy to answer this question when the colligatory descriptions employ general terms. For example, historians have written about a variety of political, industrial, and scientific revolutions. Each instance of a revolution has its individual characteristics, just as each individual person does. But each also has characteristics in common with others which warrant the use of these general terms. So a term like “the French Revolution” does designate a collection of unique events, but it draws attention to a pattern that can be found in other political revolutions, namely a great change in the country’s form of government.

Even words used to refer to values that a number of events have in common, such as “a revival of classicism” or “the growth of neo-conservatism,” are general terms that can be applied to various cultural movements. The only colligatory terms that seem to refer to unique patterns are those that refer to a unique goal or outcome: for example, “the creation of the Sydney Opera House.” This phrase does not designate a class of architectural processes with a variety of instances.

Suppose there are several different ways of describing the creation of the Sydney Opera House, several different narratives that can be told. Does this mean it is untrue to refer to each of these as a description of the creation of the Sydney Opera House? Even if they differ in detail, what they presumably all have in common is an account of the main stages in the design and construction of the Opera House. For the name of an

interpretation to be truly unique, it would have to refer to just one interpretation, for instance “John Smith’s account of the creation of the Sydney Opera House,” assuming there was only one such account.

Because the truth of colligatory statements depends crucially upon the definition of their terms, it is clear that when the definition is vague it is difficult to know precisely when they are justified. Also, when the class of events or states of affairs being colligated is vaguely defined, it is difficult to judge whether a colligatory description of them is justified or not. For example, it has been said that there was an Industrial Revolution in Britain between 1760 and 1830. Joel Mokyr’s discussion of this proposition makes clear just how vague the concept “Industrial Revolution” is, and how vague the reference to “Britain” is as well. Is an *Industrial* Revolution a revolution in technology, or in the organization of industries, or in capital investment in industry, or in all of these? And how sudden and great a change is needed for it to be a *Revolution*? A change that takes seventy years is not very sudden at all. And exactly what industries in Britain are being described by the phrase Industrial Revolution? Mokyr remarks that the “Industrial Revolution was, above all, a regional affair, affecting Lancashire and parts of the adjoining counties and the Scottish Lowlands but leaving most of the rest of the country without visible marks. As late as 1851, only about 27 percent of the British labor force worked in the industries that were *directly* affected” (Mokyr 1999: 4–5). There were major changes within some British industries, but not within British industries as a whole. If historians wish to assert that there was an industrial revolution in Britain, then to be accurate they must specify what they mean by “Industrial Revolution” and what parts of British industry were affected by it.

One reason that many colligatory concepts are annoyingly vague is that, as already mentioned, they are metaphorical. Words such as “revolution” and “evolution,” “growth” and “decline,” “birth,” and “decay” suggest mostly organic metaphors whose meaning can be quite hazy when applied to history.

Nevertheless, all colligatory descriptions of patterns in the past have truth conditions which, if satisfied, warrant belief in their truth. Deciding the truth of a colligatory description is difficult when the meaning of the description is vague, and when the class of events it purports to describe is ill-defined.

Colligation and Postmodernism

Keith Jenkins has defended a postmodern account of history. “Modernists,” he said, were those who thought historiographic interpretations could be true and objective. “Postmodernists” are aware that historiographies are verbal constructions, built upon ideas about the significance of the statements and symbols found in available evidence that are shared in the historians’ culture. Postmodernists say that the patterns of events and of social relations that historians describe exist only in the historians’ minds. They cannot really be observed in the world, neither through historiographic evidence nor in historical events. In his early writings Ankersmit defended this kind of position.

Jenkins believes that events really have occurred in the world (p. 18), but he denies that historical descriptions even of particular events can be true. To describe an event,

historians use words from their own culture which draw attention to features of the event that they find interesting. Indeed such descriptions are usually part of an interpretation of events surrounding the event in question, its context thus contributing to its meaning. So historians' descriptions of particular events are really just their interpretations of those events, drawing upon the language and concepts of their culture.

Consequently, in Jenkins' opinion historiography "now appears as a self-referential, problematic expression of "interests," an ideological-interpretive discourse without any "real" access to the past as such; unable to engage in any dialogue with "reality." In fact, "history" now appears to be just one more "expression" in a world of postmodern expressions: which of course is what it is" (Jenkins 1995: 9).

Postmodern skepticism had several sources, and Jenkins mentions some of them. Richard Rorty argued that it is naïve to suppose science mirrors nature, as the language of science reflects the theory of nature in vogue at the time. Scientific theories have changed quite often over the centuries, and there is no reason for believing any of them to have captured the absolute truth, whatever that might be (Rorty 1979). Hayden White revealed the influence of various literary tropes and political interests in historiography, showing that historiography is at least as much a product of the historian's society and culture as it is a rational interpretation of available evidence (White 1973). Anthropologists and cultural theorists, such as Clifford Geertz (1973) and Lyn Hunt (1989), explained that even those who performed the actions studied by historians envisaged them in culturally specific ways, ways that are sometimes at odds with the concepts employed by historians. So it seems there is absolutely no culturally neutral description of historical events available. As Bonnell and Hunt put it, postmodernists denied that language could ever "penetrate to the truth of existence; it itself configured the expression of social meaning and functioned as a kind of veil between humans and the world around them" (1999: 9).

Ankersmit has never been as skeptical as Jenkins, for he always allowed that descriptions of individual historical events could be true. But, as we have seen, he denied that the patterns historians discern when they examine the relations between events ever really existed. Jenkins welcomed Ankersmit's thesis as providing philosophical support for his own skepticism (p. 21).

The arguments used against Ankersmit in the last section can be reiterated against postmodern skepticism such as Jenkins'. First, the fact that we cannot literally see many social facts, such as people's roles, status, or power, does not mean they do not exist. Seeing how people behave and how others respond to them provides evidence enough of these things. Second, the fact that these words are derived from social (or scientific) theory does not mean they lack truth conditions. The role, status, and power of a shop assistant and of a shop owner are plain for all to see who know what the words mean, and who study their behavior. Given adequate evidence, it is quite reasonable to believe certain descriptions of the world to be true.

What postmodern theory has taught us is that although our knowledge of the world is a product of how the world is and appears to us, it is also a product of our culture's ways of thinking and speaking about it, and of our social and personal interests. If modernism was naïve in supposing that some absolute truth about the world could be discerned, then postmodern theory has served a useful function. It fails, however, when it recommends absolute skepticism. It is reasonable to believe many descriptions of

the world to be true, even though we cannot prove them necessarily to be so (see McCullagh 2004: ch. 1).

The Value of Colligation

The debate as to whether colligatory patterns are general or unique might seem rather pedantic and unimportant, but it is not.

Those historians wedded to narrative forms delight in describing particular events in their unique detail, eschewing comparison and generalization. They believe that every event has unique local causes, that actions are performed for unique sets of reasons, and that historians would be misleading their readers if they explained them by means of rough causal generalizations. Furthermore, they stress that every historian sees the past differently, from a different perspective, and that there is no single correct way of describing the course of historical events.

One consequence of this emphasis on uniqueness in history and in historiography is a failure to discern patterns that repeat themselves in history, patterns of great significance to people today. Fischer's book *The Great Wave* is an outstanding example of the way in which a study of patterns in history, in this case of price revolutions in European history, can be advantageous. In the concluding chapter of the book Fischer identifies the common features and causes of the price revolutions he has described, and draws out normative lessons for contemporary policy makers. He notes that "Many heads of government, leaders of corporations, business managers, economic theorists, and private investors have very little historical understanding of economic processes which they confront" (Fischer 1996: 254). His analysis of price revolutions is not only fascinating in its own right, but it yields very useful lessons for today.

There are various kinds of general patterns in history. Some are regular patterns of cause and effect. Colligatory patterns differ from these in that they involve seeing individual events as forming a conceptual whole, such as the elliptical path of a planet, the growth of freedom or the waves of fluctuation in prices. Once such patterns have been observed, it is then often possible to investigate their causes and effects.

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The Laws of History

STEPHAN BERRY

A Systematic Look at Laws in History and Nature

In this chapter, we will see that there can be productive and profitable applications for generalizations about history, ranging from prehistory to present-day politics, and that talking about historical laws does not necessarily imply “historical determinism.” But first, in order to address the question “what are laws?,” we will take a look at some issues in natural science and its philosophy.

During the nineteenth century and well into the twentieth, the prevalent view of natural laws was a pathetic vision of Eternal Truth to be found by means of such laws; they were considered the crucial agents which dictated the behavior of the universe, and finding these laws was seen equivalent to reading the Thoughts of God. Today, the production of eternal truth is no longer on the scientific agenda, and a major goal of the scientific enterprise is to solve individual problems, to decipher how specific systems work. Therefore, these more modest aims of present-day natural science are in closer resemblance to those of historiography, which is in most cases concerned with the reconstruction of individual events and processes.

Accordingly, laws of nature have lost their elevated status in a number of respects. First, there are many *indeterministic* laws in nature (Berry 1999), and this resorting to statistical descriptions is not merely due to our incomplete knowledge of the exact dynamics (as was still hoped by Planck and Einstein); rather, indeterministic behavior is an essential property of many systems. And the same reasoning applies to history, where there is no necessity to equate laws with determinism (see also the next section).

Second, there are several physical laws which are derived and used by scientists in a “sloppy” way, which from a strictly mathematical point of view, is incorrect (Davey 2003). The justification for this sloppy approach lies in the fact that laws have become instrumental for scientific problem solving, rather than ontologically independent entities.

Third, the complete computability of the world is no longer an objective of science (Schmidt 2003); for many systems complete computability is neither possible nor required for an adequate understanding. This is, on one hand, due to the above mentioned indeterminism found in many systems, but, on the other, it is also caused by *the problem of many interacting laws*. A typical experimental setup in the laboratory

is carefully chosen to enable the study of *one single law* in all desired detail, but for most cases outside the lab, a large number of different laws will apply to any given system at the same time. Take, for instance, the weather: all the ingredients – the rotation of the earth, the daily and annual intensity variation of solar irradiation, the properties of air and water vapor etc. – and the laws describing their behavior are fully known, but, nevertheless, weather is the archetype of an unpredictable system. Nancy Cartwright (1999) has accordingly proposed the universe to be “dappled,” implying the existence of places where the laws obtained in the laboratory are invalid. This, however, is a non sequitur, an erroneous conclusion that was rejected already by Nicolai Hartmann (1950: 395, 416), who maintained the validity of individual laws even in those complex cases where they become *apparently* invalid. With respect to historiography, it seems immediately apparent that societies (cultures, states etc.) in their complexity resemble more the notoriously unpredictable weather, rather than the simplified systems studied by laboratory science, and, therefore, it is possible that at least some historical laws remain undetected because the complexity of history does not allow to discern them.

The most surprising change in the status of natural laws is seen when we look at the largest scales of scientific inquiry. In contemporary cosmology it is well conceivable that our “universal” laws of nature are merely local environmental conditions of our particular region of the universe, the “observable universe,” which may be part of a much larger universe (Carroll 2006). Such properties of the universe, as we observe them today, are perhaps only one peculiar solution of fundamental laws; the evolution of our universe, which gave rise to its particular features, may be only one out of a vast number of possible alternative histories. This new way of looking at the history of the universe was proposed recently by Hawking and Hertog (2006). Accordingly, they suggest that cosmology should work *backwards* in time, starting with the evidence – the present state of the universe – and subsequently reconstruct the history that created this evidence, rather than trying to predict the features of the universe using a priori fundamental laws. The novel strategy of Hawking and Hertog thus resembles closely the methodology of historiography; as one commentator noted

Hawking and Hertog’s suggestion is indeed a radical shift of approach: it is, as befits the description of the evolution of the universe, more akin to the holistic methods of “universal history” . . . than anything familiar from the physical sciences. (Bojowald 2006: 989)

Severe philosophical attacks on the notion of laws of nature were leveled by Stephen Mumford (2005) and Bas van Fraassen (Monton and van Fraassen 2003). Both suggest dropping the notion of natural laws altogether. They do not deny the existence of the observable regularities; rather, they argue against a realist interpretation of laws. Mumford prefers to describe law-like behavior in terms of *dispositional* or *modal properties* of systems, while van Fraassen stresses the importance of *symmetries* to account for the apparent existence of “laws.” Consider the following, simplistic but instructive example: pure water will always boil at 100°C, but one does not need to invoke an external “law” here, if it is conceived as an agent that somehow governs the behavior of the molecules to ensure this regularity. Rather, it is a symmetry between all water

molecules themselves, or, in Mumford's parlance, a dispositional property of these molecules which gives rise to the observed regularity.

However, despite its problematic nature, the notion of natural law is firmly entrenched among scientists and the public. Accordingly, I also cling to this expression throughout this chapter, but it should be clear that natural or historical "laws" are conceived here as expressions of regularities and not as real entities operating upon systems.

But how, then, could laws arise at all in the course of history? The "historical necessity," which played such a prominent role in earlier systems up to Spengler and Toynbee, was devised as an external agent, a substitute God, acting upon societies and forcing their trajectories in determined directions. This type of laws may be called "genuine historical laws," because they are thought to operate directly at the level of individual historical entities (populations, societies, cultures, states etc.). In contrast, a concept of laws in history that would be acceptable by modern standards refers to recurrent, law-like features of historical processes which emerge as a consequence of symmetries.

Two major possible sources of such symmetries appear important in current research. First, through interactions of societies with each other and, in particular, with the environment. Integrative models, which use ecological regularities for elucidating historical processes, will be presented below. It is important to stress that this does not imply determinism: the laws of ecology provide the *boundary conditions*, they channel the course of history without strictly determining it. In the light of these laws, some outcomes of history are more likely than others and some are outright impossible.

The second possible source of invariants influencing history are its constituent human beings themselves:

No one makes, writes, or reads history without the continuous causal participation of human nature. Human nature is necessarily involved in everything humans do. It follows just as necessarily that human nature shapes the course of human affairs, the way humans perceive their affairs, and the way they represent their affairs. (Brown 1999: 138)

While earlier approaches have stressed alleged genetic differences between different human "races," the wealth of genetic data that became available in recent times has shown the fundamental genetic similarity of all humans and, moreover, it has become evident that the major part of genetic variation between individuals occurs already *within* human populations, rather than between different ethnic groups. So we may assume that there is a fairly constant human nature throughout history, but the application of this insight to problems of historiography is not straightforward. It is exactly this uniformity in space and time which causes epistemic problems when it comes to explaining historical *change* and *differing historical trajectories* of different societies. For instance, Brown mentions the human tendency of "group-think," the universal bias of perceiving reality in terms of "*we* versus *them*," and he refers to the ethnic conflicts of the Balkans as an example of its possible violent consequences (Brown 1999: 143). However, violence motivated by ethnocentrism does not occur in all places and at all times with the same intensity, and the particular outbreaks of intergroup violence as seen in the Balkans need more specific – genuinely historical – explanations besides a

universal human disposition. Therefore, the problem is to find general statements about human behavior, which go beyond trivialities like “everybody strives for sex and subsistence” and which can elucidate *specific* historical events.

In addition, we can expect a large number of different regularities in human behavior operating at the same time, as described by the various laws of psychology, sociology, or economy. Therefore, it is likely that some potential historical generalizations remain undetected because of the problem of complex interacting laws as described above. Human nature constrains history without determining it, in much the same way as ecology provides constraints. Invariances from ecology and human nature therefore play in history a remarkably similar role as the laws of physics and chemistry do in biological evolution: they channel and constrain the course of events, but they do not determine it in detail. No physicist working 300 million years ago could have predicted the future emergence of birds and bats, but, on the other hand, any flying animal must obey physical laws and, thus, a flying animal of the size of a Brachiosaurus is strictly impossible for any evolutionary scenario.

The History of the “Laws of History”

Among contemporary scholars, the concept of historical laws is not a popular one, but this was quite different in the past: Giambattista Vico, the founder of the modern philosophies of historiography and history, believed in such laws, and so did many thinkers after him, such as Montesquieu, Comte, Herder, Saint-Simon, and Hegel. This belief intensified in the nineteenth century, influenced by the success of the natural sciences. Given the prospect of mechanistic explanations for all phenomena in nature, it seemed logical to propose the same kind of explanations for human affairs in general, and historical processes in particular. This is illustrated by the fact that Marx, an enthusiastic advocate of laws in history, desired to dedicate his *Kapital* to Darwin. The belief in laws of history was echoed even by literary works, such as Tolstoy’s *War and Peace*.

In 1920 and 1922, Oswald Spengler published his two-volume *Der Untergang des Abendlandes*, which promised in the subtitle an “outline of a morphology of world history.” He explained world history by the individual histories of eight major cultures, each of which developed according to relentless internal laws – without a chance for human interventions that might change the course of history. This fatalism was criticized by Arnold Toynbee, who published his own *A Study of History* in twelve volumes between 1934 and 1961. But despite being critical of some aspects of Spengler’s approach, in other aspects Toynbee was more a follower than an opponent of Spengler. Like him, he saw immutable laws of history at work, and he treated world history as a succession of more or less independent major cultures, each bound to follow a similar trajectory.

Now time was ripe for a thorough philosophical analysis of the notion of historical laws, and this was provided by a series of publications by C. G. Hempel from 1942 on, promoting a model often called the “covering-law model,” sometimes also titled the Hempel–Oppenheim Scheme or the deductive-nomological concept of explanation (Hempel 1963). This scheme can be summarized as follows: An event, which is to be explained, is represented in the form of a singular statement $E(N)$. All we need for an

explanation of this event then is a general law G of the form $A(X) \rightarrow E(X)$ and the antecedent in the form $A(N)$. Substituting the general variable X by the N characterizing the peculiar case of interest, we obtain the explanation for the occurrence of $E(N)$ in a deductive way. In a more general version the scheme refers to a series of antecedent conditions $A_1 \dots A_k$ and a corresponding series of general laws $G_1 \dots G_k$. Explanations of past events according to this scheme have the same logical form as predictions of future events and this is in fact a basic assumption of the covering-law model: Prediction and explanation are regarded as symmetrical.

The scheme was initially presented as a logical reconstruction of explanation in the natural sciences and there is a vast literature on the question whether this scheme is also a valid model for explanations in history. Maurice Mandelbaum (1974) has taken an intermediate position with respect to the covering-law model (in some respects he is following earlier analyses by Dray 1974). First, it should be clear, as Mandelbaum notices, that historical occurrences are not wholly unique, they are not completely unrelated to other events. If they were, then there would be no possibility of explaining or understanding them. In this respect a certain background of law-like behavior has to be assumed in historiography and it is the merit of the covering-law theorists to have insisted upon this. However, formulating the concrete laws which are to be used in the covering-law model is problematic. In most cases proposed laws for explaining singular events turn out to be defective in at least one of the following ways: they may be true but trivial; they may be too vague to be applied in a meaningful way; they may be *ad hoc* constructions which cover only the case given and lack the generality of real laws; or they may be simply wrong. Mandelbaum also stresses another point which is true for all laws, in natural science as well as in other disciplines: laws relate classes of events to other classes of events and they do not make reference to individual cases. This was admitted by Hempel, but this is, as Mandelbaum continues, exactly the point why most historians regard the covering-law model as an inadequate description of historical explanation. The covering-law model refers merely to the necessary conditions of an event insofar as it is the representative of the respective class without including the sufficient individual conditions of this peculiar event and therefore "Hempel's analysis is really remote from the tasks which most historians have set themselves" (Mandelbaum 1974: 59ff). This is also the core of Dray's attacks on the covering-law model (Dray 1974).

Karl Popper (1957) and Isaiah Berlin (1974) denied the *existence* of laws of history altogether, mixing in a somewhat confusing way ontological (there are no laws) with ethical arguments (there must be no laws). Their main focus of attack was *historical determinism*, the assumption that the course of history is completely determined by historical laws:

The notion that history obeys law, whether natural or supernatural, that every event of human life is an element in a necessary pattern, has deep metaphysical origins: . . . the teleological outlook whose roots reach back to the beginnings of human thought. (Berlin 1974: 161)

But laws are independent of determinism and teleology. There are indeterminate laws and most of science uses laws without being teleological. The close connection

between laws/determinism and the problem of free will, as drawn by Berlin is also questionable. Arguably, if everything were determined, there would be no room for a human free will, for moral decisions, thus endangering the whole fundament of ethics. But metaphysicians did devise compatibilist systems where free will of autonomous self-determining individuals and determinism co-exist.

By the end of the 1960s, the whole concept of historical laws had been thoroughly discredited: the debate on the covering-law model turned out futile for the purposes of historiography; the grand schemes à la Spengler and Toynbee were rejected by the majority of historians; the criticism of Popper and Berlin further undermined belief in historical laws; the Bolshevik association of political totalitarianism with belief in laws of history discredited the very idea; and, finally, the natural sciences were no longer considered the authoritative model to be emulated by the humanities.

The covering-law model was also attacked because of the claimed symmetry between explanation and prediction:

The logical structure of a scientific prediction is the same as that of a scientific explanation . . . In view of the structural equality of explanation and prediction, it may be said that an explanation . . . is not complete unless it might as well have functioned as a prediction. (Hempel 1963: 347ff)

This symmetry was rejected by Scriven, who concluded

that the logical argument for correlation of good predictions with good explanations is not formally sound and has a limited range of application and little practical significance even in that range; that good predictions are impossible in large areas of the natural and applied sciences where simple quantitative laws and measuring techniques are not available; but that in such areas, as in history, good explanations of the poorly predictable events are commonly available. (Scriven 1963: 445)

And this statement is an appropriate description of the situation in many fields of natural science (see Berry 1999, for examples). A symmetry between explanation and prediction can exist only for those laws which are by themselves symmetric with respect to time-reversal, such as the Newtonian equations of motion. However, many laws of nature do not obey this symmetry, and our universe is inherently asymmetric with respect to the direction of time (Penrose 2005).

Furthermore, the bias of considering law-based explanations the ultimate goal of scientific enterprises betrays the heavy influence of physics upon the formulation of the covering-law model – *laboratory physics*, to be precise. Under laboratory conditions, one has (more or less) full control over the experimental setup, and all parameters of interest can be determined, limited only by the available techniques. Under these circumstances, the relation between empirical facts and theoretical laws and explanations may work as envisaged in the covering-law model. However, the situation is different for disciplines such as historiography and archaeology, or, in the realm of natural science, cosmology, and paleontology. In these areas one regularly encounters the problem of *incomplete* data – not all parameters can be known with the desired precision and a major task is to reconstruct unknown events from this fragmentary evidence, rather than explaining known events by means of established laws.

Therefore, it seems wise not to stress the symmetry between prediction and explanation, and to take instead a closer look at the symmetry between *prediction and reconstruction* of events that must have happened in the past. In this context, one also finds a useful possible role for laws in the historiographic sciences: they participate in the reconstruction of past events. This aspect seems to be of special relevance for prehistoric and early historical times, where evidence from written sources is scarce or absent altogether. See, for instance, Thompson (1999: 121) on the regular influence of climate upon the populations in Early Bronze age Palestine:

There were few common denominators that affected all people throughout the whole of Palestine, let alone throughout the many regions and periods of the ancient Near East. Nevertheless, historical change was hardly arbitrary. Major common factors, and frequently a direct cause of historical changes were alterations in climate and the availability of natural resources, especially as they affected the efficiency of agriculture and herding. In terms of large-scale historical changes, affecting centuries of development or decay in Palestine's past, the stability of rainfall and access to water resources can well serve us as a bell-wether for other changes and developments across the society.

Current Problems and Debates in History and Neighboring Disciplines

During the second half of the twentieth century, historiography proper and neighboring disciplines, such as archaeology, anthropology, or political science, gave rise to new fields of inquiry, where the concept of laws is involved at least *implicitly*. However, the lessons have been learned, and most scholars today are cautious: you may make generalizations about history – but avoid the “L-word!” These new ways of inquiry are often comparative and include non-western societies within a genuine world history, escaping the Eurocentrism, which had considered Europe and her descendants such as the USA as the central subjects for historiography.

The first of these research agendas after the Second World War may be called the problem of “The Rise of the West” (Gellner 1988; McNeill 1963). It was stimulated by the end of colonialism. How, and why, did Europe differ since around AD 1500 as a pre-industrial civilization, from other candidates such as the Chinese Empire or the Arab civilization? Inherent to these questions are the notions of a *typical* pre-industrial civilization and the aberrant European trajectory that left the typical path. Therefore, historiographic generalizations were at the forefront of these investigations, even though the notion of “laws” was carefully avoided.

Another focus of research from the last quarter of the twentieth century on is the inclusion of ecology in historiography, and here we see again a direct influence of the political agenda of the day on the historic disciplines, this time of the environmental movements. The theme of Ecology and History has attracted a wide range of scholars, including biologists, anthropologists, archaeologists, and historians (Diamond 2005; Fagan 2004; McNeill 2003). One of the first examples of these novel ecological approaches towards history came from Toynbee; the title *Mankind and Mother Earth* of his last book, published posthumously in 1976, can be considered programmatic for the new direction of inquiry.

One example, on the role of climate for Bronze Age Palestine, was given above. In general, the approach of ecological historiography addresses the interactions of societies with their environment, such as the influences of climate and soil or the presence vs. absence of particular plant and animal species (as key resources for the invention of agriculture) on the course of history. Is this a new type of historical determinism, this time with ecology as immutable fate? No, because the laws of ecology are seen merely as boundary conditions, which provide a framework for historical processes. Within this framework, societies make decisions – including some choices which turn out to be catastrophic, when they ignore the ecological conditions of their respective habitats (Diamond 2005). It is important to see the changed role of laws in this approach: in earlier accounts, laws of history were always conceived to emerge from the inner workings of a society. In contrast, the laws of ecology act from the outside, thus creating recurrent features of history.

These two currents from the second half of the twentieth century, large-scale cross-cultural comparisons and ecological historiography, have finally coalesced into a number of similar research programs presented as “world history,” “global history,” or “big history” (Christian 2004; Manning 2003; McNeill 2001; Nazaretyan 2005). Influenced by the political and economic issues of globalization, these approaches address on a global scale the interactions of societies both with each other and with the environment. All the issues noted above, such as the quest for universal rules for historical trajectories, or the stress on the role of ecological laws, are combined here in interdisciplinary and eclectic ways. Not only ecology, but also other disciplines from the natural sciences, such as evolutionary theory, are utilized freely. And – via biology – sometimes even a connection with physics is drawn; in particular, when the notion of *energy* as a universal driving force for biological as well as historical (cultural, technological etc.) processes is employed. An example from David Christian’s work on the history of Eurasia may illustrate this approach and the historiographic generalizations it yields:

The differential in average natural productivity between Inner and Outer Eurasia has been of immense and enduring significance . . . The most obvious consequence of Inner Eurasia’s harsh ecology was low population density. Until recently, most of Inner Eurasia lacked the dense populations that underpinned the urban civilizations of Outer Eurasia. [. . .] The societies that did most to shape the history of Inner Eurasia did so because they evolved successful ways of concentrating or mobilizing the scarce human and material resources of a region of relatively low natural productivity. (Christian 1998: 8ff)

A particularly bold approach within this broader program is that of the population biologist Peter Turchin, who investigates the laws of social dynamics and applies them to the fate of states and empires, addressing issues of growth and decline in terms of population numbers and territorial extension (Turchin 2005). He is exceptional in current historiography insofar as he explicitly uses laws – and even quantitative ones! Accordingly, his work gave rise to a lively debate (Bonneuil 2005; Schrodt 2005).

The problems that may arise from the reluctance of most contemporary historians to formulate generalizations can be seen in a related field, the issue of the “theory of empires.” Again, the stimulus for most workers in this area was present-day politics: the collapse

of the Soviet Empire, the uncertain future of the power of the United States, and the re-emergence of Chinese imperialism (Kalb 2005; Münkler 2005; Pomper 2005). The notion of empire is used frequently in current political debates, in particular with respect to the US: is America already an empire, or merely a hegemony? Is it desirable for a hegemony to strive for the status of an empire? And will the US empire collapse in the future, like the Roman or British empires did in the past? These issues are debated hotly – but in most cases without theoretical foundation and without clear definitions (Münkler 2005). Obviously, to answer any of the above mentioned questions regarding empires, it would be necessary to have a comparative theory, which provides generalizations to reconstruct the historical path of a “typical” empire. The main problem of all hitherto theories of empires has been that they acquire exceptions that needed ad hoc revisions that then led to the fragmentation of theories and their underdetermination in relation to the evidence. Whether or not this is a methodological problem that can be solved by better theories, or a deeper epistemic problem with the historical subject matter, are issues that no doubt will continue to be debated as scholars attempt again and again to realize the old dream of discovering the laws of history.

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Historiographic Objectivity

PAUL NEWALL

The problem of objectivity was called the most important and most baffling in philosophy of historiography (Walsh 1960: 94). Can we say that our beliefs about the past are true (or false), or are they just reflections of our biases? Are some accounts better than others or are they all equally valid? Is historiography a function of what we are looking for, or is the past an object that can be studied scientifically? These dichotomies may well be too crude, and a course may be steered between claiming too much and too little about what historiography can do.

Objectivity in the philosophy of historiography is related to debates about objectivity in general in philosophy. The classical question has been whether what we take to be our knowledge depends exclusively on the object of inquiry and hence is independent of what we think, hope, or expect to find? This perception of objective knowledge is often contrasted with the subjectivity of claims that depend on a person's opinions or presumptions, such as whether a movie was worth watching or yesterday's sunrise was any more beautiful than today's. Philosophers have been debating, for example, whether there is any objective knowledge of values and right or wrong, whether there is a rational basis for ethics. Some philosophers believe that there are moral rights and wrongs, no matter whether we agree with them or not, while others follow Wittgenstein and Hamlet in asserting that "there is nothing either good or bad, but thinking makes it so." Philosophers have spoken of the world and various phenomena as existing objectively regardless of whether anyone is around to observe them, and of propositions as being objectively true because they can be justified in terms that do not rely on observers or their opinions. For example, if bodies attract one another with a force described by Newton's universal law of gravitation then this is so even if no one has discovered it or agrees with it, and was so in earlier times when no one either existed or was able to observe it.

The positivists attempted to identify the meaning of propositions with their truth conditions – or what it would take to verify them as true statements, thus gaining objective knowledge. Therefore ethics which has no truth conditions had no objective value for them. Nagel argued against the positivist and classical concepts of objectivity that there is no "view from nowhere." He argued that objectivity is necessarily limited because some things "cannot be adequately understood from a maximally objective standpoint, however much it may extend our understanding beyond the point

from which we started" (Nagel 1986: 7); by trying to distance ourselves from the object of study to ensure objectivity we actually limit what we can learn about it. He claimed that the challenge is to integrate objective and subjective values, not dispense with the latter in favor of the former. Rescher (1997) tried to develop a conception of objectivity as inter-subjective knowledge that involves neither the world nor truth conditions. Instead, objectivity would mean only that reasoning should lead anybody to the same conclusions given the same evidence or premises. Objectivity then is not avoiding or overcoming a point of view, but overcoming an individual perspective to adopt an inter-subjective human perspective, which corresponds with what we usually mean by objective.

Perhaps the main problem associated with historiography is *bias*, particularly political bias. Beard (in Stern 1956: 324) insisted that "[w]hatever acts of purification the historian may perform, he yet remains human, a creature of time, place, circumstance, interests, predilections, culture," while Fulbrook (2002: 165) noted that historians "are also human beings," implying that they bring to their studies all the foibles and idiosyncrasies that we all carry with us. However, it is far from clear that bias should be a problem if value judgments are an inevitable and *necessary* part of historiographic inquiry.

Objectivity in historiography has been defined in various ways and different authors have different conceptions in mind depending on their philosophical background. However, historiographic objectivity generally concerns the relation of neutrality to objectivity. Arguably if the historian cannot be neutral because she is unable to wholly or even partially eliminate her value judgments, the historian may be unable to approach the objective ideal. Mandelbaum interpreted objectivity as neutrality. He (1977: 146) listed factors that may preclude objectivity such as self-interest, fear, and anger, alongside biases due to class, nationality, religion and race. Yet, Mandelbaum did not think that objectivity was necessary for historiography. If the *aim* of historiography is the reconstruction of the past *as it really was*, it may be possible despite the absence of neutrality, if historians can separate their inevitable values from their research. Mandelbaum (1977: 147), argued that objectivity-as-neutrality provides no "test of whether or not a statement or set of statements is true or false, in history or elsewhere." Miriam Solomon went even further by claiming that plural inevitable subjective elements may be conducive to the discovery of the objective truth in the various sciences.

Standard characterizations of historiographic objectivity by historians tend to take the form of a defence against the charge that historiography is always partisan. The historian wishing to save his or her work from outright dismissal as politically biased in one way or another must elaborate then some form of objectivity. In more recent times this critique has been pushed to its limit by the skeptical relativistic stance that historiographies are just competing stories written to preserve or challenge a prevailing orthodoxy with no claim to veracity.

Ranke's famous injunction that historiography involves finding out about the past *wie es eigentlich gewesen* (usually translated to mean "as it really was") led to the so-called Rankean paradigm that affirms that objectivity was an essential characteristic of historiography (see Iggers 1962 and Novick 1988 for more on the hold this view exerted over historians, particularly in the United States). Even though some historians insisted that historiography is "necessarily a subjective science" (Seignobos 1926:

217), the vast majority of them did not question the presumption of objectivity although there were regional variations (cf. Barros and McCrank 2004: 214–18). Still, Ranke had never intended his dictum to be taken literally or as the basis for historiography. In context, a better translation may be “as it *essentially* happened.” Indeed, he went on to say (1888; Iggers and Moltke 1973: 28–32) that two qualities were required of the historian: affection for other human beings alongside an interest in divining the general from the particular. In this way the historian should maintain an involved subjectivity, according to which he or she should try to understand why people acted as they did but remain as distant as possible (but no further) in order to minimize partiality without losing this empathetic connection. Nevertheless, a simplified version of Ranke’s thought prevailed (Iggers 1983) until recent times, when objectivity came under attack following developments in the understanding of the natural sciences.

Why should value judgments be necessary at all? The idea that a neutral *observation language* could be employed was advocated by positivists like Carnap. Observation sentences should have involved only logical and empirical elements and should have been justified (or not) via direct contact with the world around us. However, contrary to the positivist dreams, the same observation may support conflicting sentences. For example, in the famous *tower argument* appealed to by Aristotelians at the time of Galileo, a stone dropped from a tower fell at its base, providing an observation statement about the world. This was held to demonstrate that the earth did not rotate (i.e., geostaticism) because if it did (i.e., geokineticism) then the stone could be expected to fall some distance from the tower, which would have moved (just as an arrow fired vertically could not cause any harm to the unwary archer if the earth was moving while the arrow was in flight). Galileo argued instead that as the earth rotated so did everything else with it, including the air in which the stone fell, and hence the observation demonstrated exactly what would be expected on the assumption that the earth moved.

This example demonstrates that the meaning of an apparently neutral observation is not given, and that it needs to be interpreted in the light of theory in order to mean anything. Indeed, the notion that observation can take place at all without theory was mocked by Popper (1965: 46) when he instructed a group of physics students:

Take pencil and paper; carefully observe, and write down what you have observed!” They asked, of course, what I wanted them to observe. Clearly the instruction, “Observe!” is absurd. (It is not even idiomatic, unless the object of the transitive verb can be taken as understood.) Observation is always selective. It needs a chosen object, a definite task, an interest, a point of view, a problem.

Fulbrook (2002: 25) reiterated the same argument, this time specifically in regard to historiography:

nothing is simply given: specific questions have to be explicitly formulated by the historian and posed to the sources; archives have to be exhaustively and honestly sourced, sources interpreted with a critical eye, compared with other sources, evaluated in the light of the contexts of production, intended effect and audience, and so on.

N. R. Hanson (1958) and P. K. Feyerabend (1962) were able to show that observation terms are theoretical (“all the way down,” the latter insisted), and the resulting

theory-ladenness was the death-knell for this aspect of positivism and the belief that neutral statements about the world could be made. This realization was later extended when it was noted that observation is not simply a matter of perception; instead, it is a cognitive achievement that involves perceiving *that* something is or is not the case. A number of papers in neuroscience and related areas by Churchland (1991) expanded on this insight, arguing that if our brains held no hypotheses about the world when encountering it (or, alternatively, if these hypotheses were fixed) then we would not be able to *learn* from new information. This is to say that observations and experiences have to be *interpreted* to be meaningful and it is this unavoidable involvement of a theoretical dimension – even at the level of brain functioning – that constitutes theory-ladenness. Trapped in this “hermeneutic circle,” the historian or scientist alike would have no way of “achieving a standpoint entirely outside all paradigms, from which they can, in some Olympian way, be judged” (Fulbrook 2002: 181). Mandelbaum (1977: 187) expressed similar sentiment by saying that “if it were true that when a historian attempted to explain a particular state of affairs he had to start out from a position of total ignorance, he would not know where to look for the likely antecedents of that state of affairs, and he would have no tentative hypotheses concerning how any two factors in a social situation are likely to be related.”

If theory is an unavoidable part of inquiry then the identification of objectivity with neutrality must be given up. The charge that historians are biased thus becomes not a criticism but what makes historiography possible in the first place. Unless historians bring their preconceptions to bear on their material, they would be unable to learn anything from it and would be limited to the Sisyphean task of providing an inventory of what they had found while knowing that it would be meaningless without the addition of an interpretative dimension that could not be neutral. This is not far from what Ranke had advocated in the first place.

Objectivity and Methodology

Historians reacted to the demise of objectivity in the sense of neutrality and observation sentences in much the same way as scientists: if they could not start without preconceptions and could not attain objective conclusions, at least they should identify a means of proceeding that would *minimize* distortion and get as close to the truth as they could. This methodological turn was summarized by Evans (1997: 223):

While historians are certainly swayed, consciously or unconsciously, by present moral or political purposes in carrying out their work, it is not the validity or desirability of these, but the extent to which their historical arguments conform to the rules of evidence and the facts on which they rest, by which they must stand or fall in the end. In other words, they have to be objective.

Here there are two requirements: firstly, that historiography be an empirical endeavor (“the facts on which they rest”); and, secondly, that the historian “conform[s] to the rules of evidence.” Since even skeptics do not deny that historiography is based on sources, objectivity is therefore defined methodologically. Hayden White (1978: 126)

complained that “the so-called ‘historical method’ consists of little more than the injunction to ‘get the story straight’ (without any notion of what the relation of ‘story’ to ‘fact’ might be) and to avoid both conceptual overdetermination and imaginative excess (i.e., ‘enthusiasm’) at any price.” From a similar perspective, Jenkins (2003: 14) listed “twenty-five possibilities” in support of his claim that methodologies are plural, implying that there are far more. The manifesto resulting from the international *History Under Debate* forum resulted in a list of eighteen “methodological, historiographical, and epistemological proposals” (Barros and McCrank 2004: 43–54).

Generally, the requirement that historians apply a sense of integrity to the evidence they are faced with tells us little about whether or not there is a single correct way to do so. Indeed, that a plurality of methods is possible given the same empirical base is perhaps a strength of historiography just as it is of science. The requirement that any single approach be adopted is tantamount to a methodological strait-jacket (see in this connexion the arguments of Mill 1975 and their more recent application by Feyerabend 1981: 139–45).

The problem with the alleged reliance on facts is that “they” do not speak for themselves, as historians have long realized. In recent years historians have debated just how much “they” *do* allow us to say, often in response to the idea that since no one really knows what happened in the past, all accounts are equally valid. Fulbrook (2002: 193) remarked that “[i]t is possible to design ways of testing hypotheses, of checking the validity of particular interpretations or explanations, the accuracy or otherwise of particular statements of fact, and the plausibility of statements about relations between facts” such that historians can be *wrong*, so the historiographies we can write are *limited* by the traces of the past we have available. On the face of it, this suggests that, as new evidence comes to light, further constraints are placed on the accounts we have, falsifying some of them and allowing others to become more accurate.”

The difficulty is that as Duhem and Quine noted, there are in principle an infinite number of theories compatible with any finite data set, an argument called the *under-determination of theory by evidence*, a classic example of which is the clash between the Ptolemaic geocentric and the Copernican and Tychoonian heliocentric systems (cf. Kuhn 1992). Both systems seem to fit the same body of evidence, and as Kuhn argued, even cognitive values cannot help distinguish them as one system ranks higher than the other according to some cognitive values and lower according to others. Be that as it may, in practice we are not faced with a multiplicity of competing theories, but even discounting easy targets like Holocaust denial, it is plain that different historians use the same evidence to develop different historiographies of presumably the same events. If the question is one of choosing between them, or deciding which is more objective, then insisting on conforming to the facts and an honest application of the rules of evidence is insufficient. Historians like Elton (1997: 179) have reasserted that we “are firmly bound by the authority of our sources (and by no other authority, human or divine)” without seeming to appreciate the problem.

A simple – or naïve – form of evidentialism or empiricism is then untenable. All historiographies are not equally valid because some are supported by the evidence, available traces of the past, better than others, but this evidence does not allow us to choose between conflicting accounts that are empirically adequate to the same degree. Appleby et al. (1994: 10) claimed that objectivity would have to be “defined anew as

a commitment to honest investigation, open processes of research, and engaged public discussions of the meaning of historical facts,” allowing that this understanding “concedes the impossibility of any research being neutral (that goes for scientists as well) and accepts the fact that knowledge-seeking involves a lively, contentious struggle among diverse groups of truth-seekers” (1994: 254). Fulbrook, however, complained that the “relativist implications” of celebrating such a democratic, pluralist approach had not been considered, particularly the implication that “only political sympathies can ultimately adjudicate between ‘better’ . . . and ‘worse’ . . . accounts” (2002: 9). This is similar to Feyerabend’s criticism of Lakatos’ *Methodology of Scientific Research Programmes* (see Lakatos (1980) and Feyerabend (1978: 181–214)), which objected that without explicit methodological guidance for when a programme should be rejected there is no reason why a supporter should not continue to hold (and hopefully develop) it. In particular, it is far from clear that “the truth will out” as a result of open dialogue, rather than “success” being a combination of political power or rhetorical skill.

Relativism and Beyond

Many historians still consider dropping truth and objectivity from our conception of historiography to be disastrous. Elton (1991) insisted that it is “only by providing as truthful an understanding of the past as we can obtain that we can offer to the present a past which can be useful to the present, a past from which it can learn.” Questioning “the very notion of a truth in history,” he said, “leads straight to a frivolous nihilism which allows any historian to say whatever he likes.” As we have seen, though, it is more accurate to say that a historian may say “whatever he [or she] likes” *within the bounds* enforced by the evidence, the traces of the past we have, which is exactly what we would expect and certainly no more or less than scientists may do with their evidence. For Geras (1995: 110), “[i]f there is no truth, there is no injustice,” which ignores the possibility that moral rights and wrongs could be intersubjectively agreed without requiring an ultimate arbiter such as truth. Evans (2002) has argued along similar lines:

If we don’t believe it’s possible to distinguish between truth and falsehood, then we have no means of exposing racism, anti-Semitism, and neo-fascism as doctrines of hate built on an edifice of lies; indeed we have no real means of discrediting them at all. We can say of course that we disapprove of them in moral and political terms, but neo-fascists can just put forward opposing moral and political arguments of their own in response, and in the end there are no objective criteria by which we can choose between the two positions.

This kind of argument from consequences is supposed to establish the necessity of keeping some form of objectivity, even though the truth value of a proposition does not depend on its consequences. Musgrave (2003: 155–6) suggested instead that without a correspondence form of truth between history and historiography,

we must rely on our ethics, generosity toward and respect for the views of others, personal integrity, political awareness, and as strong a sense of social responsibility as we can muster.

We make choices every day without needing to resort to “objective criteria” and we argue that ideas are misguided for a variety of reasons. In any case, if a racist can develop an account of the past that accords with the available traces but supports his or her racism, then the respect the historian must show towards the evidence has been satisfied. Somehow, distaste for racism is used to imply that empiricism can – and should – do more than it is able to.

Scientific Historiography

Tucker (2004) has argued that historiography is scientific. In his view, historiographic accounts are abductive inferences to the best explanation, characterized in terms of Bayesian probability theory. Usually, historiography supported by a “uncoerced, sufficiently large and uniquely heterogeneous consensus” is the best explanation of the evidence. The best explanation of this sufficiently large and uniquely heterogeneous consensus is that historians within that consensus share as an objective intersubjective knowledge of the past. Tucker applied this approach to interpret biblical criticism, classical philology, comparative linguistics and evolutionary biology (2004: 46–91), before providing a formal justification of his Bayesian methodology (2004: ch. 3). While agreeing that underdetermination is a problem for naïve (historical) realists, he insisted that “the evidence may still be sufficient for eliminating many improbable hypotheses” (2004: 142) and that

the theories historians use and the available evidence are incomplete, insufficient, for determining parts of historiography. To reach an opinion on such historiographic issues, historians add controversial theories to the theoretical core and the evidence they agree on. (2004: 143)

From this perspective, historians can agree that their accounts of the past are limited by the evidence at hand while accepting that a multiplicity of possibilities remains tenable. In the absence of new evidence they must argue for these on extra-empirical grounds, which can be the narrative dimension that so-called postmodernists point to. Historians can even make predictions on the basis of their accounts of where new evidence is likely to be found or what it can be expected to indicate, and so a form of confirmation may be possible. However, there is no need to yoke this to a conception of truth beyond Tucker’s inference to the best explanation, where the historian argues that his or her account is more likely – in probabilistic terms – to be the case, rather than a simplistic appeal to objectivity and asking that the evidence does more than it can. We would expect a true account to be a good explanation but the converse does not hold; for example, phlogiston theory was very useful but was eventually discarded as false. However, “[i]f historiography is the best explanation of the historical evidence, it does not imply that it is a representation of the past” (2004: 257), although Tucker argued that the best explanation of this success is that we *do* have knowledge of the past.

Approximating the Truth about History

The final issue to consider is whether historians can say that their accounts of the past are *truthlike*; while we may accept that no one can get at the whole truth, can we nevertheless say that one account is closer – or more verisimilar – to it than another? The problem with degrees of verisimilitude is that they appear to beg the question: how can we determine how close we are to the truth without needing to know the truth in the first place, rendering the truthlike alternatives moot? We can safely say that if the question is “what is twice two?” then the answer “five” is closer to the truth than “eighteen,” but how can we do something similar for differing historiographies of an event?

Early attempts at understanding truthlikeness, such as Popper’s (1965), were based on the *content* of theories. These foundered because false theories were shown to be no further away from the truth than any other (see Tichy 1974 and Miller 1974). Similarity approaches (see Niiniluoto 1987 and Oddie 1986) involve measure metrics and rely again on the true conclusion being known. Tucker’s Bayesian approach provides a means to assign greater or lesser probability to a theory, although the prior probabilities required in Bayes’ theorem might be disputed (cf. the discussion of fidelity as a means of “bootstrapping” evidence to act as a measure of the likelihood of other evidence (2004: 119–34). At the very least we can say that truthlikeness is a difficult concept and considerably more work is needed to apply it to historiography if we want to give any content to robust assertions that one account is closer to the truth than another. Nagel (1986: 9) nevertheless cautioned that we should not abandon the search for truth even if we cannot be sure we can attain anything approaching it, and Feyerabend (1981) provided a sophisticated methodological argument to this effect, claiming that empirically adequate theories provide no motivation to continue developing them. The realist, on the other hand, believing his or her ideas to be but approximations of the true state of affairs, is inclined to try to improve them ceaselessly.

Conclusion

Objectivity in historiography functions as an ideal that motivates historians to remove or reduce the influence of extra-empirical factors, but only insofar as they are able to do so. Although accounts of the past are based on the evidence historians have at hand and hence limited by it, not all possible historiographies are equally valid. Historians accept that we cannot say that their accounts are *tout court* true, but it is also not clear that we can even say they are *approximately* true.

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Realism about the Past

MURRAY MURPHEY

Traditionally the problem of realism has been seen as the question of whether or not universals are real. This traditional dispute has not been the focus of attention among historians and philosophers of historiography and history. What has exercised those interested in historiography is the question of the reality of past persons and events, of social “forces,” social structures, and cultures. “Realism” in this sense may be taken to mean that the referent actually exists or has existed at some time in the past. Various types of realism may be distinguished: commonsense realism, representative realism, analytic realism, and materialism. They are opposed by philosophers who call themselves “anti-realists.”

Commonsense Realism

In historiography, the term “realism” means the reality of the past and of the persons and events of the past. The issue of the reality of the past itself was raised by Russell (1921: 159):

There is no logical impossibility in the hypothesis that the world sprang into being five minutes ago, exactly as it then was, with a population that “remembered” a wholly unreal past.

Russell’s point was that there is no logical connection between events at different times. But aside from Russell no philosopher of history or historiography has seriously questioned the reality of the past. What has been challenged is the extent of the “deep” historical past – that is, the past beyond the reach of living memory. There are a great many people, some of them historians, who accept the account of creation given in the Book of Genesis and its traditional interpretation and therefore believe that human history goes back only about six thousand years. Most historians however accept the thesis of astrophysics that the world is billions of years old and the testimonies of paleontologists that human beings came into existence about a hundred thousand years ago. Most of this period lies before the invention of writing and is often referred to as “pre-history.” But the division between history and pre-history is entirely arbitrary and

is becoming increasingly dubious. Current researchers are using DNA to trace the migrations of peoples over a period of some forty thousand years and no one questions the labeling of such work as historiography. Similarly, archaeologists, who are also historians, deal with human remains that are dated long before 4004 BC – the date on which, according to Bishop Usher, God created the world.

Representative Realism

The reality of the persons and events of the past is universally accepted by practicing historians. Even the question of their reality is never seriously raised. Only rarely do historians even consider these issues in metaphysical terms. The reality of the past is basically a philosophical question. What gives bite to this question is the fact that the persons and events of the past no longer exist, and so the question arises as to what we can know about them and how we can know it. Where their reality is asserted as a presupposition or assumption of historiography, we have a form of representative realism. The basic claims of this position are (1) there is a real past with real people and events, (2) we can access that past through sensory experiences of documents and artefacts that survive to us, evidence that preserves information transmitted from the real past, (3) from this information, if it is adequate, we can reconstruct what happened in the past, and (4) our reconstructions may be incomplete, but given adequate data they are approximately true. The challenge for this position is to explain *how*, in which sense of truth, our reconstruction can be true? The traditional answer was the correspondence theory of truth. The object of historiography is claimed to be to describe “what really happened” in the past, and this is taken to mean that our ideas of the past must correspond to the actual occurrences in the past.

The problems of the correspondence theory of truth are well known, but they are particularly severe in attempting to elucidate a correspondence relation between historiography and history. The correspondence theory of truth asserts that our concepts and statements “correspond” to external things. No historian denies such a theory as applied correspondence to historiographic evidence – the documents, ruins, artefacts, etc. that have survived to us from the past. The problem for philosophers of historiography is whether or not their statements and theories can “correspond” to historical persons and events, or “facts,” in the past, since these persons, events, and facts no longer exist. Most historians accept some form of the correspondence theory, though usually not in the naïve form of a copy theory, and so do many philosophers of historiography. Thus Maurice Mandelbaum stated his position:

Caesar crossed the Rubicon” is true if the relation which it expresses did in fact hold of the objects with which it is concerned, if the action which it states was done was actually done. The correspondence theory of truth means this and nothing more: that a statement made “concorde” with the fact with which it purports to deal. (Mandelbaum 1938: 186)

This is to say that there must be a one to one correspondence between the entities and actions asserted to have existed in the historian’s theory and the persons and events

that actually existed in the past. But how one can prove that this correspondence obtains remains a major problem, not to say a mystery.

A variant of this position also asserts representative realism of the sort described above but substitutes for the correspondence theory a “correlational” theory of truth. Psychologists and anthropologists have shown that our perceptions of the external world are influenced by the peculiarities of our perceptual apparatus, by our culture, and by our needs, interests, and desires, as well as by external stimuli. The correlational theory tries to accommodate these findings by holding that our statements about the past are true “if there *were* things in the world which *would have* produced perceptions of the appropriate kind under normal conditions for members of the culture which made these descriptions” (McCullagh 1998: 30). The test of truth of our statements in this theory is whether or not the future experiences they implied actually occurred as predicted. These predictions form the truth conditions of our statements.

The use of counterfactuals is particularly alluring to historians and philosophers of historiography who hold pragmatist or verificationist views. Just as on these theories the truth of a statement consists in the success of all the predictions it entails, so it seems natural to say that the truth of a historiographic statement consists of the assertion that if the event referred to in the statement had occurred, then certain other events would have occurred. This design is often used to assess the significance of historical events by holding that, if the event in question had *not* occurred, then certain further events would *not* have occurred. The plausibility of such methods shows the importance of the problem of counterfactuals for historiography and the inadequacy of the material conditional for such purposes (1998).

The pervasiveness of the belief in a real past combined with a correspondence theory of truth is surprising; it appears where one might not expect it. F. R. Ankersmit (1983) has been an advocate of what he terms “Narrative Idealism” and is generally regarded as a leading postmodern theorist. But Ankersmit does accept the existence of a real past with real people and events, and holds that historiographic statements that are singular constative statements can be true if they correspond to past reality. On the other hand, he held that statements involving such concepts as “the Renaissance,” “the Enlightenment,” “the Cold War,” and even “Germany” could not be true in his system. These are linguistic structures used in the narrative presentation of history, but they do not correspond to any past reality. Ankersmit’s truth conditions are so exacting that most historiographic statements cannot satisfy them.

Analytic Realism

Such theories have not satisfied all philosophers of historiography. One of the most widely read is Thomas Kuhn. Kuhn describes himself as a “post-Darwinian Kantian” (Kuhn 2000: 7). Like Kant, he holds that the world of our experience is a phenomenal world, but in place of the Kantian categories he substitutes “paradigms” – concrete scientific achievements including theory, law, instrumentation, and application – that serve as exemplars, attract scientists away from competing theories, and leave multiple problems to be solved by their adherents. Such paradigms structure the phenomenal world for their followers. But Kuhn also admits the existence of a real world beyond

the phenomenal one. If reality were only phenomenal, then barring errors of logic, whatever the paradigm theory implied would be true. But Kuhn's theory of how science develops requires the existence of anomalies – predictions that turn out to be false and problems that the paradigm cannot solve. Anomalies require the existence of a real world that can invalidate the paradigm's predictions. But this real world Kuhn holds to be unknowable by us; like the *ding-an-sich* we know it is "there" but we never know what it is really like. Accordingly for Kuhn, truth, if a legitimate notion at all, is relative to particular paradigms (Kuhn 1970: 170–3).

An even more skeptical view has been presented by Leon Goldstein (1976). According to Goldstein, there is a real past with real people and events, but he believes this reality is inaccessible to us. History for him is "constituted" by historiographic research, on the basis of available evidence. He claimed that historians "constitute" historical persons and events. The objectivity of this process is shown by the fact that historians largely agree about what has happened in the past. But the picture of the past so constituted does not correspond to the real past; since the real past is unknowable, no such correspondence can be proven. Are historiographic accounts then true? Goldstein's answer is ambiguous: on the one hand, he seems to endorse a coherence theory of truth; on the other he shies away from any claim that the constituted picture is true. He seems to regard the question of the truth of historiographic statements as a problem yet to be solved, but insisted that any answer must come from the analysis of the process of historiographic research itself.

Problems of this sort have led to an alternative view that holds historiographic accounts to be theories that explain the data (evidence) of history – that is, the presently existing artefacts and documents that form the evidence. If historiography is an empirical discipline, as most historians believe, then it must be based on observations, and the only things that a historian can observe are present data. On this view, so-called historical facts are theoretically laden and thus form parts of historiographic theories; the observational base is provided by observations of the evidence. From an epistemological point of view, the past and its contents so postulated are constructs, but ontologically they are real if the theory that postulates their existence is true, whereby a theory being "true" is meant that it explains the relevant evidence, continues to do so as new evidence is discovered, and is consistent in itself and with other accepted theories. Our currently accepted theories are held to be our best estimates of the truth. This position abandons the metaphysical postulate of the reality of the past and its contents, but it is a realist theory since true historiographic theories describe the real (Murphey 1973).

Collingwood was a realist but of a special kind. He takes the idea of history to be a *a priori*, which he equates to innate. It is something intrinsic to the human mind, and our efforts in historiography must be to satisfy this *a priori* principle by creating a coherent picture of the past. With respect to past events, he distinguishes what he terms the "outside" from the "inside" of the event. The outside embraces whatever can be described in terms of bodies and their movements; the inside is what can be described in terms of thought (Collingwood 1961: 213). For Collingwood, the subject matter of historiography is human thought; there can be no history of anything that is not thought (1961: 304). Thus there cannot be a history of nature or of the outside of events. He does not deny that nature and events not embodying thought are real,

but he holds that there cannot be a historiography of them since they cannot be re-enacted. Collingwood asserts that what one person has thought can be re-enacted by another; what Plato thought can be re-enacted by the historians studying Plato. By “re-enactment” he does not mean “copy”; for him thoughts are universals that can exist in multiple minds. What the historian of Plato has in his mind when he re-enacts what Plato thought is identical with what Plato thought.

Collingwood distinguishes reason from other mental phenomena that are specific to the individual and cannot be re-enacted. What he means by “thought” is self-conscious, reflective, purposive thought. When the historian re-enacts Plato’s thoughts, he can at the same time critique them, but it is Plato’s actual thought he is critiquing. For Collingwood there can be historiography only of what can be re-enacted. Many critics of Collingwood have taken him to hold that we have an intuitive access to the thoughts of past figures. But Martin has argued persuasively that this is not what Collingwood meant (Martin 1977: ch. 3). Rather, Collingwood held that from present evidence we must reconstruct what actors in the past thought, and that our methods of doing so are not different from those generally used in historiographic research. Collingwood is certainly less than clear on this question, but he does explicitly hold that what the historians has in mind is identical with what his historical subject thought.

This conception of historiography is not as narrow as one might think. Collingwood believes there can be historiographies of politics, wars, economics, morals, religion, science, and art – in fact, of any activity in which human thought plays a significant part. Thus in writing about the history of the Battle of Austerlitz, the historian can think the thoughts Napoleon had during the battle; he can, as it were, work his subject’s calculation.

Clearly, Collingwood is a realist in several senses. He believed that there were real events and people in the past. He also believes in the reality of universals in the form of rational, self-conscious, purposive thoughts that can be instantiated in multiple minds. In this, Collingwood’s position is more extreme than that of most historians and philosophers of history and historiography. The classic debate over the reality of universals is not often raised by either historians or philosophers of historiography, but it does sometimes appear. Ankersmit (1983) apparently believed that only historical particulars exist since he held that only singular constative statements could correspond to past events or persons, and denied the possibility of truth or falsity to statements involving concepts such as “the Renaissance.” These concepts in his theory represented “narrative substances” which are sets of singular constative statements, so he clearly accepted the reality of sets. But Ankersmit’s chief interest is in the narrative presentation of history rather than in historiographic research.

Materialism

The influence of Marx is still sufficient to require comment on this work. Marx is a materialist, but his materialism is not reductive; he does not regard reality as reducible to the clash of atoms. Nature for him is material, and to use modern terminology, mind is an emergent property of material systems. Both nature and mind are real. But

reality for Marx is not simple; it is complex, differentiated, and stratified. Because of this structure of reality, contradictory statements can both be true of reality. There is therefore a need for a logic of development that can contain opposites, and Hegel's theory of dialectic is the method he adopted. Nevertheless, Marx regarded his theory as belonging to the natural sciences and as having the same sort of warrant that they enjoy.

Marx entertained no doubts about the reality of persons and events, past or present, but he also regarded as real the forces and entities that are asserted to exist in his theory. For him, the fundamental determinants of social life are the productive forces – those which produce the goods that sustain life. These include not only technology but also labor. Productive forces give rise to productive relations, such as the ownership of machines by individuals. This economic base of society determines the “superstructure” of political and social relations and institutions, of philosophy, art, and science. But this determinism is not one-way; there is a feedback between the superstructure and the base. The resulting social structure is however unstable because the productive forces constantly change as new processes and technologies are invented, so the output of the productive forces constantly increases. Hence the changes in the productive forces result in conflict with the established relations of production. The result is the division of the society into two classes: the bourgeoisie who purchase labor and the proletariat that sells labor. Resistance to change by the bourgeoisie will deepen the division until the workers recognize their common plight and revolt. Marx sees human history as governed by these processes.

Anti-realism

The traditional opponent of realism was nominalism. But at present the role of opposing realism has been taken over by “anti-realism” The issue here is not the status of universals but of unobservable events and objects. Probably the best known champion of this position is Bas van Fraassen (1980). His position is an extreme form of empiricism that denies the reality of anything not perceptible by our senses. This position was put forward in opposition to the claim that the postulated but unobservable entities of science, particularly physics, are real. In van Fraassen's view, theories that postulate such entities are useful calculating devices and can and should be used, but their purpose is to generate consequences regarding observables. For him, such theories are black boxes that take as inputs statements involving only observables and have as outputs only statements regarding observables. These empirical consequences of the theories we should accept as legitimate, but the contents of the black boxes – the theories themselves with their postulated entities – should not be regarded as true or as telling us anything about reality.

The application of this view to historiography leads to serious problems. Since past objects and events are not now available for sensory examination, they cannot, on van Fraassen's theory, be real. Present historiographic data or evidence can be observed and so is real, but it is not clear in what sense they can be said to be “historical” since the past no longer exists. The argument that historiographic theories have as inputs and outputs only observational statements regarding data eliminates history entirely

and leaves the data unexplained, since real effects cannot be due to unreal causes. Nor does van Fraassen's position permit the claim of "observability in principle" since making such observations is logically impossible as travel back in time is a self-contradictory notion. Whatever advantages van Fraassen's view possesses for understanding physics and its relation with nature, it would annihilate history.

A much more plausible theory that is also opposed to historical realism is Michael Dummett's (1978). Dummett rejected realism about the past. But in his more recent "Dewey's Lectures," Dummett (2004) expresses dissatisfaction with his earlier view and sets out what he calls a "justificationist" position. Dummett holds a theory of meaning derived from intuitionist views of mathematics according to which a statement is true only if there is an effective way of constructing a proof for it. When this theory is applied to non-mathematical statements, it requires that the statement be "verifiable," which he takes to mean that there must be empirical grounds for asserting it. Such verifications may be "direct" if the entities involved are now available for observation, or "indirect" if the truth of the statement can be inferred from observable evidence.

How is this theory to be applied to the past? Dummett first specifies who the possible observers are. They are any human beings that are members of the human "community" – that is to say, all persons living or dead. So if we have a statement from General Grant describing Sherman's conduct at Shiloh, we can accept that as direct evidence concerning Sherman. Similarly, one presumes that the post-crucifixion appearance of Jesus must have actually occurred since it was witnessed by his disciples (Luke 20: 19–23). But how on this view are we to deal with events or persons for whom no verifying descriptions are known to exist? Dummett's answer is that the occurrence of such events (or persons) is still assertable if it is the case that, had someone been at the appropriate place at the right time, he could have observed such an event (or person). Dummett is by no means the only philosopher to suggest the use of counterfactuals in historiography, and many would agree with his use of it provided we already know that such an event (or person) did exist at that time and place. If we do not already have this information there would seem to be no constraint on the past events (or persons) we might imagine that would indeed have been observable by any well placed observers, had they occurred. Thus one can imagine aliens landing on earth forty thousand years ago; surely that event would have been observable to one so situated that he could watch the proceedings. Obviously, this is not the sort of thing Dummett had in mind, but he did not specify the constraints that would be necessary to make this a workable doctrine.

Dummett admits that his "justificationist" position is a step away from his earlier anti-realism and toward realism, but it does not go the whole way. It does not require that states of affairs must exist that verify or falsify all the statements we can make about the past. But it does require that whatever statement is true must be always true. Thus Dummett holds that if, at a certain time and place, he scratched his right ear, then it is true that at that time and place he did scratch his right ear. But suppose that after a year he has forgotten about his scratching and no one else remembers it either or made a record of it. Then the statement that at such and such a time and place Dummett scratched his right ear would no longer be true, and this Dummett (2004) holds to be an absurd conclusion. But it is not absurd. Historians assume that past persons behaved like normal human beings, with itches to be scratched. But if one

year from now we have no evidence that Dummett scratched his ear at a given time and place, then the statement that he did so is not assertable as true. That does not prove that it is false; it just means that our knowledge of the past is incomplete, as it obviously and regrettably is.

The fundamental problem with Dummett's theory is his definition of the community as including all people who ever lived. If we must give equal credence to all eyewitness accounts from the past, then we will have a mass of statements that include testimonies about miracles, witches, angels, giants, divine interventions, and so on. Observers in the past are not members of our community but of their own historical communities whose conceptions of the world were very different from ours and who "saw" things that we today do not believe ever existed. Historians do not take such past statements at face value but regard them as data that have to be explained, and part of that explanation requires explanation in terms of the beliefs current in that culture. The visions of saints provide observational accounts of supernatural beings that, were these statements made today in our present culture (community), would be grounds for questioning the sanity of the "observers." Cultures change over time, and what was once acceptable may not be so now.

There is however a different form of anti-realism that has gained popularity over the last few decades. Historical works, by and large, are presented in narrative form. This is of course not true of all historical works, but it is true of most. The reasons for this use of narrative are fairly obvious. A narrative usually consists of a sequence of temporally ordered statements that are matched to a similarly ordered sequence of events. Since historians usually deal with change over time, the narrative offers a fairly adequate means for presenting such sequences of events and the changes that occur in the subject. Further, since the direction of causation is from past to present, narratives can accommodate causal chains without difficulty.

The problem arises, as Louis Mink (2001: 219) pointed out, from the fact that narratives are imaginative creations of their authors and these linguistic structures may not "correspond" to historical reality. Furthermore, literary theorists have been much concerned of late with the question of the role of narrative discourse in knowledge. If narratives are taken to be cognitive instruments, then the issue becomes what sort of cognitive structures they are, and to what degree they are true. The most influential American historian to have taken this approach is Hayden White, whose *Metahistory* (1973) has been the outstanding work in the field. White is very frank in saying that historiographic theories are not true in any usual sense; they have, rather, a form of "poetic truth." In White's view, the "evidence" that historians use – documentary or non-documentary – does not yield the historian an adequate picture of his subject. They must therefore be transformed by the historians' "poetic" insight. As White portrays it, this imaginative reconstruction of the past is "precognitive" and "pre-critical." It is achieved by bringing to the interpretation of the data certain literary categories – those of metaphor, metonymy, synecdoche, and irony. As White puts it

In the poetic act which precedes the formal analysis of the field, the historian both creates his object of analysis and predetermines the modality of the conceptual categories he will use to explain it. (White 1973: 30–1)

These categories determine what the plot structure will be – romance, tragedy, comedy or satire; the form of the explanation, which will be formist, organicist, mechanist, or constructivist, and the ideology of the work – anarchist, conservative, radical, or liberal.

There is a peculiarly Kantian ring to White's doctrine. The evidence, if it is to serve the historians' purpose, must be poetically synthesized by the use of literary categories. Analysis will then find in the resulting synthesis what the categories have put there, and all the major characteristics of plot, explanation, and ideology will follow from this original synthesis. The result will be a "poetic" work of historiography, but no truth claims are relevant to it. Telling the truth about the past is not, as White sees it, possible for the historian. He can at best achieve a poetic and imaginative tale about the past. This form of "literary historiography" is clearly not what most historians aspire to or what philosophers of historiography have thought should be done, since it abandons the attempt to write true historiography, but it is sign of what the wedding of philosophy of historiography and literary theory has produced, a reduction ad absurdum.

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Anti-realism about the Past

FABRICE PATAUT

Anti-realist philosophers of historiography who are sometimes called *constructionists* claim that historiography is not a representation of the past, but a construction in the present (Goldstein 1976, 1996). Constructionists regard historiography merely as something historians construct in the present. This construct need not be arbitrary; statements about it can be an interpretation of present evidence. Since the past is inaccessible or statements about it cannot be asserted, all we have is what historians tell us, further ontological assumptions about the past are to be taken at one's own risk. By contrast, *historiographic realists* claim that historiography is a representation or reflection of history; historiography then, is largely a true account of the events of the past.

There can be three types of anti-realist constructionisms: *determined constructionism* regards historiography as having established a consistent family of theories and methods for the interpretation of evidence. Consistent application of these methods yields determined interpretation of evidence according to strict professional norms. Determined constructionists would balk at making any ontological claims about the past. Determined constructionism denies that determined historiography is a true representation of the past or anything more than the most plausible interpretation of the evidence. Historiographic realism and determined constructionism agree in their epistemic descriptive analysis of historiography and on the determined relations between historiography and evidence; but disagree in their ontological interpretation of the relation between historiography and history. Second, *underdetermined constructionism* holds that there are several historiographic methods of interpretation of evidence. Though different interpretations lead to inconsistent results, there are no independent criteria to decide among them. Inconsistent historiography cannot be a representation of history because historical reality must be consistent. Third, *skeptical constructionism* holds that there is no privileged set of historiographic propositions because they are all equally indeterminate. Historiography is ontologically and epistemically indistinguishable from literary fiction.

In the last forty years of the twentieth century, anti-realism has come to the fore of philosophical discussion largely as a result of the work of Michael Dummett. I take it for granted that anti-realism consists of a cluster of semantic theses about meaning and truth in general and in particular about the historical past. My main objective is to assess

the prospects of anti-realism about the past, when the past is taken to be the proper topic of historiography, when historiographic studies in their various aspects are taken to constitute objective truth. Finally, I consider anti-realism in the context of denial of the possibility of historical objectivity.

Realism vs. Anti-realism in the Semantics of Mathematical Language

Contemporary philosophical anti-realism was formed and, indeed is at its strongest, as an interpretation of mathematics. Therefore, before embarking on an examination of an anti-realist interpretation of historiography, a few words about the origins of anti-realism in the philosophy of mathematics are appropriate. A realist about a class of mathematical statements, such as any equation, must hold two theses: first, that their truth or falsity are independent of our abilities to either prove or disprove them, and that their meaning consists in their truth-conditions. If, furthermore, the realist endorses the (admittedly controversial) idea that a theory of meaning for a language is a theory of what the speakers' understanding of that language consists in, he must also be ready to argue that to understand or grasp the meaning of statements couched in the language of mathematics, amounts to a knowledge of the conditions which must be fulfilled for such statements to be true. Dummett's challenge to realism then comes to this: the realist is urged to show that the two-fold claim about meaning, truth and understanding holds, even when we are *not* able to recognize whether the truth-conditions of statements of mathematics obtain or not, i.e., when these statements are *pro tempore* undecided.

Although it has been much refined and amended in recent years in the work of Crispin Wright (1987, 1992) and Neil Tennant (1997), the anti-realist position championed by Dummett and his followers is provisional: the anti-realist proposes that until a non question-begging argument has been provided in favor of the idea that mathematical truth may transcend our abilities to find, or construct, proofs for mathematical statements, we should replace *truth-conditions* by *provability-conditions* as the central concept of a theory of meaning for the language of mathematics. Therefore, an anti-realist interpretation of mathematics is the most plausible. (For an exposition and critical discussion of these issues, see Hale 1997.)

Anti-realism about the Empirical Realm and, in Particular, about the Past

According to Dummett, verifiability-conditions are the most likely sources of meaning for any language we may use to express claims about the natural world, other minds, or the past. These will be the conditions which must be fulfilled so that the users of the empirical language will be able to verify, or check, or justify in some appropriate way, the truth of their claims. This implies that, contrary to the truth-conditions of a realist semantics, verifiability-conditions will not be transcendent or inaccessible. Eschewing

the notion of truth, or replacing it with an epistemic assertibility or verifiability, should guarantee that speakers are able to recognize that the assertibility or verifiability-conditions of their claims obtain when they do, and do not obtain when they do not.

When we attempt to justify claims or beliefs about the past, whether or not we are successful, we use evidence in the form of documents, testimonies, and memories, private or collective. Justifications or warrants, for the occurrence of past events share their characteristic properties with warrants for other kinds of empirical claims, e.g., claims about the occurrence of secondary qualities such as colors and textures: they are gradual, partial and defeasible. We may be *more or less* justified in claiming that a past event, or chain of events occurred at a certain time typically because the scope of evidence that increases their likelihood may be broader or narrower or become broader or narrower in the course of research. It may also turn out that further contrary evidence will lead us to retract our claim or stronger new evidence for alternative claims would increase our degree of belief. It could also be that the available positive evidence will weaken to the point where it cannot increase the probability of the belief anymore. Secondly, the evidence we may possess to justify a claim may be only for a particular aspect of the event. Thirdly, whatever historiographic claims we make about what happened in the past turn out to be better confirmed when we are able to check them against as many different, independent, and varied sources of evidence as possible. Such variety, or *consilience*, in the origin and nature of our warrants is an epistemic virtue, and the incremental nature of empirical warrants is a function of consilience and independence.

These features, either individually or combined, cast doubt on any *indefeasible* certainty concerning any contingent or empirical statement. Past tensed statements constitute one privileged class that will always fall short of the standards of conclusive verifications. Such statements will not be “superassertible” in Wright’s sense (Wright 1987: 295–302). Their assertibility will not be stable following reassessments of our knowledge of the past, or after further accretions to that knowledge.

The warrants we may gather for or against the occurrence of past events do not form a coherent class. Memories, which historians rarely use, are psychological events or states. Documents, which historians use in most cases, are physical objects, varying in kind, from private papers to official or legal records, and statistical data. Hearsay might also come in handy. It is one thing to suspect that something happened in the past; quite another to believe it took place on the basis of strong evidence. A coherent story based on reports may be sufficient for detaining a suspect, but a much higher standard of evidence is required for actual conviction, and the degrees of belief required for accepting historiographic hypotheses are more similar to those in civil law than to the considerably higher degrees in criminal law.

Let us now take stock of these particular features of empirical evidence for the past.

Historical Significance and Historical Insignificance

From what has been said so far, it might seem an anti-realist interpretation of historiography stems from the rather trite and mechanical application of broad semantic theses about meaning and truth, to the specific case of statements pertaining to the

various historiographic disciplines. It is important to show that it is not so, because anti-realism of the sort discussed here may be sensitive to some of the specific features of historiography.

Suppose we have sufficient evidence to believe that Caesar crossed the Rubicon in 50 BC and pronounced the famous words "*Alea jacta est!*" One might remark that, although this sequence of events is ontologically on a par with any other as far as being in the past is concerned, comparable to, say, yesterday it was raining and somebody was uttering the uninspiring words "It is raining." Still, historical events possess properties the second sequence falls short of having: historical significance, importance, relevance, or "meaning" (obviously, in a sense quite distinct from the one we have used in the characterization of realism, perhaps in a sense close to Dilthey's "value" or "purpose").

No matter how precise and "scientific" we may have been in securing warrants for statements about past events and sequences of events, if these claims are isolated, they lack historical weight, relevance, and even interest. What counts, once we've gathered the gradual, partial, and defeasible evidence that Caesar crossed the Rubicon, is *not* the individual event which took place, or rather most probably took place, but, its historical origins and consequences, its place in an interconnected network of events. The historian wants to establish that a past event bears specific relations to *other* events and sequences which occurred earlier or later. One possibility is that the event was the cause, or one of the many converging causes, of further events which would have themselves become part of the past by the time the historian considers them. The historian does not look just for warrants in favor of past events, but also for warrants in favor of *causal relations* holding between them, for example to build a strong case for the conjecture that Caesar's decision to cross the border between Italy and Gaul in 50 BC led to his march on Rome, which itself led to a civil war, and to the military victories in the Balkans, the Middle East, Africa, and Spain, which eventually secured the building of an empire. The historian may also want to establish the existence of general historical patterns, or to secure nomic generalizations. Philosophers of history may even try to ground grand-scale truths about history, e.g., to show that every major social change has been preceded by an advance in knowledge and a change in opinions and modes of thinking, in the manner of Comte, or Mill; but this is a different type of endeavor altogether, which concerns the way very large historical sequences interact within the framework of the overall history of mankind. See, on this point, Mill ([1843] 1987: especially ch. 10, sec. 7). A view *about* historiography, about what historiographic studies should achieve as a scientific discipline yielding a rational discourse in the market for truth, is at stake here. An anti-realist of the kind considered in the preceding sections must take a stand about the generality of historiography and the objectivity of the kind of knowledge obtained by historians.

Both issues matter to the anti-realist because they pertain to the way historians *justify* their claims, and whether, in doing so, they may argue beyond the particular. Moreover, at the core of the anti-realist's concern, is the problem of determining whether historiographic statements, general or particular, are true or false, i.e., whether the semantic principle of bivalence applies to them. Suppose they are genuine truth-bearers. Could they also bear truth beyond all possible verification, i.e., be true in the sense *rejected* by the anti-realist?

Generality and Holistic Explanations

Some deny that historiography may reach beyond the particular. Historiography then is largely an account – to quote Aristotle – of what individuals such as Alcibiades did and how they suffered. This may be judged a serious drawback, and even serve as ground for denying that history could ever be the subject of serious, scientific study. Descartes' anti-Aristotelian view was precisely that historiographic judgments and reports, concerned with the contingent and particular, amount to a confused heap of memories, gossip, tales, and even fables: there simply aren't any clear and distinct elements of the historical past which could be subjected to general laws, or to rules of inference, and from which firm and irrefutable conclusions could be rigorously attained. From the Cartesian point of view, the work of the historiographer – in the *Oxford English Dictionary's* primary sense of the term, of compiler or chronicler – is *not* in the market for truth. Historiography, certainly of the somewhat literary type of Tacitus, Hume, Gibbon, Macaulay, or Michelet, does not meet the minimal requirements for rationality.

Some deny that historiographic reports of occurrences of individual past events have a cognitive content, or status, independently of a larger holistic historiographic background, such as Hegel's contention that consciousness of a *telos*, purpose, in any given community, is a necessary condition for its objective history to exist at all. Under this conception of how general claims may be handled, the business of the historian is not to give a linear account of past events which will be true to the past facts and the temporal order in which they occurred, but to draw parallels, to compare particular cases, to assess structural similarities. Accordingly, his reasoning will be holistic throughout. Others claimed that there are no laws, or warranted empirical generalizations, in historiography. As Hume pointed to long ago, universal judgments cannot be verified in the narrow inductive sense. But even if there are no laws and no genuine predictions (either retrospective or projective) based on our knowledge of the past, there may be an "interconnected tissue of generalizations," part of which may be deductively connected locally (see Berlin [1960] 1978: 115–22). At stake is how far an anti-realist would be willing to go with regard to the recognition of the "interconnected tissue" of historiographic generalizations.

Suppose we believe we possess warrants justifying the claim that the new political system resulting from Caesar's crossing of the Rubicon shaped the content of Roman law and, from there, thanks to the Christianization of the Roman Empire, that of canonical law. When and where will it be advisable to stop looking for interconnections and parallels? At some point, we shall eventually be committed to some form of open-ended holism, and this will affect the way we shall be looking for warrants in favor of particular and general statements alike. There will be no limits to the *size* of the warrants which will confirm (or disconfirm) any conjecture or hypothesis regarding past events and their relations. For any potentially true historiographic statement we set out to establish, there will be auxiliary hypotheses, or new available evidence, the possibility of which may never be ruled out in advance.

The anti-realist about the past cannot countenance such an unbounded holism in relation to historical evidence: it is incompatible with the requirement of finitude. Our scheme of historiographic explanation may not refer, either directly or indirectly, to

any past event or fact we would construe in such a way that it will end up playing the role of warrant. Only a finite number of evidential warrants for historiographic facts, events and relations may play a genuine evidential role.

The Objectivity of Historiography

It is often claimed that the realist about the past is trying to describe the world from an atemporal position, that he helps himself to the epistemological standpoint of a cosmic exile (see Dummett [1964] 1978, [1969] 1978, and, more recently, 2006: chs. 4, 5). From that standpoint, the realist considers (or believes he has the right to consider) any series of temporal positions he chooses, in such a way that the past, the present, and the future tenses are all fixed. However, past, present, and future tenses cannot *permanently*, or absolutely, characterize moments or events. Whatever will have happened in the future, will later become the past. The realist who attempts to describe events from an atemporal position must therefore consider that temporal positions are only *relatively fixed*.

An anti-realist can argue against this view by claiming that true statements about the past must have some relevant evidence *now*. An anti-realist about the past allows the truth of a statement in the present tense to rely on present evidence, whether or not it is perceived. A statement about a state of affairs which will be part of the past at a later time, e.g., "Caesar crosses the Rubicon" is independent of its recognition by us. The *global* anti-realist, on the other hand, holds a stronger position, according to which the truth of "Caesar crossed the Rubicon" is constrained by, or depends upon the available evidence, so that there is *no* difference in truth-value between "Caesar crosses the Rubicon" and "Caesar crossed the Rubicon." The conception of the past which emerges from this view is that reports of past events were, are, or will be true, at any time t , just in case we can, at such a time t , acknowledge either that there is at t , or will be in the future at $t+1$, some recognizable warrant in its favor. Our understanding of the meaning of *all* tensed statements amounts to our capacity to recognize either the present, or the future availability of a warrant, and to establish the truth of a truth-value link thesis holding for all tensed statements.

It might seem that if our only bona fide conception of the past is one which is constrained by whatever warrants we are able to establish, or recognize as being available in the future, at some given point t in time, the reality of the past will turn out to be relative to us in some strong, constitutive sense. The global anti-realist would therefore be committed to rejecting historiographic objectivity.

Any argument to the effect that one cannot establish truths about history from outside history, is indeed an argument against any form of metaphysical realism about the past, in particular about the historically significant past. It would be illegitimate, though, to jump to the conclusion that since there is no neutral, atemporal or non-historical point of view, historiography is nothing over and above our subjective interpretation of the past from a point of view shaped by our present concerns, say in terms of some current political or ideological agenda, or perhaps an all-encompassing *Weltanschauung*, i.e., nothing over and above, in the last analysis, the result of a social

construction. A strong version of this standpoint yields the view that history is meaningless, that it is nothing but a random collection of interpreted events. This is, by and large, the post-modernist outlook defended by Lyotard (Lyotard 1979) and, to some extent, by Rorty ([1980] 1984 and Rorty 1984).

If constructivism is right and the historiographic model of the past must be constructed rather than found, it doesn't follow that *any* construction will do, or that *none* is acceptable. There is no reason to conclude that an anti-realist will have to deny the possibility of objective truth about history. On the contrary, whether or not a statement is justified, is, from the anti-realist point of view, an objective matter. The notion of justification as cognitive warrant for historiographic claims must be objectively sound and grounded (see, e.g., Dummett's answer to similar questions about ethical claims in Pataut 1996).

Goldstein argued that an ideal observer (a cosmic exile in Dummett's sense), who would be dealing in some privileged way with a fixed and immutable past, would *not* be dealing with the constructed past of historiographic enquiry. The historiographic image of the past changes as new evidences and warrants emerge, and new perspectives of study are opened (see, e.g., Goldstein 1996: 130–1 and O'Sullivan 2006 for a good discussion of this point). Historiographic statements do not describe a corresponding past reality, which would be permanently fixed independently of its epistemic access. The evidence which constrains historical truth is not evidence *simpliciter*, but only evidence relative to a hypothesis, or theory. Just *which* facts would make up, say, the Roman Empire, is something which is itself open to discussion. The immediate, primary, subject matter of historiography is evidence and not events. Accordingly, historiography uses theoretical and technical language just like other sciences that use evidence rather than direct observation of events to infer unobserved realities (Goldstein 1976: xviii, 11, 26–7).

[P]hilosophical writers have virtually ignored . . . problems concerning the emergence or constitution of the historical past in the course of historical inquiry . . . because the history books they tend to read, full blown accounts of much-studied periods, tend to obscure them . . . [W]riters of philosophical essays on history . . . may note . . . that the historian's account must . . . be based upon evidence, but . . . have not the slightest idea of precisely how – unless it be that widely held but totally erroneous idea that he finds his facts in old texts and copies them out . . . It is very easy, indeed, on the basis of such reading, to take the historical past for granted in some realistic way and treat it as something there to be described and explained. (Goldstein 1976: 50–1)

Goldstein (1996: 9–10, 135ff) argued that usually historiographic evidence can be recognized by virtue of its relation to historiography. Goldstein (1996, 9–10) argued that historiographic evidence *confirms* historiographic hypotheses, and the hypotheses *explain* the evidence. Still, confirmation and explanation require more than historiography and evidence; they require theories that connect historiography with evidence and identify the evidence as such in the first place. Whether such theories about evidence, about the transmission of information in time from event to evidence can be interpreted as consistent with an anti-realist approach to historiographic theories is a topic Goldstein did not discuss.

Conclusion

The historiographic discourse bears the external and recognizable marks of any discourse that expresses claims that may be judged objectively true or false. Statements in the past tense may be negated, taken and used as hypotheses, embedded within propositional attitude ascriptions (“X believes it was the case that *p*”), and so on. If their assertoric content is in part guaranteed by surface syntax, and in part by constraints pertaining specifically to the historiographic sciences, then there is room for what Wright calls “cognitive command” for their claims (Wright 1992: ch. 4). This means that: (1) a particular point of view about the nature and reality of the past ought to be held, (2a) the failure to hold it is the result of a cognitive shortcoming, (2b), this failure of rationality may itself be assessed as a failure to build objective arguments in favor of the position it is rational for everyone to hold.

The important philosophical task is to identify the conditions of historiographic objectivity. The vulgar yet common association or even identification of the kind of semantic anti-realism discussed here with the relativism commonly linked with postmodernism and related forms of irrationalism is deeply flawed. On the contrary, it is mandatory for an anti-realist to draw a distinction between claims which are acceptable and those which are not, *given an epistemologically constrained conception of truth*. Global anti-realists, anti-realists with respect to all tensed statements, do not have to discriminate between objectionable and unobjectionable historiographic claims *from outside* the temporal and historical process in which they themselves are embedded. This is a virtue anyone wishing to deny the possibility of objective knowledge of history should be able to appreciate.

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Narrative and Interpretation

F. R. ANKERSMIT

Introduction

The term “narrative” (derived from the Latin word “*narrare*” = to tell, relate, narrate, recount) as applied to historiography suggests that the historiographic text is, basically, a story told by the historian about some aspect or part of the past. The proposal to conceive of the historiography in this way goes back to antiquity (cf. Quintilianus: “*historia scribitur non ad probandum sed ad narrandum*;” “historiography is written not in order to prove but to tell”), was taken up by nineteenth-century historians in Germany (Ranke, Droysen), France (Barante, Michelet) and England (Carlyle, Macaulay) and would determine a large part of the discussion of the nature of historiography in the second half of the twentieth century.

Origins of the Contemporary Debate

Contemporary debates about historiographic narrative originated mainly from a dissatisfaction with a philosophy of historiography focusing exclusively on the issue of historiographic explanation, as was the case with the discussion of the so-called “covering law model” in the 1950s and 1960s. This dissatisfaction had two closely related sources.

In the first place, it was pointed out that historiography may contain explanations as discussed by advocates of the covering law model, but that these ordinarily are merely *parts* or *components* of the historiographic text as a whole and that, consequently, the explanation paradigm did not account for the nature of *the historiographic text as a whole*. Narrative analysis of historiography can find the cognitive message of historiography in the text *as a whole* rather than in *its constituent parts*.

The contention that explanation given in a historiographic text as a whole is not simply the sum of all the explanations of individual events given in it was nevertheless criticized. The historiographic narrative of the Russian October Revolution has the form of a long chain of events where each of them can be explained taking into account the previous one plus extra initial conditions. Ernest Nagel had called this “genetic explanation” (Nagel 1961: 56–8; White 1965: 230–2; Danto 2007: 251–6).

It follows that the notion of narrative *as a whole* must be condemned as cognitively redundant: it adds nothing that cannot be reduced to the sum of the individual (causal) explanations in a narrative. Two objections can be made against this explaining away of historiographic narratives. First, the “genetic” model suggests that all historiographic narratives should have the character of describing some chronological evolution in time, where each phase in this evolution can be (causally) related to the previous one. This does certainly fit many historiographic narratives. However, books like Huizinga’s *Waning of the Middle Ages* or Braudel’s *La Méditerranée* are “cross-sectional,” in the sense of describing a historical epoch rather than an evolution. Since the term “narrative” (with its associations with the prototypical nineteenth-century realist novel) enhances the misconception of historiography suggested by the genetic explanation model, one had better speak of “historiographic representation” than of “historiographic narrative.”

Second, Haskell Fain expounded the decisive weakness of the genetic explanation model by insisting that it necessarily lacks a guide for *which events* are to be causally related. Without any such guide the genetic model might get the narrative moving into simply any direction. It might end up in mere circumstantial detail, in what is wholly irrelevant, or perhaps even in the neurophysiologic processes in the minds of the principal historical agents. But even if such accounts of the past would satisfy all requirements of (causal) explanation, we would find them to be both useless and incomprehensible. Apparently there are criteria for what counts as a satisfactory narrative; and we can only get hold of these criteria by investigating the historiographic narrative as a whole (Fain 1970).

Historiographic Research and Writing

Fain’s criticism can be reformulated in terms of the old distinction between “historiographic research” (“*Geschichtsforschung*”) and “the writing of historiography” (“*Geschichtsschreibung*”). Historiographic research aims at the establishment of factual truth about the details of the texts, either statements or causal explanations. The writing of historiography deals with how factual truths can, or should be integrated into one coherent narrative whole (Droysen 1857: 331). Crucially, narrative coherence cannot be reduced to truth (in much the same way as the problem of the validity of scientific theories cannot be reduced to factual truth). This irreducibility of coherence to truth implies that the historiographic text raises a set of philosophical problems. The sub-field of philosophy of historiography that deals with narratives is irreducible to other subfields that examine issues about the truth of historiographic statements and historiographic causation and explanation. Accordingly, the philosophy of historiography should focus on the analysis of historiographic narrative, in addition to the analysis of historiographic research. In sum, from a cognitive point of view, three levels should be discerned in the historiographic text: (1) that of the statement of individual facts, (2) that of explanation, and (3) that of the holistic narrative. Put provocatively, one can both uphold a covering law model as far as the analysis of particular explanations is concerned, and still use a narrativist approach for the holistic analysis of historiographic texts.

The next question is to what extent narrative explanation of historical events is *sui generis* and irreducible to other forms of explanation. If historiographic narratives may

clarify or explain parts of aspects of the past and if this clarification/explanation cannot be justified in terms of non-narrative models of explanation, it is the philosopher's task to develop a convincing analysis of narrative explanation (Ankersmit 1983: 227–47).

The so-called “linguistic turn” added further to the urgency of the issue. Quine's attack on the analytic/synthetic distinction demonstrated that language cannot be left out of the picture if we aim at an adequate account of the origins of our true beliefs. Quine's argument in favor of the linguistic turn was translated by narrativist philosophers of historiography into the claim that we must focus on the historiographic text as a whole for a correct understanding of how historiography may account for some part or aspect of the past. It follows that philosophy of historiography has its own indispensable contribution to make to philosophy of language. The general problem of how complex texts as a whole may account for some part or aspect of the world, may well be best exemplified by historiography.

Two Variants of Narrativist Philosophy of Historiography

Narrativist philosophy of historiography has taken either of two forms connected with two different groups of theories and research programs: In the first place, the problem of narrativism may be formulated as a primarily philosophical analysis of the (epistemological) relationship between the historian's language and the past recounted in it. This will give us the philosophical approach to narrative. Second, one may decide to turn for help to literary theory. Literary theory has a long tradition of investigating literary texts, such as the “historical novel.” One may therefore expect that the findings of literary theory can fruitfully be applied to historiography. Nevertheless, so-called “narratology” – a branch of literary theory specially devised to deal with narrative – has only rarely been appealed to by narrativist philosophers of historiography (Herman 2005). For reasons given below, I call this the rhetorical approach to narrative.

The Philosophical Approach

The main issue here is whether one is a realist or a nominalist regarding the relationship between historiographic narrative and history, whether historiography describes aspects of the past as they were, or whether that relation remains undetermined. Louis O. Mink opted for the exclusively nominalist view. He started with the claim that philosophy of history nowadays is just as alive as it was in the days of Kant, Hegel, and Marx. Philosophies of history claimed to tell us “the” story of the past in the sense of accurately reflecting the narrative order present in the past *itself*. This made them vulnerable to Popper, Hayek, and Mandelbaum's accusation of being “metaphysical.” Nevertheless, this still is how both historians and philosophers of history and historiography intuitively think about historiography and how it relates to the past. We feel, with regard to historiographic narrative, an almost ineradicable need to tie together, in Gadamer's words, Logos and Being, the order of speech and the order of reality. We are continuously tempted to believe that: “the story of the past needs only to be

communicated, not constructed" (Mink 1987: 194). However, Mink requires us to abandon radically "the idea that there is a determinate historical actuality, the complex referent for all our narratives of "what actually happen," the untold story to which narrative histories approximate" (p. 202). The past is not an "untold story"; stories only come into being when we speak about the past. Or, as Mink summarized it, all "stories are not lived, but told" (p. 60).

David Carr and Paul Ricoeur attacked Mink's position (shared by Arthur Danto, Hayden White, and Frank Ankersmit) from a phenomenological point of view. They argued that narrative inheres already in life. We perceive everything narratively, as a story, just as we can only become aware of a melody by holding together the different tones constituting it in our mind. Indeed, we live and conceive of our lives narratively; narrative is the ineluctable pattern organizing all our actions and thinking. "Louis Mink was thus operating with a totally false distinction when he said that stories are not lived but told. They are told in being lived and lived in being told" (Carr 1986b: 125, 126; cf. Ricoeur 1983).

The nature of the disagreement between Mink et al. and Carr et al. is notoriously difficult to pin down. It may be argued that there is more agreement than disagreement between the two of them. Mink need not deny that we can tell the story of ourselves and act accordingly. And Carr need not deny that there is a categorical difference between story-telling and acting. Even more so, both story-telling and action require the capacity to reduce the manifold of the relevant contextual data to one single perspective from which either the narrative is organized or action is decided upon. So both oblige us to a (synoptic) taking together of what is given to us only separately and sequentially in time – just as we can only recognize a melody on the condition of our capacity to put together somehow the melody's individual notes.

In order to perceive correctly the nature of the disagreement one had best turn to Mink's main source of inspiration, Arthur Danto's account of historiographic narrative. Danto's narrativist philosophy has its point of departure in an apparently most banal fact about how our language deals with time, but from which he derives the most breathtaking results, justifying his claim to offer a "descriptive metaphysics" of narrative and temporal language in Strawson's (1959) sense. He demands of us to consider "narrative sentences" which are true description of events and defined by him as: "the class of descriptions . . . [that] refer to two distinct and time-separated events E-1 and E-2. They describe the earliest of the events referred to" (Danto 2007: 152). One of Danto's (many) examples is the "narrative sentence": "the author of the *Principia* is born in Woollethorpe on Christmas Day in 1642." Note, then, that on Christmas Day 1642 one might well have said "Isaac Newton is born today in Woollethorpe" but not that "the author of the *Principia* is born in Woollethorpe" since that statement, appropriately tensed, could only be uttered after 1687. The implication is that time causes there to be a radical asymmetry in what we can say about a certain event: there are true statements about the event of Newton's birth in 1642 that can only be made much later. Danto reformulates the insight by requiring us to conceive of an "Ideal Chronicle," i.e., of a narrative consisting of all the true statements that could be made about the world right from Adam and Eve down to the present. We must conclude that the Ideal Chronicle will necessarily always be incomplete since in the future new true statements can be

added to it that we presently could not yet know of, and could not be mentioned in this Ideal Chronicle.

Danto's "project-verbs" add further to the structure of narrative language. "Project verbs" are verbs like "planting roses," "building a ship," or "writing a book." Danto points out that we may use these verbs for describing the actions of people with a view to an, as yet, indefinite future. For suppose that somebody is putting the seeds of roses in the soil behind his house. We could then properly describe his actions, his "project" as Danto would have it, as "planting roses" – notwithstanding the fact that because of a drought these seeds may never actually grow into roses. In this way there is a potential asymmetry between, on the one hand, our description of what this man is doing right now and, on the other, the actual results of his action. For we say that he is planting roses, whereas future historiography may record that he had not been planting roses, after all, since the seeds died in the soil. In this way, a potential tension, or *asymmetry* arises between the *language* we happen to use, on the one hand, and *actual history*, on the other. The implication is that this is how *historiography* may come into being: for we happen to use a kind of language (i.e., the language of "project verbs") that may be belied by what history actually is, or rather, would be like. If we didn't have these "project verbs" with their implicit reference to the future, language would always nicely and reassuringly correspond with what actually happens – and historiography and its narrative would never manifest itself (Danto 2007: 159ff).

In his later work Danto extrapolated his argument about narrative sentences to historiographic texts as a whole, or, as he would preferably put it himself, to historiographic *representations*. He argues that historiographic sentences are *intensional*, their truth depends on their meaning and context as well as on their reference, by their formulations as well as by the world. The intensionalism of narrative sentences is obvious: the statement about the event of Newton's birth "the author of the *Principia* was born in 1642" is true only after 1687, while being false or undecidable before that year. Even more important is the observation that everybody, including historians, continuously devise new vocabularies in terms of which they conceive of the past and that were unknown to the people in the past themselves, for example, the people who lived through the Industrial Revolution were not aware of it. So intensionality is far from being a merely accidental property of historiography. Historiographical progress will partly have to be defined in terms of intensionality.

The intensionality of historiographic narrative language contributes to the asymmetries between past and present observed above. People living in a certain historical epoch experience their world directly, that is to say, in the light of the beliefs they hold to be true. The historian, however, adds the dimension of intensionality: the historian joins these beliefs together within a *representation* of the world. Hence, a "representation" is the set of beliefs the historian ascribes to people who lived in a certain epoch in a way that would have been impossible in that epoch itself. Put differently, as soon as this narrativist intensionality, as expressed in or by a representation, has come into being, we have moved out of a certain historical epoch; it has become "history" and one has entered a new, and later historical epoch:

And something of the same sort is true for the historical period considered as an entity. It is a period solely from the perspective of the historian, who sees it from without; for those

who lived in the period it would be just the way life was lived. And asked, afterwards, what it was like to have lived then, they may answer from the outside, from the historian's perspective. From the inside there is no answer to be given; it was simply the way things were. So when the members of a period can give an answer in terms satisfactory to the historian, the period will have exposed its outward surface and in a sense be over, as a period. (Danto 2007: 183)

So representation inexorably separates us from a world whose "outsiders" we have become, but, at the same time, gives us an access and insight into it (as granted by representation) that the "insiders" living at that historical period could themselves never gain.

The Transcendentalization of Narrativist Philosophy of Historiography

Danto's narrativist philosophy of historiography found its ultimate perfection in that of Hans Michael Baumgartner. Baumgartner radicalizes Danto's argument into a *transcendentalist* analysis of historiographic language (Baumgartner 1972: 278ff). Baumgartner accused Danto of contaminating his "descriptive metaphysics of historical existence" with the remnants of a naive historical ontology. Danto still speaks about notions such as "The Middle Ages," "the French Revolution," or "the Renaissance" as if they referred to things with the same ontological status as is possessed by what proper names like "Caesar" or "Napoleon" refer to. But for a satisfactory appraisal of historiographic language the subjects of biographies are misleading. The unity or continuity of persons or individuals such as Caesar or Napoleon is warranted by notions such as "person" or "individual," hence, by sortal concepts (to use the vocabulary of the philosopher of language) denoting categories of objects always possessing unity and continuity through time simply because of the meaning of these concepts. And this is essentially different with notions such as "the Middle Ages," "the French Revolution" or "the Renaissance." For such typically historiographic notions do not *presuppose* unity and continuity, as is the case with the notions of "person" or "individual," but *create* them (Ankersmit 1983). So historiographic language – into which Danto gave us so many profound insights – is the condition of the possibility of having knowledge of typically historical "things" such as "the Middle Ages," "the French Revolution," or "the Renaissance." Hence, Baumgartner's thesis was that Danto's argument in *Analytical Philosophy of History* still awaited its transcendentalization, following Kant's transcendentalism, the concept of the task of philosophy as the articulation of the conditions of any experience.

However, as Baumgartner demonstrates, Danto himself was prevented from developing the transcendentalist implications of his own account because of his belief that there cannot be historical change without there also being "subjects of change" (Baumgartner 1972: 289). Needless to say, this is how we intuitively tend to look at the issue: first there is say Napoleon, the subject of change existing in the past itself, and next, we can give an account of his complex path through space and time. And the latter is impossible without the former. But, as Baumgartner insists, in the case of

historical phenomena such as “the Middle Ages” there simply *is* no subject of change that is given to us prior to the historiographic narratives. These phenomena lead their lives exclusively, so to speak, in historiographic narratives.

Further, Baumgartner considered the unity and continuity of narrative as the transcendentalist conditions of historical meaning and the transcendentalist standards for measuring the historian’s success in making sense of the past. Unity and continuity are the product of narrative synthesis and do not mirror the features of an object existing in the past itself (Baumgartner 1972: 301). This, then, is the meaning we should give to Danto’s claim that narrative can explain *as narrative* and that we may discern in narrative explanation what differentiates historiography from the sciences.

Let us return now to the disagreement between Mink and Carr: Mink was right to say that “stories are not lived but told” and Carr was right in arguing that stories are part and parcel of our lives. But both were wrong by failing to take into account the representationalist transcendentalism of narrativism separating life and story, even if we tell the story of ourselves. For precisely by doing so, we become the historians of ourselves and, as Danto so admirably made clear, this makes us into a *different* person, just as we have moved into a new historical epoch as soon as we recognize a previous one as an historical epoch. And this is all the more true of traditional historiographic narrative.

Rhetorical Narrativist Philosophy

Summarizing his intellectual effort in one sentence, Hayden White wrote: “but in general there has been a reluctance to consider historical narratives as what they most manifestly are: verbal fictions, the contents of which are as much *invented* as *found* and the forms of which have more in common with their counterparts in literature than with those in the sciences” (White 1978: 82). The ambiguity of the word “fiction” in this quote indicates the difference between the philosophical and the rhetorical approach to historiographic narrative. On the one hand, the term is suggestive of something that is being “made” or “invented” in the way that one may say that the scientist “makes” or “invents” a theory: theories do not present themselves in the way this can be said of trees or mountains. The scientist has to develop his theories on the basis of his experiments. On the other hand, the term “fiction” is also related to fictional literature, then having the connotation of being “fictional” and not in agreement with actual fact. White combines these two meanings of the term “fiction” implying that a historiographic narrative may convince us in much the same way that a novel may do so. In such cases readers become convinced by the historiographic narrative through the historian’s capacity to write compellingly – much like the way that a talented political orator may make us agree with the course of action recommended by him, not by fact or evidence. Hence, I use the term “rhetorical narrativist philosophy.”

White was not the first to deal with historiographic narrative in this way. Roland Barthes and Lionel Gossman (to mention the most important) had preceded him. Gossman, a literary theorist of great renown, studied the writings of Thierry and Michelet as if they were literary texts. Though he initially liked to infer from his findings conclusions about the nature of historiography, he gradually became more

skeptical about the procedure (Gossman 1990). This more cautious approach is shared by Lynda Orr, Ann Rigney, or Larry Shiner's writings on several French nineteenth-century historians (it is worth noting that the rhetorical approach works best there whereas it seems to yield no interesting results if applied to contemporary professional historiography writing). A wholly new departure was Bann's effort to use the literature and the visual arts of the first half of the nineteenth century for grasping the kind of historiographic narratives then produced (Bann 1984); as was also done by Tollebeek (Tollebeek 2000) for the same period. Roland Barthes went even further in two essays on the so-called "reality effect" in historiography: he claimed that the nineteenth-century realist novel effected the illusion of recounting something that had truly happened by mentioning all kind of circumstantial and irrelevant details. And he went on to say that much the same is true of all of nineteenth- and twentieth-century historiography, while singling out the historian's footnotes as being especially successful for achieving the "reality effect." The footnotes are not part of the story told by the historian and yet they are necessary conditions for its truthfulness.

Hayden White

Hayden White has been more influential than any of these authors. White proposed a more subtle and promising explanation of the rhetorical success of historiographic narratives. Crucial in White's narrativism is the notion of "*metahistory*." The term denotes the "precritically accepted paradigm of what a distinctively 'historical' explanation should be" (White 1973: ix). The facts of the past that the historian has brought together in his research are arranged according to these structures; or, as White puts it himself, they guide the historian in an act of "prefiguration." White discerns four components in them: first, tropes affect a reduction of the manifold of the past to a communicable content, in the way that, for example, metaphor reduces this manifold to its "essence"; second, modes of emplotment; third, modes of argument; and, fourth, modes of ideological implication. Each of these components has, in its turn, four variants: the tropes are metaphor, metonymy, synecdoche, and irony. Emplotments are romantic, tragic, comic, and satirical. Arguments are formist, mechanistic, organicist, and contextualist. Ideologies are anarchist, radical, conservative, and liberal (White 1973: 29).

Much depends on the origin and logical status of these paradigms of prefiguration. One cannot fail to be struck by their similarity to the Kantian categories. Just like these categories, White's paradigms seem to be the conditions for the possibility of making sense out of the quasi-noumenal manifold of historical data (Kellner 1989). The problem with this view is, however, that unlike Kant, White does not offer a "transcendental deduction" of his paradigms of prefiguration (Ankersmit 1983: 83). He derived them from existing literature on tropes, without justifying them on the basis of an a priori argument about the nature of any knowledge of history. They are presented, rather, as a statement of fact about the sociology of historiographic knowledge. Unlike the Kantian categories, these paradigms are optional: for White the historian is free to choose between them. Since each choice results in a narrative, much different from one that was inspired by a different paradigm and since White claims that historiographic debate can never compel us to accept one narrative in favor of another, it is not surprising that White

has often been accused of skepticism. All this may make clear, furthermore, where White's *Metahistory* differs from the transcendentalist philosophy of narrative we encountered above, and why it fits within the rhetorical approach to narrative. Metahistory may explain *why* historiographic narrative convinces (or not), but not what *rational justification* can be given for this.

More importantly, initially White insisted that there are typical correlations between specific tropes, modes of emplotment, arguments, and ideological implications. In *Metahistory* he claimed that the most interesting historians did not respect these correlations or elective affinities. This would be the point of departure for most of White's later work. He then emphasizes the free choice of the historian in presenting the data of the past and the element of creative activity in these choices. Aesthetic, political, or moral considerations then become the source from which all historiographic insight originates. Hence, the historian should always be profoundly aware of his moral and political responsibilities. This is the lesson White urges us to learn from Sartre's existentialism (Paul 2006).

Conclusion

Our main conclusion must be that contemporary philosophy of language is condemned to remain a mere torso without legs and arms for as long as its practitioners stubbornly persist in their present disregard of narrative. Historiographic narrative is the best point of departure to remedy this lacuna. The present state of affairs in narrativist philosophy of historiography happens to be in surprising harmony with that in the philosophies of language and science. The significance of Danto, Mink, Ricoeur, Carr, and Baumgartner's narrativism for philosophy of language is obvious. The rhetorical approach to historiographic narrative nicely dovetails with contemporary distrust of transcendentalist and aprioristic ways of thinking; it is the obvious counterpart to the present interest in a "naturalized epistemology" and what is referred to as "the logic in use." More specifically, the rhetorical approach to narrative may show how a historiographic epistemology arises out of the actual practice of writing historiography. This philosophical approach may then complicate and augment present insights into the nature of knowledge.

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F. R. ANKERSMIT

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The Ontology of the Objects of Historiography

LARS UDEHN

When asking about the ontology of historical objects, we are asking for the “stuff,” or ultimate constituents, of historical reality. What is historiography about, events or social structures? The first answer is probably the most common or, at least, it used to be. It is easy to exaggerate the predominance of this type of historiography, however. There were always other types of historiography, as well. There was the universal history first suggested by Bousset (1627–1704) and recently defended by Francis Fukuyama, the philosophies of history by Hegel and others, the historical materialism of Karl Marx, and the histories of civilizations by Toynbee among others. In the twentieth century the interest of many historians has shifted from political, first to economic, then to social and, more recently, to cultural history. Among the more significant manifestations of this change is the historiography written by members of the French Annales School. Fernand Braudel, in particular, challenged the orthodox view that historiography is about events. According to him (Braudel, 1980, *passim*), it is, or should be, also about social structures prevailing over longer time spans than events.

It has been argued that the distinction between events and structures is misguided, since social structures are made up of events (Roberts 1996: ch. 7). More precisely, social structures are particular types of complex and recurrent events. On this view, events could be simple or complex, unique or recurrent. The murder of the Austrian Archduke Franz Ferdinand in 1914 was a fairly simple and unique event, while the French Revolution in 1789 was a complex and unique event. In metaphysics, a distinction is sometimes made between events and substances. The ultimate constituents of the world were usually thought to be something more substantial than passing events. So what is the “substance” of history?

An important feature of “substances” is that they have some continuity, or identity, over time. This is a feature that events are lacking, but which the idea of structure seems to convey. If we accept the idea that what is real in history must have some identity over time, then we should look for the ultimate constituents of historical reality among entities such as individuals, groups, organizations, institutions, and structures. These are the kind of entities most often discussed in ontologies of social and historical reality. The main issue is between those who maintain that society is made up of, and history made by, individuals, and those who see some kind of social entities; groups, institutions, societies, as realities in their own right. The former are usually

referred to as individualists, or social atomists, the latter usually called collectivists, or holists. The debate between them is usually conceived of as a matter of the relation between individual and society (Carr [1961] 1964: ch. 2).

The issue between individualists and holists is basically ontological. Closely related to the ontological issue, however, is a methodological one about the proper way to understand and explain social phenomena. According to the principle of “methodological individualism,” social phenomena must be understood and explained in terms of individuals. According to its opposite, variously called “methodological collectivism” and “methodological holism,” social phenomena must be explained in terms of social wholes, or collectivities. In addition to the ontological and methodological issues there is an epistemological question about the possibility of gaining knowledge about different historical objects. All three issues are closely intertwined and often mixed up, even confused. The protagonists often seem to misunderstand one another and argue at cross-purposes.

Among individualists, it is common to lump together holism and a version of historicism (Popper ([1957] 1961: 17–19). Popper’s critique of holism and historicism reflects a common fear, among individualists, that these doctrines rob individuals of their freedom of action and reduce them to mere playthings for supra-individual forces, and thus legitimize totalitarian ideologies (see also Berlin [1954] 1969). This is an extreme view, but undoubtedly methodological and political individualism are connected (Dray 1972: 53; Gilbert [1989] 1992: 428ff).

Background

The first individualistic theory of society was the theory of the social contract, so called because society, or the state, was seen as the result of an agreement (covenant) between individuals in a pre-social state of nature. Its most famous exponent was Thomas Hobbes (1588–1679), who saw the state as a deliberate creation of rational and self-interested individuals for their mutual benefit. Different versions of this theory dominated thinking about society until the end of the eighteenth century when it was replaced by more evolutionary accounts of state and society. Of particular importance was the idea that social phenomena may be the unintended consequences of human behavior. Adam Smith (1723–1790) applied it successfully to the market and showed that, in this particular case, self-interested individuals are led, as if by an invisible hand, to contribute to the common good. Adam Smith was not, himself, what we now call a methodological individualist, but his theory of the market is the origin of the theory of general equilibrium, which is the most individualistic of all social scientific theories. Neoclassical economics, as a whole, is clearly the most individualistic social science. Historiography, and disciplines such as anthropology and sociology, owe more to German Romanticism and historicism, which are the main sources of social holism.

The sociologist Emile Durkheim is one of the main representative of holism in social science. He has also exerted important influence on the *Annales* School in historiography. Durkheim ([1895] 1982: ch. 1) famously argued that society is a reality *sui generis*, which means that it exists in its own right. Society is made up social facts; most

importantly social institutions, and they have two characteristics: (1) they are external to individuals and (2) they have coercive power over them. To support the first thesis, Durkheim points out that we are all born into an already existing society, with innumerable social institutions, which we must accept as given to us. The coercive power of social facts emanates from their place in the collective consciousness, which is a creative synthesis of the consciousness of individuals and, therefore, more than the sum of them. Social representations, while existing in the minds of individuals, are irreducible to individual representations.

Methodological Individualism

The principle of methodological individualism did arise as an antidote to the holistic excesses of German Romanticism and historicism. An early version of it can be found in John Stuart Mill, who denied that there are genuine causal laws of society and claimed that the social sciences must be based on psychology. "The laws of phenomena of society are, and can be, nothing but the laws of the actions and passions of human beings united together in the social state . . . Men are not, when brought together converted into another kind of substance" (Mills [1843] 1974: 879). The main source of methodological individualism, however, is the Austrian School of economics. The founder of this school, Carl Menger, launched his "atomistic method" in his attack on the holistic approach of the German Historical School of economics (Menger [1883] 1963).

According to Max Weber, sociology must treat the individual as its basic unit, or atom. Weber was also critical of the use of collective concepts, such as "state," "association" and "feudalism." These concepts must be understood as categories used to refer to certain kinds of joint human action. According to Weber, it is the task of sociology to reduce these concepts to the understandable action of the participating individuals (Weber [1913] 1981: 158). Weber's individualism follows from the subjectivism of his interpretive sociology, which is "a science concerning itself with the interpretive understanding of social action and thereby with a causal explanation of its course and consequences" (Weber [1922] 1978: 4).

Weber's methodology was a main source of inspiration for two economists of a later generation of Austrian economists: Ludwig von Mises and Ludwig von Hayek. In their hands methodological individualism turned into an ontological thesis about social reality, which says that social phenomena are subjective and exist in the minds of individuals. According to Hayek, "most of the objects of social or human action are not "objective facts" in the special narrow sense in which this term is used by the sciences . . . and they cannot be defined in physical terms. So far as human action are concerned the things are what the acting people think they are" (Hayek [1942–4] 1955: 26ff). This goes for all human creations, artefacts as well as social institutions. Terms denoting tools and instruments, for instance, cannot be defined in terms of their physical attributes, but must be defined in terms of their intended function (p. 27). The objects of economic theory, money for instance, must be defined in terms of the views people hold about them, not in objective or materialist terms (p. 31). Hayek recognizes the fact that people frequently use collective concepts such as "society," "capitalism," and

“imperialism,” but these are popular abstractions not to be mistaken for facts. He distinguished *constitutive* ideas, which motivate people in their actions, from *speculative* ideas, which people use to explain social phenomena (pp. 37ff). Collective concepts are expressions of speculative ideas, or popular myths. The task of social science on the other hand is to construct models of social phenomena out of the constitutive ideas that guide individuals in their actions.

There are many similarities between Austrian and Popperian methodological individualisms. But Popper and his followers dropped subjectivism and moved the focus from concepts to explanations. They also tried to avoid ontology and defend a strict *methodological* individualism, but with little success. According to Popper, not only the popular ideas, but most objects of social science, the “war” or the “army,” for instance, are abstract objects or theoretical constructions, models which must be analyzed “in terms of individuals, their attitudes, expectations, relations, etc. a postulate which may be called ‘methodological individualism’” (Popper 1957: 136). Popper argues that methodological individualism is distinct from, and irreducible to, psychologism, but nevertheless defends “the quite unassailable doctrine that we must reduce all collective phenomena to the actions, interactions, aims, hopes, and thoughts, of individuals” (pp. 157ff; see also [1945] 1966: 98). In addition to methodological individualism, Popper defends institutionalism and the two doctrines seem to be incompatible. At least, they led to two different developments of Popperian methodological individualism: the strictly individualistic version of J. W. N. Watkins and the institutional individualism of Joseph Agassi.

For Watkins, the principle of methodological individualism states that “social processes should be explained by being deduced from (1) principles governing the behaviour of participating individuals and (2) descriptions of their situations.” This principle is supported by the ontological thesis that “social ‘things’ like laws, prices, prime-ministers and ration-books, are created by personal attitudes” and the epistemological thesis that social scientists and historians do not have direct access to the behavior of social systems (Watkins 1953: 729). In a later article, methodological individualism is, not only supported by, but stated explicitly as, an ontological thesis: “According to this principle [methodological individualism], the ultimate constituents of the social world are individual people who act more or less appropriately in the light of their dispositions and understanding of the situation” (Watkins 1957: 105ff). In another statement, “methodological individualism means that human beings are supposed to be the only moving agents in history,” while its opposite, “sociological holism means that some superhuman agents or factors are supposed to be at work in history” (p. 106).

Joseph Agassi (1960, 1975) argued that social institutions are important parts of most situations in which individuals act and also important causes of human behavior. Agassi called this new version of methodological individualism *institutional individualism*. More recently, a similar philosophy assigned an important role to social structures in the explanation of the social action of individuals and of large-scale social phenomena (Udehn 2001: 199, 295–306, 318ff). Since institutional and structural individualism are less demanding than the original version it makes sense to distinguish a *strong* from a *weak* version of methodological individualism.

Problems with Methodological Individualism

The strictly methodological principle of strong methodological individualism has proved difficult to maintain. There are practical difficulties. It is not likely that social science and history could ever do without collective concepts for descriptive purposes. Historiography is full of concepts denoting civilizations, cultural epochs, economic systems, social movements, nations, states, classes, organizations, juristic persons, groups, etc., and would be quite impossible without them (Danto 1968: 258ff). But it has been argued, especially by Popperians, that concepts are nothing, explanations everything.

There is reason to doubt the version of strong methodological individualism that demands that all historical events be explained in terms of individuals. It is extremely difficult to find examples of strictly individualistic explanations in the social sciences and historiography. Even the most individualistic of the social sciences, economics, is not individualistic enough, as there is no successful individualistic reduction of macroeconomics to microeconomics and microeconomics is not strictly individualistic (Arrow 1994). J. W. N. Watkins recognized the existence and legitimacy of “half-way explanations” of large-scale social phenomena, though his ultimate goal remained “rock-bottom explanations” in terms of individuals, their dispositions, beliefs and interrelations (Watkins 1957: 106; Elster 1985: 6, 359).

There are fundamental epistemological arguments against strong methodological individualism. Maurice Mandelbaum (1955) argued that historiography and social science have to use holistic concepts which are not reducible without remainder to individualistic concepts. Mandelbaum’s argument was really about the understanding of social facts, but it is usually interpreted as an argument about concepts (Bhargava 1992: 25, 33–5). It is impossible to understand and describe an act such as cashing a cheque, without referring to the role of bank-teller and to the banking system of which it is a part, and the same goes for all social action. We are caught, as it were, in a hermeneutic circle of social facts. Though Mandelbaum’s analysis is almost uncontested, the typical response of methodological individualists has been to deny its relevance. The important thing is explanation, not concepts.

However, there is a similar argument against explanatory individualism: all individuals are born into an already existing society, made up of a host of social institutions. Any attempt to explain the behavior of individuals without reference to at least some of these institutions is in most cases impossible. We behave the way we do because of institutions and as part of them. Arguably, these institutions are created by individuals, but the individuals are explained by other institutions and so on, the attempt to avoid institutions altogether leads to an infinite regress. This argument has been used against methodological individualism by many social scientists and philosophers, including Popper ([1945] 1966: 93), who used it against psychologism, Gellner (1956: 170ff) and Nozick (1977: 358), who used it against Austrian methodological individualism.

Supervenience concerns the relation between two levels of reality, such as body–mind or individual–society. It says that if two objects in the subvenient base share all properties in common, they are also completely alike in their supervenient properties, but not vice versa: Two objects that share all properties in common at the supervenient level might have multiple realizations in the subvenient base. Applied to the

relation between individual and society, a supervenient property of a social entity, say democratic, or hierarchic, can have many realizations at the microlevel. This is obviously an obstacle to reduction. If you want to reduce a statement about democratic societies to statements about individuals, you would have to do it in the form of a disjunctive statement about all possible realizations of democracy at the individual level.

The arguments about supervenience and multiple realization has led some philosophers and social scientists to give up methodological individualism, while retaining ontological individualism (see, e.g., Little 1991: 192–5; Kincaid 1996: 187–90, 1997).

The Nature of Social Reality

While there are pretty strong reasons to believe that strong *methodological* individualism is not a viable position, the arguments against *ontological* individualism are apparently less convincing. Among philosophers, at least, there is a strong resistance to the idea that there might be something else than individuals in society and even more resistance to the idea that there might be other causes than human action at work in history. “History is made by men” (Marx [1852] 1973: 146) “not of their own free will; not under circumstances they themselves have chosen but under the given and inherited circumstances with which they are directly confronted.” Few would disagree with this statement, at least not until the circumstances are specified. What would the candidates be? The physical environment, of course, and other human beings. Marx, however, believed that individuals create social institutions and structures that somehow take on an existence of their own, which confront human beings as objective facts.

It was argued above that historiography and social science cannot do without concepts, denoting social institutions. This is now a commonly accepted view. But it has also been argued that this view has definite implications for social ontology. Holistic concepts are not just descriptive, they are constitutive of social life. A well known argument to this effect was advanced by Peter Winch in his *Idea of a Social Science* (1958). Winch maintained that social concepts are embedded in and inseparable from forms of life. Social life is, one might say, conceptual. It is not clear, however, that any holistic ontology follows from this argument. Most methodological individualists, and the Austrians in particular, agree that people use and act upon holistic concepts. But this is no threat to an individualist ontology as long as these concepts are in the minds of individuals (Elster 1982: 453).

A stronger argument for a non-individualistic ontology can be found in the writings of John Searle. In his first major work *Speech Acts* (1969), he made the distinction between regulative and constitutive rules (pp. 33ff). While the former regulate previously existing behavior, the latter create and define new types of behavior. The game of chess, for instance, cannot exist without the rules of chess. For Searle, it is not just that individuals are guided by social concepts, it is because social institutions are made up of constitutive rules, that they must be described, specified, and defined in terms of these rules. Also, constitutive rules do not come alone, they come in systems of rules and they constitute systems of interdependent actions.

Searle ends his analysis of constitutive rules with making a distinction between “brute” and “institutional” facts. The latter, whose existence presupposes certain human institutions and these institutions are systems of constitutive rules (pp. 50–3).

Searle develops his analysis of institutions in greater detail in *The Construction of Social Reality* (1995). His argument is complex and utilizes a special terminology, but it has three main components: (1) collective intentionality, people cooperate and share beliefs, desires, and intentions, (2) the assignment of function both to material objects like chairs and money and to occupants of social positions like police officers and professors, and (3) constitutive rules.

With this apparatus, Searle goes on to give an account of constructed social reality. From the notions of collective intentionality and assignment functions it follows that social reality is mind-dependent, or observer-relative. The question here is whether social institutions exist in their own right. Searle seems to deny this since he maintains that they are epistemologically objective, but ontologically subjective (Searle 1995: 12ff, 63). This appears to be the position of Hayek, with which Searle’s account has many similarities. But a more holistic interpretation is possible. Searle admits that an external view of social institutions is possible, but that the internal (subjective) view is ontologically primary. Could it be that social institutions have a “secondary” existence? Searle argues that social reality is a complex web of interlocking and interdependent institutions (p. 35). He also maintains that social structures play a causal role in the explanation of human behavior (ch. 6). People adapt, sometimes consciously, but mostly unconsciously, to existing social institutions. This sounds like admitting that social structures are real, but then, again, “Social objects are always . . . constituted by social acts; and in a sense, the object is just the continuous possibility of the activity” (p. 36). “What we think of as social *objects*, such as governments, money, and universities, are in fact just placeholders for patterns of activities” (p. 57). According to Searle, process has priority over product. This is individualistic talk.

Even though Searle is obviously concerned with the ontology of social reality, it is not easy to grasp exactly what entities he accepts. It becomes a bit clearer in a recent article (Searle 2001). He now says that social institutions, even though mind-dependent, exist as objective entities in social reality (p. 17) and this is probably what he wanted to argue also in *The Construction of Social Reality*. Social reality contains then in addition to people, also social institutions and even social structures, about which he does not say much. According to a common view among social scientists, social structures are made up of relations between individuals or the positions they occupy. Are social structures, in this sense, real or just configurations of individuals?

Many individualists admit of social relations in their explications of strong methodological or ontological individualism, without feeling that social structures exist in their own right and have causal powers of their own. This view is nowadays defended by the so-called critical realists (Bhaskar 1979) and, with special application to historiography, by Christopher Lloyd (1986). The difference between them seems to lie in their different concepts of social relation. For the individualist, social relations are between individuals, and the properties they have as parts to relations; wife, priest, officer, are relational properties of individuals (Elster 1982: 453). For the holist, many social relations are internal relations between positions in social structures and, as such, are emergent properties of these structures. The characteristic mark of an internal

relation is that it constitutes the parties to it. For instance, one cannot be a husband without a wife, or vice versa. Another difference follows: For the individualist, social structures are constituted by social interaction and they have no causal powers of their own. For the social holist, social structures do have causal powers and they determine the interaction of individuals occupying positions in these structures.

Social wholes with structures, in the above sense, have been discussed by Raimo Tuomela, who argues that they are compatible with ontological individualism, albeit of a special brand called “interrelationistic” individualism (1995: ch. 6). It may be argued that an ontological individualism which accepts a distinction between social positions and the holders of those positions is question-begging. Such ideas are usually associated with holism (see, e.g., Watkins 1953: 729). Tuomela’s ontological individualism also accepts the (vicarious) existence of social institutions and of society. It only denies their independent existence (Tuomela 1995: 356, 377ff). As Tuomela admits, this is a liberal version of individualism. The question is, if it is not vacuous. It is difficult to find a single social holist who maintains that social entities exist independently of individuals.

Tuomela also points to the close connection between the existence and causal efficacy of social entities. It is a generally accepted view that if entities have causal efficacy they exist. Causal efficacy may, therefore, be used as a marker of existence. Now, both common sense and serious reflection tells us that everything that takes place in history is the result of the actions of individual human beings, acting alone or jointly. This is the most basic intuition behind methodological individualism and it has to be accepted as an indubitable truth. If social entities have causal efficacy, therefore, this efficacy can be found only in the actions of individuals. “If a holistic social entity (such as a nation, organization, or a small group) is to have causal impact on the world, so to speak, surely that impact must at bottom be exerted and come about due to some concrete individual persons (and their actions, interactions, and various relevant relationships)” (Tuomela 1995: 362). It may be noted, though, that the causal impact of group members is an impact *qua* group members. It is, to a large extent, a joint impact and it is an impact of people in a social and normative setting and as occupants of social positions in a social structure. The conclusion of Tuomela is that social institutions do have causal efficacy and the same goes for social artefacts such as church buildings, cars, chairs, and books (Tuomela 2001: 130ff).

I have reviewed some arguments to the effect that the circumstances in which human beings make history may consist of social institutions and structures. But Marx also believed that history is made by collective subjects, such as classes, with particular interests and a consciousness of their own. What is the ontological status of entities such as these? For individualists, it is axiomatic that, in a strict sense, only human individuals have minds and intentions, and can act. There is reason to believe that Marx shared this view. In recent philosophy, however, there has been a lively discussion about things like collective intentions and plural subjects which has some bearing on the ontological question at issue here.

Raimo Tuomela has developed a general theory of social action and of society since the 1970s, which includes the notion of *we*-attitudes, especially *we*-intentions and mutual beliefs. The main statement of this theory, so far, is in the *The Importance of Us* (Tuomela 1995). A *we*-intention is an intention two or more people have of doing

something together, such as singing a duet or going to the cinema. What they do together is called joint action. We-intentions are based on agreement and a commitment to perform a certain joint action. The basic intuition behind Tuomela's notion of we-attitudes is that individuals sometimes think and act in terms of we. But it is still individuals who think and act. Tuomela's account of we-intentions and joint actions, therefore, is individualistic.

As we have seen, Searle assigned an important role to collective intentionality in his account of social reality. "By this I mean not only that they [people] engage in cooperative behaviour, but that they share intentional states such as beliefs, desires and intentions" (Searle 1995: 23). Collective intentions are crucially involved in the construction of social facts and they are irreducible to individual intentions. Searle sees a potential conflict with the requirements of methodological individualism (Searle 1995: 25ff, 2001: 24ff), but concludes that there really is no conflict, since collective intentionality exists only in the minds of individuals (Searle 1997: 427).

In contradistinction to Tuomela and Searle, Margaret Gilbert has developed a theory of plural subjects which aspires to go beyond individualism. She agrees that Tuomela's theory of We-intentions is individualistic, but argues that Searle's idea of collective intentionality is holistic in one sense and individualistic in another. It is "internally holistic," since it denies that "we intend" is an additive function of "I intend," but not "externally holistic," since it is, nevertheless, an additive function of "we intend" (2000: ch. 9). Gilbert's own idea of plural subjects is also externally holistic, since it implies that a plural subject is more than a plurality of individual subjects. A plural subject is a unity with beliefs and intentions of its own. Gilbert presented her idea of plural subjects in her seminal book on *Social Facts* ([1989] 1992), which includes careful studies of the ideas of three sociologists: Max Weber, Georg Simmel, and Emile Durkheim; and two philosophers: Ludwig Wittgenstein and David Lewis. The most important concept in this book is that of joint commitment. As we have seen, Tuomela analyzed joint actions as the result a commitment on the part of individuals to do something together. A joint commitment, however, is something more. It is a collective commitment to do something together and it is this joint commitment, which creates rights and obligations for the plural subject.

Conclusion

Recent developments in the debate about methodological individualism suggest that the strong version of this doctrine is untenable, while the weak version is more promising. Turning to the ontology of historical objects, a definite conclusion is much harder to produce. While the arguments for the existence of institutions and structures are quite persuasive, the ideas of collective intentions and plural subjects are less convincing, at least to the extent that they imply the irreducibility of these entities to the beliefs and intentions of individuals. Supervenience may be a more promising formulation of the relationship between the two levels. Further research into the ontology of historical objects may then use not just discussions of ontology from the philosophy of the social sciences, but also the study of supervenience in metaphysics and the philosophy of mind.

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Origins: Common Causes in Historiographic Reasoning

AVIEZER TUCKER

Inferences of common causes from their effects are ubiquitous in historiography: Evolutionary biologists infer knowledge about extinct species from similarities between their descendants and fossils. Historians and detectives infer descriptions of past events from testimonies. Textual critics infer properties of lost texts from present exemplars. Geneticists and comparative linguists infer the migration routs of our pre-historic ancestors from the present geographic distributions of genes and languages. Many philosophers (Reichenbach 1956; Sober 1988; Cleland 2002; Tucker 2004) agreed that much of our knowledge of the past is founded on inferences of common causes from their present effects.

Much of the philosophical literature on the inference of common causes originated with Reichenbach's (1956) quixotic attempt to deduce a principle of inference of common cause from the second law of thermodynamics. Reichenbach (1956) believed that when the probability of the conjunction of properties is higher than expected, there is a common cause that explains the higher than expected probability. For example, suppose that two friends suffer from food poisoning in 1 percent of the cases when they eat out. Without a common cause, one would expect them to develop together food poisoning in 1 in 10,000 cases of eating out together. However, if that probability is much higher, say, they always develop food poisoning together after eating in a specific restaurant, clearly there is a common cause, the shared meal. Reichenbach put it in symbolic notation, when A & B are probabilistic frequencies of a property or a set of properties in a population (such as the one percent of food poisoning), and when (P) denotes "the probability of" if $P(A \& B) > P(A) \times P(B)$, then there is a common cause C for A & B that satisfies the condition $P(A \& B | C) = P(A | C) \times P(B | C)$, when the vertical line $|$ denotes "given that." The whole principle reads then: The probability of the conjunction of probabilistic frequencies A and B , given common cause C equals the probability of frequency A given common cause C multiplies by the probability of frequency B given that same common cause C .

Reichenbach sought to deduce this common cause principle from the Second Law of Thermodynamics with the "hypothesis of the branch structure," referring to the branching of a physical system from the general direction of entropy in the universe (Reichenbach 1956: 157). Branch systems are isolated from the main system but are connected to it at their beginning and end; one end has low entropy and the other has

high entropy. “[I]n the vast majority of branch systems, the directions towards higher entropy are parallel to one another and to that of the main system” (1956: 136). Reichenbach considered these hypotheses as “empirical hypotheses which are convincingly verified” (1956). Since any physical system should display increasing entropy, cooling, disorder, however slowly, if and when we encounter increasing order, it must be caused by an intervention, a common cause, from without the system.

Reichenbach sought to demonstrate the direction of time from past to future by discovering asymmetries between past and future. “[S]ince the cause leaves traces in the effect, it can be inferred from its effect. Here is the origin of the distinction between past and future . . . although the past can be recorded, the future cannot, is translatable into the statistical statement: *isolated states of order are always postinteraction states, never preinteraction states* . . . if positive time is defined as the direction of increasing entropy” (1956: 155). “The cause produces the effect, the effect records the cause” (1956: 156). Reichenbach did not consider that some causes neither leave traces in their effects nor generate records. The extent to which the “future” records the effects of the “past” has been the subject of recent debate where both sides inferred from anecdotal cases conflicting conclusions about the possibility of knowledge of the past (Cleland 2002; Turner 2005; cf. the more cautious conclusions in Sober and Barrett 1992). Further, on the time scale of human history and affairs, entropy appears flat and descriptions of ordered correlations cannot be reduced to descriptions of rise of order in entropic terms and so do not imply a common cause (Tucker 2007).

Most interpreters of Reichenbach’s common cause principle have considered it an explication, a priori prescription with few illustrations that is not subjected to systematic empirical testing. The prescriptive aspect of explications of science renders the examination of actual scientific practices redundant because they either conform to the a priori prescriptive recipe, or approximate it, or are just bad science. Empirical evidence cannot *refute* an explication that is based on an a priori formal model. Rather than refute, empirical evidence can demonstrate the *irrelevance* of these explications for understanding how Darwin inferred that all the species of finches in the Galapagos Islands had a common cause or how Rasmus Rask and Franz Bopp’s inferred that all the Indo-European languages had a common cause. Further, aspects of the explications of the common cause principle are vague and incoherent (Tucker 2007).

The universe is replete with positive correlations between frequencies of events that do not result from any common cause. Correlations between properties such as having wings or fins may be homoplasies, result from independent adaptations to similar environments at different times and places (Sober 2001). Berkovitz (2000: 55) noted that there are plenty of correlations that require no explanation at all. For example, perfect correlation between independent series of coin tossing is highly improbable, yet requires no explanation.

Some Common Cause vs. a Particular common Cause

Reichenbach and his explicators did not distinguish the inference of *the properties of a particular common cause*, from the inference that *some common cause existed* without characterizing it further (Sober 1989: 281–2). It is one thing to say that men and apes

had some common ancestor; quite another to infer the properties of that ancestor. “In applying the principle of the common cause to examples, Reichenbach (1956) and Salmon (1984) often treat postulating a common cause and inferring the state of that cause interchangeably” (Sober 1989: 282, n. 3; cf. Sober and Barrett 1992). The histories of evolutionary biology and historical linguistics clearly display two stages: First, Rasmus Rask and Franz Bopp and Darwin established respectively that groups of languages and species had some common causes. A generation or more later, other linguists and biologists attempted to infer the properties of these common causes (Tucker 2004: 63–8, 85–91). Likewise, “the methods used in phylogenetic inference *assume* that the species surveyed are genealogically related. The question is to determine which tree is best supported by the data” (Sober 1999: 267). Darwin was certain that the Galapagos finches had a common cause-ancestor. But he did not surmise which properties possible common ancestors could have had.

Type vs. Token Common Cause

“A token event is unique and unrepeatable; a type event may have zero, one or many instances” (Sober 1988: 78). For example, if we share the same type of biological causes, the same type of sperm and egg, we most likely belong to the same species; if we share tokens of these causes, we are twins. Salmon (1984) and Hitchcock (1998) mixed common cause types with tokens in their interpretations of the common cause principle.

Hitchcock (1998: 427–8) distinguished insignificant “statistical correlations” from “probabilistic correlations” that warrant in his opinion the inference of common causes. He interpreted *statistical correlations* leading to the inference of a common cause *token* from *token* effects, and *probabilistic correlations* as leading to the inference of a common cause *type* from correlated *types*. Probabilistic correlations are of frequencies of a property or properties in a class. Statistical similarities between occurrences, without correlations between frequencies, do not indicate a common cause, claimed Hitchcock, they may be anecdotal. Hitchcock’s (1998: 436) example for the inference of a common cause type from a probabilistic correlation between types is of hypothetical languages like proto-Indo-European that should explain the correlations between classes of languages. However, in historical comparative linguistics descriptions of languages, hypothetical or historical are of tokens, processes that lasted for a definite period among a people spread over space. Hitchcock may have confused the meaning of “language” as a type studied by the philosophy of language or Chomsky’s generative linguistics with its meaning as a token studied by comparative historical linguistics. Historical linguistics infers common cause tokens from correlations among the most slowly mutating subsets of token languages that refer to places, fauna, flora, immediate family members, body parts and the first few numbers. Further, probabilistic correlations are insufficient for inferring the existence of a common cause since the world is full of insignificant correlations.

Sober (1988) and Hausman (1998: 207–8) interpreted the inference of a common cause as that of a token. “A common cause explanation postulated a common *token* object . . . That is why homoplasies induced by numerically distinct but qualitatively similar selection processes do not have common cause explanations” (Sober 2001: 339).

The inference of common cause tokens may well distinguish the *historical* from the *theoretical* sciences (Cleland 2002; Tucker 2004).

Information Preservation and the Inference of the Existence of Some Common Causes

It is impossible to describe and compare the infinite number of properties of any set of events or objects. Therefore, the common cause and its effects are best considered as *properties of events* rather than events (Sober 1999). Greg (1927) called classes of objects that share properties/ variables *variational groups*. Let's adopt this useful term and ask: how do historians infer that a variational group had some token common cause? This question needs to be specified further because as Sober (1988: 100) noted, any collection of events shares trivially some common cause token: at the very least, the Big Bang where our universe has originated. Most random collections of historical events would share many more common causes; all events in human history share the creation of the solar system, the mutation that gave rise to the human race etc., as common causes. Yet, the Big Bang and the birth of the solar system do not *explain* correlations between texts, languages, or species. Sober suggested that scientists explicitly or implicitly specify "contrast classes" whose members were not affected by the requested common cause. Contrast classes specify which common cause is relevant for the explanation of a variational group. For example, if we ask for a common cause to explain the correlations between the properties of dolphins and whales, we also imply a contrast class of, say, land mammals. Biologists examine then whether there was a common ancestor of dolphins and whales that was not also a common ancestor of land mammals. If we formulate the contrast class differently, the requested common cause would differ as well. Likewise, when textual critics compare texts and infer their common ancestors they group them according to certain variables that other texts do not share (Greg 1927).

The properties that scientists look for in variational groups in order to infer their common causes are those that tend to *preserve information*. For example, the exclusively male Y chromosome, and the exclusively female mitochondrion are passed respectively from father to son and mother to her descendants. Thus, they tend to preserve information respectively about male and female ancestors, the common causes of contemporary frequencies and correlations of Y-chromosomes and mitochondria, and are useful for the inference of genetic historiography. By contrast, each throw of the dice erases information about previous results, and so correlations between frequencies of results of throwing the dice are causally meaningless. The selection of variational groups according to their information preserving qualities is theory laden. Information transmission theories may be as complex as the combination of genetics with evolutionary biology, or as simple as those of corresponding sounds in linguistics, such as the English sound "W" and the German sound "V." Such information is *nested*, it can be inferred only with the aid of theories that link properties explicit in the information signal with information that is "nested" in it (Dretske 1981: 71–80).

Some processes tend to preserve in their end states information from their initial state more than others. Processes have varying levels of *fidelity*. Fidelity measures the

degree to which a unit of evidence preserves information about its cause. *Fides*, fidelity, is a term used by textual critics to evaluate the reliability of texts (Maas 1958). Fidelity is used in this context as *reliability* is used in probability theory or *credibility* in jurisprudence (Friedman 1987). Biblical criticism, classical philology, and historiography leaped forward following the recognition that oral transmission has a lower fidelity than written transmission, and that memorized prose has a lower fidelity than memorized verse. Historical linguistics leaped forward when it discovered that names of places, names of fauna and flora, and words for the first few numbers, body parts, and immediate family members have higher fidelities than other parts of language. Since there is sufficient evidence to establish continuous historiographies of some languages over thousands of years, it is possible to infer an average rate of fidelity across all these languages. The fidelity of this high fidelity vocabulary is according to Maurice Swadesh 86 percent per thousand years.

When attempting to infer a common cause, historians look first for high fidelity properties that are shared by members of a variational group. The evaluation of the fidelity of properties of events may also involve the examination of evidence for the causal chains that purportedly transmitted information from some common cause to members of the variational group, for example, the fossil record. *Ceteris paribus*, the more distant is a member of the variational group from the common cause on a causal-informational chain; the lower is its likely fidelity. Variations tend to multiply between source texts and their transcribed copies (Greg 1927: 9) and between genetic and linguistic ancestors and descendants.

The Meaning of the Existence of Some Common Cause

When historians infer that a variational group had *some* common cause, whose *properties are unknown or unspecified*, it may mean one of five kinds:

- 1 *A single ancestral common cause*: For example, a variational group of historical testimonies may be explained by a common event they describe or a common text they copied.
- 2 *Several common causes*: For example, the testimonies in the variational group may reflect identical common sources.
- 3 *The common cause may be a member or several members of the variational group itself*: For example, one or more testimonies may be copied by the others.
- 4 *All the members of the variational group may mutually cause each other*: For example, the “wave” theory of language suggests that variational groups of languages may be the descendants of a group of unrelated languages that influenced each other in a historical period when they were spoken by geographically adjacent peoples who influenced each other’s languages.
- 5 *Combinations of 1 or 2, with 3 or 4*: For example, recent research suggests that humans and chimpanzees had a common ancestor (1) but also that after a period of separation between the species, there was again a period of hybridization (4) followed by final separation. The greater similarity between human and chimpanzee X chromosomes than between autosomes indicates that later hybridization (Patterson et al. 2006).

Likelihoods of the Variational Group given Common and Separate Causes

Sober has proposed in a number of publications that scientists infer common causes by comparing the likelihoods of a variational group given a common cause and given separate causes (Sober 1999: 255–6, 2001: 242): *likelihood* means *the probability of something given something else*. For example, *ceteris paribus* the likelihood of change of government in democratic elections given an economic recession is higher than given a record rise in per capita income.

It is often difficult to establish *precise* likelihoods, but precision is unnecessary because *historians do not prove the high likelihood of the variational group given the hypothesis they favor, as much as prove its negligible likelihood given its sole alternative*. Historians utilize auxiliary evidence and theories, especially theories about the transmission of information in time and evidence for causal chains that connect, or not, common causes with their effects. The assessment of the likelihood of a variational group given some common cause depends on background information, auxiliary evidence about links in the causal chains that could transmit information from the hypothetical common cause to the variational group and theories about the preservation of information through its transmission from one link to the next. For example, a fairly rudimentary probabilistic biological theory that holds that organic like begets like is at the foundation of cladistic inferences in biology because it increases the likelihood of similarities between related species, given some common cause (Sober 1988, 1989, 1999). The discovery of evidence for links on the causal chains that connect some common cause with a variational group, such as transitional forms in evolutionary biology, increases the likelihood of the variational group given some common cause.

The proximate likelihoods of the variational group given common and separate causes must be multiplied by their estimated prior probabilities, their probabilities given everything else that we know, how coherent they are with everything else we know of the past. Historians estimate priors of common cause hypotheses by examining whether causal chains that extend backwards from the units of the variational group could have intersected, before they encounter the causal chains that extend backwards from the units of the contrast class. For example, if we wish to examine the prior probabilities of the hypotheses that the similarities between the English word “tea,” the Czech and Russian “čaj,” and the Hebrew “te” that other words in these languages do not share resulted from a common cause or from separate causes, relevant background information about the history of the words for tea and the drinking habit itself, would tell us that it was initially a Chinese beverage that was exported westwards. It is probable then that the causal-etymological chains that lead backwards from the words in the variational group converged in some common cause, some word in one of the Chinese dialects that was spoken during the European Middle Ages, though without further research one cannot know the properties of that word, what the original word for tea actually sounded like. Therefore the similarities between the above words for tea are more likely given a highly probable common cause than given separate causes. To take another example with the opposite conclusion, the hypothesis that ascribes the similarity between the Egyptian and Aztec pyramids to a common cause has very low prior probability because there is no evidence for intersection between causal chains that

stretched back from Aztec and Egyptian architecture. Given pre-modern means of transportation and information technology, the prior probability of such an intersection is vanishing.

The function, the adaptational advantage, or the rationality of the shared properties of the members of the variational group, may be decisive for deciding between common cause and separate causes hypotheses. For example, prior to the discovery of columns and arches, the only likely shape of a big and tall building is that of a pyramid, solid wide base that can support a lighter structure above it. This is the only functional solution that ancient Egyptian and Aztec architects could have devised for this type of problem. Separate tokens of this type of cause, the constraints on the shape of large buildings that do not use arches or columns, make the similarity between pyramids on both sides of the Atlantic highly likely. The prior probability of the separate causes depends on what we know of their historical context, whether indeed the builders did not know of columns and arches. Similarly, various cultures independently invented agriculture, the taming of domestic animals and the wheel because these are the best functional solutions for the universal human needs for food and transportation. *Homoplasy*, similarity of biological traits that results from separate but similar environments, different tokens of a type of cause, such as that of fish and sea mammals, is quite likely given separate causes. Natural selection favored wings and fins several times – though insects, birds, and bats; fish, ichthyosaurs (sea dinosaurs), and dolphins did not evolve out of each other, nor are they closely related to each other – because they are the best functional forms for organic movement through air and water.

Variational groups that share properties that have no functional value, or are even dysfunctional, such as grammatical mistakes in texts or redundant properties of species such as the feathers of birds that cannot fly, which Darwin called *rudiments*, are highly unlikely given separate causes. Imperfections and errors in transcriptions are the best evidence for common causes because the odds against separate processes arriving at the same dysfunctional forms are vast (Dennett 1996: 135–43). Similarly, Maas (1958) considered similar textual *anomalies* evidence for some common source. If there were equally useful *simpler* ways to express what the anomaly conveys, the anomaly must be the result of some common cause, a mutation in copying. The likelihood of any single grammatical mistake or non-adaptive trait is low; the likelihood of several identical ones, given separate causes, is vanishing. The likelihood of non-adaptive traits given some common cause, as low as it may be, is still significantly higher. Therefore, geneticists and textual critics alike cherish and assiduously search for them. This is the usual method by which scientists infer some common cause, *not by proving that it has high posterior probability, but by proving that the only alternative, the separate causes hypothesis, has a vanishingly low posterior probability.*

When the likelihood of each member of the variational group given separate causes is low, the effect of multiple members, such as several similar testimonies, is to decrease exponentially the likelihood of the variational group given separate causes. Therefore, evolutionary biologists, comparative linguists, as well as police officers and journalists, exert themselves to discover extra members of variational groups, such as testimonies. Multiple members of a variational group such as witnesses *do not increase the posterior probability of some common cause; rather they decrease exponentially the posterior probability of its only alternative, separate causes.*

For example, in the historiography of ideas, Skinner (1988: 46) compared the likelihood that one thinker like Locke was influenced by another like Hobbes (common cause), against him developing those ideas by himself independently (separate causes). Relevant evidence is of one thinker having read the other (information transmission): “the necessary conditions . . . for helping to explain the appearance in any given writer B of any given doctrine, by invoking the “influence” of some earlier given writer, A . . . at least have to include (a) that there should be a genuine similarity between the doctrines of A and B; (b) that B could not have found the relevant doctrine in any writer other than A; (c) that the probability of the similarity being random should be very low (i.e., even if there is a similarity, and it is shown that it could have been A that B was influenced by, it must still be shown that B did not . . . articulate the relevant doctrine independently).” Sometimes the results of such comparison of likelihoods remain controversial. For example, evolutionary geneticists debate the relative likelihoods of variational groups of genomes that share a single non-functional mutation, given common cause and separate causes. But usually, when the variational group shares more than a single variable, the inference of a common cause enjoys a consensus.

Alternative Common Cause Hypotheses

If the likelihood of the variational group given some common cause multiplied by the prior probability of a common cause given everything else that we know apart of the variational group is considerably higher than given separate causes multiplied by their prior probabilities, alternative common cause hypotheses compete over which is the best explanation of the variational group. This comparison between alternative common cause hypotheses proceeds in two stages: First, the five above mentioned (under the heading “The Meaning of the Existence of Common Cause”) possible causal nets, a single or multiple ancestral common causes, mutual influences among some or all the members of the variational group, or a combination of ancestral causes and mutual influences, compete over conferring the highest likelihood on the variational group. Second, once one of the five possible causal nets is chosen, historians may attempt to infer the actual properties of the common cause.

The comparison between the five possible causal net hypotheses requires the expansion of the scope of evidence beyond that of the effects of the common cause, the variational group. The distinction between hypotheses of common cause or causes (kinds 1 or 2) and those of mutual causal influences (kinds 3, 4, or 5) is between describing members of the variational group as *independent* of each other or not. *Independence of members of a variational group is the absence of intersection between the causal-information transmitting chains that connect them with their common cause.* Ascertaining whether the causal-informational chains intersected usually requires evidence for links on the causal-information chains that connect common cause events with their effects. Such evidence may prove that the causal-information chains could not have possibly interacted after their original common cause: for example, if witnesses were isolated from each other after the events they testify to. Evidence may allow historians to infer the history of a text, it may allow historians, biblical critics and classical philologists to reconstruct at least a part of the genealogies of variational groups of texts they study. When

evidence is scarce, more than one of the five possible kinds of common cause hypotheses may make the evidence equally likely. For example, in historical linguistics, though it is certain that the Indo-European languages had some common cause, it is impossible to determine whether it was a single language, proto-Indo-European, or whether several geographically proximate languages mutually influenced each other, or both, a single language had several descendants who later influenced each other.

If there is sufficient evidence to eliminate the probability of intersections between the causal information chains, or if there is sufficient evidence to “peel off” layers of mutual influence in the fifth kind of causal net, historians need to distinguish type one, a single common cause, from type two, multiple common causes causal nets. Relevant historical evidence may mention multiple sources, even if they are lost at present. Compilations of documents may preserve linguistic differences that through comparisons with other documents indicate different times and places of composition. Accordingly, historians and textual critics look for discontinuities in style, conceptual framework, and implicit values; internal contradictions, gaps in narratives, and parts that are inconsistent with the alleged identity of the author, all indicate multiple common causes.

Often there is sufficient evidence to determine which of the five possible common cause hypotheses is most probable. For example textual critics proved that the Bible and Homer are most likely given the fifth kind of common cause: the various exemplars of the Bible and Homer’s epics had initially multiple common causes, sources, and then they influenced each other in the process of editing. Such composite documents preserve linguistic and other differences that indicate different times and/or places of composition.

When it is possible to determine that there was a common cause or causes, historians attempt to determine which of the common cause hypotheses that specify their properties increases the likelihood of the evidence more than its competitors. They evaluate first the prior probabilities of competing common cause hypotheses, whether the hypotheses cohere with established beliefs as well as internally coherent. The likelihoods of variational groups given competing common cause hypotheses depend on theories of information transmission and preservation. In biology, when paired copies of a gene stop recombining, their sequences will increasingly diverge. A relatively small number of differences implies that recombinations stopped fairly recently; a larger number means it halted long ago. By comparing DNA sequences across species, biologists can often calculate roughly when species separated. The assumption of constant rates of mutation, which geneticists use in their inferences of common causes in the history of life is a statistical rule like: *All transmission of information (like languages, genetic data, and oral traditions) generates mutations, including errors in transcription, additions and subtractions. Ceteris paribus the average rate of mutation per medium per transmission is constant.* Constants of spontaneous variation would vary according to the type of information that is copied and the method of transmission/ copying.

Fidelities have the same value if we assume constant and uniform rates of mutation across species and languages. If we do not, or cannot, as in historiography, they may have different values. Beyond evolutionary biology, genetics and comparative linguistics, in historiography, textual criticism and detective work, fidelities are affected by myriad factors, most notably voluntary human agency, and so it is difficult or impos-

sible to infer average constant rates of mutation of information. The evaluation of fidelities in these sciences requires the examination of the causal information transmission chain that transmitted information from the common cause to the units of the evidence. Historians search assiduously for *primary sources, evidence that possesses the highest degree of fidelity by preserving more information that is relevant for a particular historiographic hypothesis than other relevant evidence*. For example, historians exclude evidence written by authors that were separated by time from the events they describe, unless there is evidence for a credible causal chain that could have transmitted information from events to evidence. Historians and detectives may assess the general fidelity of some sources according to whether they tend to cohere with other independent sources of high fidelity consistently. Historians recognize the high fidelity of parish registries, while they consider the official statistics of totalitarian states unreliable. Alvin Goldman (1999: 123–5) proposed that the fidelities of testimonies are evaluated according to the competence of the witnesses to detect the kind of information they offer and their record for honest reporting. Ranke assumed a theory of memory that implies low fidelity of memoirs and high fidelity of contemporary eyewitness accounts written immediately after the events.

The ultimate assessment of the fidelity of evidence has to consider all the above elements. Its complexity may account for the absence of algorithms for such computations in historiography or detective work. Historians, evolutionary biologists, and historical comparative linguists must examine the causal information chains that should connect hypothetical event with evidence. The more such evidence there is, the more certain is the assessment of fidelity. Therefore, paleontologists labor hard to discover missing links on the evolutionary tree, such as rare fossils, and highly value their discovery.

I outlined three consecutive stages of inference of common cause: (1) that there was a common cause rather than separate causes, (2) which of five causal nets of common cause is most likely, and (3) what were the likely properties of the common cause.

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Phylogenetic Inference

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Introduction

The task of reconstructing the history of life on earth falls, in part, to systematists. Practitioners of systematics study the historical pattern of evolution among groups of living things, i.e., phylogeny. Phylogeny is not directly observable; instead, it must be inferred from data that are incomplete at best. Systematists face a set of problems that is similar to those confronted by anyone who makes claims about the past: is there sufficient evidence? Given the available evidence, what questions can or should be asked and answered? What methods render the most reliable or justifiable conclusions? What are the limits of these methods?

While the shape of these problems is familiar to philosophers and historians, there are particular problems of inference in a field where evidence can be so scarce or difficult to interpret. We can observe fossils, which may give information about common ancestors of living taxa. However, some ancient forms presumably left no descendants, so systematists do not always know what to look for. This situation is compounded by poor or non-existent fossils of soft body parts, as well as by a spotty fossil record (Vermeij 2006; Sober and Steel 2002). There are, of course, other kinds of clues to phylogenetic relationships. The genetic, morphological, and behavioral characters of living taxa suggest patterns of ancestry. This evidence is sometimes problematic, as it is often difficult to discern whether these characters are similar because of common ancestry or for other reasons (Sober and Steel 2002).

So what can biologists meaningfully say about phylogeny? This is one of the central problems in systematics. Broadly, two different issues have been at the center of recent systematics debates: given epistemic limitations, whether *any* inference of phylogeny may justifiably be drawn; and given an affirmative answer, what methods ought biologists use to justifiably infer phylogenies, and what are the limits of these inferences? Here I will place these debates in their historical, scientific, and conceptual contexts.

There is general agreement in the systematics community that meaningful inferences of phylogeny may be drawn. Contemporary systematists are typically called "*phylogenetic systematists*" or "*cladists*" to reflect their commitment to the task of reconstructing these relationships (though, as will be seen below, debates over methodological tools have given rise to controversy over the application of these labels). Though the issue

of whether phylogeny reconstruction is a legitimate task for biologists is largely settled, the shape of that debate set the tone for contemporary conceptual debates in systematics. In the next section I describe what was at stake in that debate, and how it left the nature of phylogenetic inference as a central conceptual issue in systematics. With the important issue of *whether* systematics is possible largely settled, there have been several debates centered on *how* to go about it. Phylogeny reconstruction methodology, and the subsequent shape and justification of phylogenetic inference, are still very much live issues. I provide an overview of recent and current disputes in section three.

From Art to Science: An Introduction to Schools of Thought

A demand for rigor: numerical taxonomy

The publication of Linnaeus' *Systema Naturae* in 1735 may mark the beginning of modern systematics, and the publication of Darwin's *On the Origin of Species* obviously had a major impact on biologists. However, it is only rather recently that the broad goal of systematics has been to display the evolutionary relations of taxa using phylogenetic trees (figure 20.1) (Hull 1988). The *cladistic* school arose in the late 1960s/early 1970s and challenged what was known as *numerical taxonomy*, or *phenetics*. *Pheneticists* advocated using similarity algorithms as a method of classification of taxa. This involved quantitatively coding characters of the taxa in question (e.g., morphological traits such as fur length), then using a similarity matrix to cluster those groups that were most similar (figure 20.1b). Notably, pheneticists did *not* think that the evolutionary relationships of these groups could be validly inferred, and strove for what they claimed would be an objective classification based on observable character states:

Before proceeding, it is necessary that we clearly define our use of the term "numerical taxonomy." We mean by it *the numerical evaluation of the affinity or similarity between taxonomic units and the ordering of these units into taxa on the basis of their affinities.* (Sokal and Sneath 1963: 48, emphasis in original)

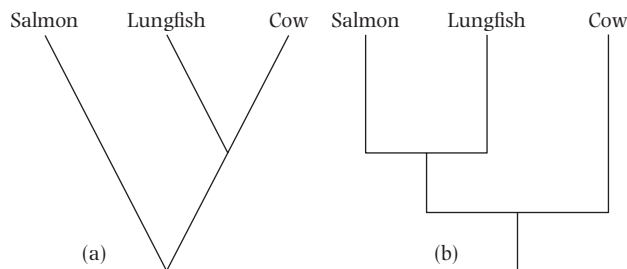


Figure 20.1 A phylogenetic classification (a) groups taxa by evolutionary relatedness, as defined by most recent common ancestor. The result is a cladogram displaying nested sets of clades. Phenetic classifications (b) produce phenograms that group taxa according to similarity indexes, and do not display evolutionary relationships

Numerical taxonomy arose in response to entrenched methods of taxonomy – what came to be known as evolutionary taxonomy. The primary criticism from pheneticists was that taxonomy was being practiced more as art than science. Pheneticists argued that taxonomists ought to strive for a *scientific* approach to taxonomy – namely, using methods that were both *objective* and *repeatable* (Sokal and Sneath 1963: 49). This amounted to a demand for rigor and articulation of method, i.e., a dispute over what could be justified as a scientific explanation or hypothesis.

For advocates of pheneticism, a scientific approach to taxonomy amounted to using transparent, explicit methods that produced repeatable outcomes that could be tested by anyone familiar with the methodology and with access to the data set. This stood in contrast to traditional practice in taxonomy, which pheneticists charged relied primarily on intuition and appeals to authority. Testability, repeatability, and transparency of methodology was tied to a notion of objectivity – in particular, to the notion that overall similarity between organisms is a function of the similarity of the characters being compared between the organisms (Sokal and Sneath 1963: 50, axiom 3). The pheneticist line was that incorporation of more characters would produce better overall similarity measures, and group taxa in the most informative way. (Notice, too, the emphasis placed on characters, as opposed to history. This concerns a related debate over the nature of biological taxa.)

The “objectivity” criterion that anchors the other scientific criteria identified by pheneticists is easily misunderstood. “Objective” was not meant to imply that the affinities between entities were theory free; simply that given some set of parameters and data, a mean similarity could be determined without resort to subjective interference (Sokal and Sneath 1963: 268–9). Whether the notions of “objective” advocated by pheneticists in theory were consistent with phenetic methodology in practice, and whether these notions are coherent is a matter of some controversy (Hull 1970).

Phenetic claims of objectivity should also not be understood as equivalent to staking out a realist position with regard to hypothesized taxa. These are independent issues, though often conflated; pheneticists could just as easily be instrumentalists as realists with regard to proposed taxonomies. That said, there was an underlying assumption in numerical taxonomy that selection and identification of characters was a strictly empirical procedure, and that theory should not enter into such selections. This is resonant in the resistance pheneticists exhibited towards partitioning of data into informative and non-informative, ultimately producing a clash with cladists over what counts as evidence:

Until and unless methods are developed for objectively assessing and quantifying the phylogenetic significance of character differences or affinities, the consideration of such information is incompatible with our stated aim of objectivity and repeatability for the taxonomic process. (Sokal and Sneath 1963: 55)

Objectivity: the phylogenetic perspective

Phylogeneticists argued that no such objective classification was possible based on the inductive methods of phenetics, and that inferences of evolutionary relationships of taxa could, indeed, be justified. Phylogeneticists followed Willi Hennig in taking the aim of

systematics to be the construction of phylogenetic trees that reflected nested sets of sister-taxa, that is, taxa that are descended from a common ancestor (figure 20.1a) (Hennig 1966). These sets of sister-taxa are *clades* or *monophyletic groups* (i.e., an ancestor and all and only its descendants), hence the term *cladistic analysis*. Phylogenetic analyses produce *cladograms* that display phylogenetic relationships, but do not necessarily convey any objective similarity between the taxa. This reflects the phylogenetic claim that morphological similarity alone does not necessarily correlate to evolutionary relatedness, and that only derived traits are informative in systematic analysis. Not surprisingly, phenetic and phylogenetic analyses of data can produce significantly different results (figure 20.1).

Indeed, cladist criticisms of phenetics call into question the entire notion of objective similarity in evolutionary analysis (Hull 1970). In its place, cladists argued that in a phylogenetic context relevant similarity should be understood in terms of shared, derived history. This argument amounts to the claim that absent some theoretically justifiable notion of relevance, similarity is an empty claim (Griffiths 1974; see also Goodman 1972). Specifying phylogenetic context as a theoretical basis for relevance provides content to claims of similarity. This is an instance of a more general issue concerning the nature of how similarity relations hold between theoretical models and designated systems (Giere 1988; Teller 2001; Callender and Cohen 2006).

The pheneticists' notion of objectivity was also called into question in a closely related debate over the nature of higher taxa. Many cladists embraced the thesis that taxa (including species) are historical individuals/systems, as opposed to classes (Ghiselin 1974; Griffiths 1974; Hull 1976). Committing to this tenet recasts much of the debate, *e.g.*, over what constitutes a "biologically interesting group" – characters or ancestry (Griesemer 2000; Hamilton and Haber 2006; Ghiselin 2007).

Cladists rejected the notion that objective similarity was something that could be discovered using quantitative analysis, instead advocating a systematic analysis justified by appeal to theoretically driven notions of relevance. Nonetheless, cladists *embraced* the notions of repeatability and testability as criteria of scientific methodology that must be met to produce satisfactory hypotheses of phylogeny. These criteria concern which techniques are suitable for inferring phylogeny; hence the shift in systematics from debates over *whether* to infer phylogeny to debates concerning *how* to infer phylogeny.

How to Infer Phylogeny, Or, Why Some Cladists Aren't "Cladists"

The phylogenetic technique of choice among cladists was broadly known as parsimony (Hull 1988; Sober 1988). Though there are some variants of parsimony, for the purpose at hand these can be ignored. Which phylogenetic tree is most parsimonious depends, of course, on what is getting counted. Proponents of parsimony analysis count evolutionary events, i.e., hypotheses of the evolution of a trait. The most parsimonious tree is that one that requires the least number of evolutionary events yet is consistent with the observed data – *viz.*, the distribution of characters across groups. Phylogenetic trees could, however, be ranked for parsimony based on other features. This is just to say that simplicity is not simply read directly off of these phylogenetic models, but is itself a claim in need of justification or explanation. This is reminiscent

of modern formulations of the problem of induction (Goodman 1955). All this leaves aside another, more fundamental, issue: the nature of parsimony in science. (See Sober 1988 for a discussion of the role parsimony plays in scientific reasoning, and Skipper (2002) and Plutynski (2005) for an example of how different positions in that debate might be applied to cases in biology.)

Many leading cladistic theorists initially justified parsimony techniques by explicitly appealing to Karl Popper's falsificationism (Wiley 1975; Eldredge and Cracraft 1980; Farris 1983). Briefly, falsificationism is a philosophical thesis about the scientific method. On Popper's account, scientific theories and hypotheses should not be evaluated on the basis of confirmation (or verification), but rather of falsification (Popper 1959). Experiments producing confirming evidence of a theory are of little to no value; only experiments that test scientific theories by seeking falsifying evidence offer evaluative information conforming to the scientific method. For Popper, the best theories are bold hypotheses that have passed rigorous attempts at falsification, and are, thus, corroborated.

Popper presented falsificationism as the scientific method that successfully solves the problem of induction. Cladists pressed falsificationism into service to solve the problem of phylogenetic inference, while simultaneously satisfying the criteria of scientific methodology inherited from prior debates in systematics. Phenetic methods were decried as confirmationist, while parsimony analysis was framed in a falsificationist framework (Wiley 1975; Farris 1983). The most parsimonious phylogenetic tree was said to be the most corroborated phylogenetic hypothesis, and a bold hypothesis of the evolutionary relationships between the taxa being studied. This phylogenetic tree was subject to being falsified if the discovery or addition of new characters revealed a more parsimonious phylogenetic tree.

The reconstruction of phylogeny using parsimony involves the construction of a tree-like (i.e., bifurcating) model to represent a section of the actual phylogenetic pattern of a historical lineage (e.g., new branches represent a speciation event). Note the importance of the distinction between the "true tree" and a "phylogenetic tree." The "true tree" is the actual historical lineage of life, (or part of that actual historical lineage). The "phylogenetic tree," on the other hand, is a historiographic model that systematists treat as a hypothesis about the structure of the actual lineage (Haber 2005). Parsimony provides a way to both construct and evaluate different hypotheses of a section of the actual historical lineage.

In the late 1970s, phylogeneticist Joseph Felsenstein discovered that lineages of a certain shape were subject to a systematic error in parsimony analysis (Felsenstein 1978, 1979, 1981, 2004). One example of this problem is called long-branch attraction (after the shape of the lineage described by Felsenstein; figure 20.2). In phylogenetic trees, branches connect nodes to other nodes or taxa. The length of these branches can be used to represent the amount of evolutionary change along a branch. In long-branch attraction, the taxa at the ends of long branches of lineages are mistakenly grouped together by parsimony instead of with the groups with which they actually share a more recent common ancestor (figure 20.2b).

Long-branch attraction is not merely a problem at an operational level, but also presents a challenge to the falsificationist underpinnings of parsimony. As more data (in the form of characters) are discovered or added to analyses, the previously most

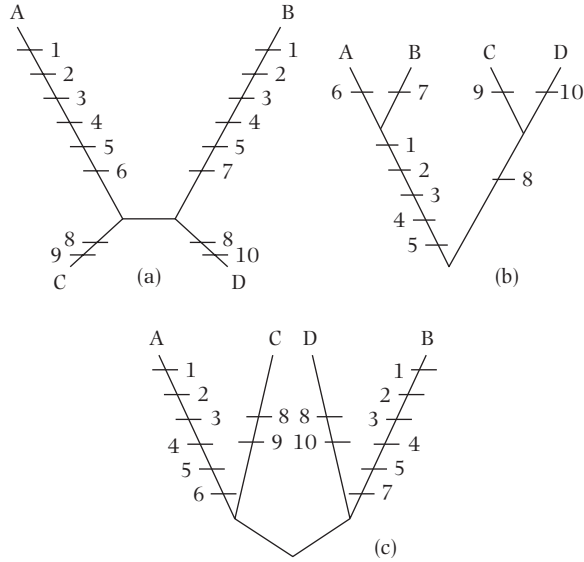


Figure 20.2 (a) represents the actual pattern of evolution (i.e., the actual historical lineage of A, B, C, and D), with the branch lengths indicating amount of evolutionary change. This tree is unrooted, i.e., it is not time directional. The phylogenetic trees at (b) and (c) are rooted, with extant taxa at the labeled tips. (b) is a parsimony analysis of the data from (a), whereas (c) is an ML analysis of the same data. Note that the evolutionary relationships displayed in the parsimony phylogenetic tree are not isomorphic with the actual evolutionary relationships. The long-branch error occurs in (b) in hypotheses based on the number of proposed evolutionary events needed to account for all the characters exhibited by the taxa. For example, hypothesis (b) would receive a parsimony score of 10, whereas (c) would receive a parsimony score of 16

parsimonious phylogenetic trees are subject to being falsified in favor of more parsimonious phylogenetic trees. These new phylogenetic trees, then, are held up as bold hypotheses and either corroborated or rejected in favor of ever more parsimonious trees that are subject to being tested. Felsenstein showed that as more characters are added, parsimony techniques become *more subject* to making a long-branch attraction error. This is because as more characters are added, it raises the possibility that there will be a long branch in the tree. In other words, parsimony techniques are more prone to rejecting hypotheses that correctly capture actual phylogenetic relations while corroborating less accurate but more parsimonious phylogenetic hypotheses – seemingly producing a systematic inferential error!

To avoid long-branch attraction, Felsenstein proposed using maximum-likelihood (ML) techniques to formulate phylogenetic trees (Felsenstein 1981). ML picks out the phylogenetic tree that has the highest likelihood value (conditional on the data); i.e., the phylogenetic hypothesis that confers the highest probability on the data is the phylogenetic tree given the highest ML value. Simulation studies have shown that ML methods are not subject to long-branch attraction (figure 20.2c) (Huelsenbeck and Hillis 1993; Hillis et al. 1994; Swofford et al. 1996; Huelsenbeck and Rannala 1997).

Many cladistic theorists did not welcome ML methods. A split formed in the systematics community between those that advocated using parsimony techniques exclusively, and those that advocated using statistical techniques such as ML. The former group appropriated the name *Cladists* (whom I shall call capital “C” cladists); the latter group identified themselves as *statistical phylogeneticists*. Statistical phylogeneticists typically consider parsimony methods to be just another statistical method (or, at least, as a method that can be construed as using or derived from statistical methods rather than from a falsificationist framework (Tuffly and Steel 1997; Sober 2004)). Cladists, on the other hand, do not consider statistical techniques to be valid forms of phylogenetic inference, and some question whether statistical methods conform to what they consider the “scientific method,” viz., a particular form of falsificationism (Farris 1983; Kluge 1997a, 1997b). The debate is primarily over the justification of techniques and subsequent inferences that may be drawn. Indeed, Cladists have argued that parsimony analysis itself should be rejected as unsubstantiated if formulated using statistical methods:

If reasoning from unsubstantiated suppositions cannot legitimately question parsimony, then neither can it properly bolster that criterion. The statistical approach to phylogenetic inference was wrong from the start for it rests on the idea that to study phylogeny at all, one must first know in great detail how evolution has proceeded. That cannot very well be the way in which scientific knowledge is obtained. (Farris 1983: 17)

Just as long-branch attraction poses a conceptual challenge to Cladists, the resistance to explicitly statistical techniques is grounded in conceptual concerns. At stake is whether particular methods satisfy some scientific criteria – typically, whether ML techniques satisfy falsificationist criteria or are best understood as embodying a verificationist scientific method (in which case, the argument runs, they ought to be rejected on principle). This dispute has two common fronts: (1) whether statistical techniques can be properly construed in a falsificationist framework; and (2) how to best understand falsificationism (Siddall and Kluge 1997; de Queiroz and Poe 2001; Kluge 2001; de Queiroz and Poe 2003). As a result, literature in systematics can read like Popper studies.

Another question is the extent to which the falsificationism espoused by systematists actually resembles that discussed in the philosophical literature (see Farris 1983; Hull 1983, 1999; Sober 1988). There do seem to be at least some important differences, which may have bearing on the debates over phylogenetic inference (Gillies 1990; Urbach 1991; Siddall and Kluge 1997).

Most contemporary philosophers of science are critical of the idea that falsificationism is the *only* acceptable method of science (Sober 2000). Many question whether it is even a very good thesis about scientific methodology (Howson and Urbach 1993), and most post-Quine and Kuhn philosophers of science have come to reject the thesis that any scientific hypothesis can be falsified in isolation. So it is perhaps somewhat surprising to philosophers of science that very few systematists have argued against the premise that falsificationism is *the* desired scientific methodology (though see Sober and Steel 2002, for an account of parsimony presented to systematists that is openly distanced from falsificationism). This situation is likely to change with the

emergence of a new group within statistical phylogenetics: *Bayesians* (Huelsenbeck and Ronquist 2001; Alfaro and Holder 2006).

Bayesian phylogeneticists, as the name would imply, use Bayesian phylogenetic techniques to construct phylogenetic trees. The purpose of this analysis is to distribute posterior probabilities (probabilities that take into account evidence and prior knowledge) over a range of possible phylogenetic trees. Bayesian phylogeneticists use what are known as Markov Chain Monte Carlo (MCMC) algorithms to approximate the rational distribution of these posterior probabilities that lead to the selection of which phylogenetic trees (or consensus trees) are most probable (Larget and Simon 1999; Huelsenbeck and Ronquist 2001; Huelsenbeck et al. 2001; Huelsenbeck et al. 2002; Alfaro and Holder 2006). Though non-Bayesian statistical phylogeneticists typically consider Bayesians fellow statistical phylogeneticists, some harbor skepticism towards the Bayesian phylogenetic methods. Indeed, this points to what will likely constitute the most important debates concerning phylogenetic inference: (1) the nature of specification and distribution of relevant “priors” for phylogenetic analysis, i.e., how do biologists evaluate the prior probabilities of phylogenetic trees prior to considering new evidence that causes the posterior redistribution of probabilities; and (2) the use of these posterior probabilities as justification of phylogenetic inference (Alfaro and Holder 2006). Another challenge that Bayesian statistical phylogeny shares with philosophical discussion of causation in general is the assumption of Markov conditions; namely, that “A variable represented by a node in the Bayesian network is independent of all variables represented by its non-descendent nodes in the Bayesian network, conditional on all variables represented by its parents nodes” (Bovens and Hartmann 2003: 69). This assumption has often failed to hold in the history of life because of hybridization, the merger of two species. Life, in other words, is causally “incestuous.” Accounting for hybridization is a challenge for all methods of phylogenetic analysis. Though each method has particular strategies for accommodating this, typically it simply gets ignored. That is, phylogenetic models, like scientific models more generally, include simplifying idealizations, one of which is the assumption that branches split, but do not coalesce. Once again, philosophers will recognize these issues as familiar, and will have the opportunity to constructively contribute to how these debates unfold in systematics.

Summary and Synthesis

Systematists widely agree that reconstruction of phylogeny is a central task in systematics. As a result, justification of phylogenetic inference is a central conceptual issue for systematists. Broadly, three major schools have emerged in modern systematics, each tackling the problem of phylogenetic inference differently: Cladists, statistical phylogeneticists, and Bayesians. Cladists espouse using parsimony analysis on falsificationist grounds, looking to Popper for a solution to the problem of inference. Statistical phylogeneticists advocate using ML methods, relying on classical statistical techniques (which may or may not be grounded in falsificationism) and simulation studies. Bayesians aim to incorporate inference more directly into their phylogenetic hypotheses, explicitly displaying prior assumptions and posterior probabilities.

One of the central problems to be tackled is how to adjudicate among the products of the various methods in phylogenetics. These methods reflect different philosophical stances that may be staked out by scientists concerned with historical events, reflecting a difference in how various research goals and problems are valued and prioritized. What makes a hypothesis about the past best or better? Hypotheses that more closely resemble history as it in fact unfolded, hypotheses that conform to methodological criteria, or hypotheses that present clearly testable explanations of particular historical phenomena?

One possible model for sifting through the issues raised by competing methodologies in phylogenetics is offered by Wilson and Sober's (1989) *competing conceptual frameworks*. Rather than conceive of competing phylogenetic techniques as hypotheses in direct competition, they may be better thought of as competing conceptual frameworks. Whereas competing hypotheses are exclusionary (i.e., if one is right, then all others must be false), competing conceptual frameworks should be understood as competing differently. Competing hypotheses share a vocabulary, i.e., the meaning of theoretical terms is consistent across competing hypotheses. Competing conceptual frameworks, on the other hand, may share *terms*, though these terms may have very different meanings in different frameworks, i.e., there may not be a shared vocabulary. So though at a surface level competing conceptual frameworks may seem exclusionary, they are often, in fact, simply making very different claims about the same system. As a result, the relation between competing conceptual frameworks might not be contrary or contradictory, but complementary.

The notion of competing conceptual frameworks begins to capture the state of the art in systematics, for to truly understand what a phylogenetic hypothesis amounts to, one must have a deep understanding of the nature, scope, and strength of justification that a particular method imparts on that hypothesis. It is for this reason that though two phylogenetic trees may share a topology, if produced from different methods they must be taken as conveying different hypotheses about phylogeny. Though the topology of the trees may be inter-translatable, the same may not be said of the inferences drawn from those trees (*pace* Sober 2004).

Note that in all three phylogenetic schools of thought the hypotheses of phylogeny are conditional claims. Phylogenetic systematists are all too aware of the epistemic limitations they face, hence the heated debates over the proper justification of phylogenetic inference. Philosophers have paid some attention to systematics, but not nearly to the extent that the material – or even the scientists – demands. Systematists have been openly engaging in philosophical discourse in the biological literature, but have very different research interests and agendas than philosophers. There is a rich story here, waiting for philosophical analysis.

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Historicism

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The term historicism was coined in the late nineteenth century and came to have widespread use and to be the object of controversy by the 1930s. For example, Edmund Husserl's 1911 essay "Philosophy as a Rigorous Science" (1965) specifically devotes a section to criticizing the doctrine of historicism (among whose proponents Husserl includes Wilhelm Dilthey). Since the meanings of historicism in philosophical use have become more complicated and diverse over time, this entry proceeds cautiously to outline three meanings of historicism. I do not choose one of these meanings as the term's central one and I have more to say about the issue of whether there is a central concept of historicism in the conclusion, along with criticisms of what I consider some past and current misuse of the term.

The first use I discuss below is under the title heading "Historiographic Concepts" and it is found in Heinrich Rickert (1962, 1986). Rickert contrasts historicism and naturalism as two general viewpoints on the nature of reality within his investigation of the conceptual preconditions for the sciences of history and nature.

The second use of the term as a philosophical position is found in Karl Popper (1961) and F. A. Hayek (1955). They both use the term to refer to those thinkers who accept the existence of laws of history allowing for predictions and explanations in historiography. Popper attributes historicism to, among others, Mill and Marx. I call this section "Historical Laws."

Finally, there is an idea that is less often explicitly associated with the term in which historiography is characterized as a matter of understanding or interpreting events and treated as distinct from the aim of explanation or prediction within the natural sciences. The classic expression of this third meaning of historicism is found in Collingwood (1956), Croce (1960), and Dilthey (1962); among these three only Croce uses the term. He calls his position "absolute historicism" for a reason discussed in the conclusion below. I call this third section "Historiographic Interpretation."

This third sense of historicism has, in recent years, come to be what most commentators mean by it, but with qualifications discussed in my conclusion. It is, for instance, the position that Peter Winch (1990) defends in his attack on social science as a whole and this third sense now names a contemporary movement in literary theory (Veese 1994). However, if this meaning of historicism is not understood alongside and perhaps in contrast with the other two, readers will be perplexed and

confused when reading more widely in the philosophies of historiography and social science.

Historiographic Concepts

Heinrich Rickert was a leading figure in the philosophical revival of Kantianism in Germany at the end of the nineteenth and early twentieth centuries. Though not uncritical of Kant, Rickert embraced Kant's idea that philosophy is fundamentally the theory of knowledge rather than ontology, and he saw philosophy's central task, as Kant had, as the clarification of the a priori conceptual preconditions for knowledge and thus for the possibility of metaphysics.

Rickert's central arguments, however, are against naturalism which he considered to include empiricism, evolutionary biology, and psychology. But specifically Rickert targets the views of John Stuart Mill (1888) for whom the natural and social sciences share a common methodology and common objects of study. Mill presents a system of unified scientific laws comprehending both nature and culture. Rickert's opposition to Mill is not an opposition to the sciences, nor does it follow some kind of dualism about the reality of nature and culture. Rather, the key difference is that Rickert was a transcendental idealist with regard to the different objectivities forming the a priori conditions for any possible experience. In other words, Rickert held that the objectivity of nature and history are each made possible by distinct a priori concepts of understanding. Thus for Rickert, as for Kant, the natural world is knowable only because there are forms of intuition, the a priori preconditions of space, time, and causality, making any experience of empirical objects possible. Rickert extended this style of argument to the cultural world where he concluded that the objects of historiographic knowledge require distinct a priori conditions of objectivity.

With respect to the two conceptual frameworks of naturalism and historicism Rickert defends two conclusions. First, historiography is an empirical science of causal processes, but it proceeds by way of conceptual preconditions for objectivity distinct from the forms of objectivity or causation of the natural sciences. Rickert held that the natural sciences study objects as natural kinds whereas historiography studies objects as unique individuals. It is this point that leads him to conclude that causation is distinct between these two domains. "History . . . with its individualizing method and its orientation to values, has to investigate the causal relations subsisting among the unique and individual events with which it is concerned. These causal relations do not coincide with the universal laws of nature . . . the selection of what is essential in history involves reference to values even in the inquiry into causes . . . for the realization of valued ends" (Rickert 1962: 94).

Second, Rickert defends the view that the study of philosophy is autonomous from both historicism and naturalism. The "logical-epistemological" task of philosophy, as Rickert calls it, does not depend on the results of historiographic, psychological, biological, or physical inquiry. I stress both of Rickert's conclusions about historicism and naturalism because, as will be shown below, some versions of historicism as "historiographic interpretation" reject both of Rickert's conclusions.

Rickert's most influential idea is then this distinction between preconditions for natural kinds in natural scientific generalization as against the concept of the unique individual as the basis for cultural science. Rickert's distinction is sometimes conflated with Wilhelm Windelband's distinction between "nomothetic" and "idiographic" inquiry. It is a distinction Windelband also claims separates natural and cultural sciences. Rickert explicitly cautions, however, that Windelband's distinction is quite different from his own. According to Rickert, Windelband's idea of an idiographic science confuses objectivity with the notion of forming a complete picture of a single object. Rickert's point then is that the historiographic sciences no more picture individuals than the natural sciences picture natural kinds. He thinks objective knowledge of states of affairs is not the production of a copy of the object in the mind. Rather, the critical difference resides in the concept of the object that makes possible knowledge of the object as unique and specific in historiography. Rickert thus says of Windelband's view; "There is just as much a *problem*, and just as little a *solution* to the problem in saying, with Windelband, that the procedure of the historical sciences is 'idiographic' " (Rickert 1962: 85).

Since for Rickert the objects of natural and cultural science are distinct, then scientific generalization as well as causation functions differently in both. This point is a matter of philosophical clarification since it falls to philosophy to investigate the a priori conditions that make possible the type of conceptual generalization possible for any object. It thereby follows from Rickert's transcendental critique that laws of history or culture are conceptually impossible or, as he puts it, "logically absurd." Therefore, while an empirical science of history with causal explanations is possible, such results could not lead to the discovery of any historical laws for the same reason that they could not allow for the discovery of any social or historical kinds. While studies of historical objects are causal, these results could not be evidence for historical laws. For Rickert, historicism is the framework within which a science of history is possible but within which neither historical laws nor historical kinds are possible.

There are two main objections to Rickert's idea of historicism and the associated claim of the impossibility of historical laws. The first problem with Rickert's position concerns the strength of his conclusion. He must demonstrate that historical laws are conceptually or logically impossible. But Rickert only argues that the historian's object of study concerns the uniqueness or individuality of cultural or historical objects. Assuming for now that Rickert's claim about the conceptual preconditions of objectivity is defensible, it is not enough to show that historical laws are impossible. Rickert has only shown that historical laws, whether there are any or not, are irrelevant to what he claims makes historiographic knowledge possible. That conclusion is far from establishing the logical absurdity of the concept of a historical law. Also, since Rickert grants that historians study causal interactions, he has not shown how it is conceptually confused for historians detecting patterns in such causal interactions to see them as evidence of law-like regularities.

Second, Rickert's argument turns on the difference in the function of generalization within naturalism and historicism. But scientific laws in either domain are based on universalization, not generalization. Thus scientific laws of any sort can include specificity about the objects covered by the law; thus the unique, specificity of objects

does not alone preclude law like regularities. Therefore, Rickert's argument for distinguishing uniqueness from generality in the sciences of nature and history does not establish either that there are two distinct sciences or that historical laws are impossible.

Historical Laws

With the publication of *The Poverty of Historicism* (the book was written in 1935) Karl Popper (1961) reintroduced historicism into philosophies of historiography, history, and the social sciences. Yet Popper's writings added considerably to confusion about the term. In part this confusion was due to some persistent misunderstandings of Popper's views. For instance, he was widely misread in the 1950s as a positivist, and often his social and political philosophy (which is where he discusses historicism) was read without a proper grasp of his core views about philosophy and philosophy of science (D'Amico 1989; D'Amico 1990–1; Wilkins 1978).

Since Popper likely knew of Rickert's work, it is somewhat mysterious that he chose to use the same term but give it an opposite sense. For whatever reason, Popper chooses to define historicism as the position that there are historical laws, and that such laws allow for a science of history involving prediction and explanation of historical events. Popper's prime examples of historicists are Mill and Marx, though he thinks it is a view widely held by social scientists and intellectuals. Popper also links historicism to what he calls "methodological holism." He means that some historicists also hold the view that the study of history involves postulating the existence of collective entities or subjects such as labor, capital, the state, or the church. It is then a methodological assumption that these collectives act as individual agents. Popper adheres to methodological individualism that denies the possibility of collective agency. He traces the belief in collective agents back to Hegel's absolute idealism. Popper, like Hayek (1955) and Husserl, also invokes holism as part of the charge that historicism is a type of relativism because it holds that the truth of historiographic claims and the validity of historiographic concepts are relative to different cultures and historical periods. As Hayek characterizes that view: "All concepts of individual phenomena, according to this strict historicism, are to be regarded as merely historical categories, valid only in a particular historical context" (Hayek 1955: 75).

Popper's central argument against historicism turns out, somewhat like Rickert's, to demonstrate that historiography cannot be scientific in the same manner as a natural science. But Popper does not claim that historical laws are conceptually impossible nor does he accept the notion of two distinct conceptual frameworks or methods for the study of history and nature. For example, while Popper's language echoes Rickert when Popper distinguishes between "generalizing sciences" and "historical sciences" Popper explicitly defends the hypothetical-deductive method as common to the natural and social sciences. Thus, for Popper, any scientific explanation or prediction must contain at least one universal, law-like premise.

Given this understanding of Popper's view, his argument could not lead to the conclusion that historical laws are conceptually impossible. Popper must intend a weaker objection. Either he must intend that should historians produce any explanations, the

universal premises within their explanations would be analytic truths rather than scientific laws. Or he must intend that historians do not in fact produce explanations at all.

Popper adopts both of these critical approaches. First, he argues that while there are universal claims wherever there are candidate historiographic explanations, these universal claims are trivial and unimportant. His examples are such claims as “power corrupts” or “scarcity of resources is accompanied by competition” or “larger, better-equipped armies normally win more battles.” Popper dismisses such premises as analytic, trivial, unimportant, or not testable.

Second, Popper argues that there are in fact no historiographic explanations in any strict scientific sense and thus there is no historiographic knowledge. He concludes that the best the historian can do is to produce “general interpretations” of history. General interpretations are not capable of being tested, however, and thus there will always be alternative, inconsistent interpretations, consistent with whatever documentary evidence can be found. Though Popper does not entirely dismiss the value of crafting such interpretations, and includes his account of the clash between open versus closed societies in history as an example of such a general interpretation, he makes clear that he does not consider such work knowledge. Ironically, Popper’s recommendation that the study of history can admit of only general interpretations is now called, by some, historicism.

Accepting Popper’s use of the term historicism for now, I think Popper has given little support to the charge that historicism is a kind of error or fallacy. First, Popper’s claim that the candidate laws are trivial simply begs the disputed question. The charge of triviality is largely based on Popper’s formulation of some candidate historiographic generalizations, but more importantly the charge falls short of what is needed. He would have to show that any and all historical laws are trivial, not just these candidate generalizations. But Popper gives no independent reason for agreeing with him that these formulations and all others are in principle incapable of becoming historical laws.

Popper defends his negative conclusion by noting how often the universal premises of historians when fitted into the hypothetical-deductive method can be formulated analytically. The same comment could be made for candidate explanations within the natural sciences. It is a feature of the hypothetical-deductive method that required universal premises can be formulated as law-like or analytic. But that alone does not allow Popper to conclude that the universal premises must be analytic when used in historiographic explanations. He could do so only if he already had a reason to think historical laws were not possible.

Popper’s second approach defends general interpretations rather than historiographic explanations. As defended by Popper, it suffers a serious flaw: If historians pursue Popper’s methodological guide for historians, it does not show that there are no historical laws. Popper accepts at least the possibility of sociological or psychological laws. If those laws are possible, and thus not analytic truths, then Popper has not identified an error or fallacy committed by the historian who accepts both the possibility of psychophysical scientific laws and general interpretations.

While Popper’s use of the term historicism is somewhat idiosyncratic, his conclusion that historiography requires a method of interpretation through what he calls “situational logics” deserves more attention than it has received.

Historiographic Interpretations

Popper thinks that since general interpretations depend on holism there is no way to adjudicate among them. But before deciding the limits of interpretation, I turn to a philosophical treatment of interpretative inquiry as unique to historiography and yet capable of producing knowledge. After all, experimental testing is only one means for the justification of knowledge and it cannot, without argument, claim to be the sole mean.

This discussion is now most often associated with the term historicism, even though the central proponent of it in my discussion never uses the term historicism to characterize his view. R. G. Collingwood (1956) is the key figure because he provides the most ambitious philosophical defense of this doctrine. He also writes about these issues from a deep familiarity with German idealism and the historiography of the eighteenth and nineteenth centuries, and he presents the ideas that have now come to be historicism as part and parcel of philosophy of mind and metaphysics, as Rickert had also tried to do.

Collingwood agrees that historiography and the "sciences of human nature," as he calls such disciplines, are distinct from and not analogous to the natural sciences. Collingwood's central defense of this conclusion depends on a distinction between actions and events which he explicates by way of a metaphor concerning the "inside and outside" of events.

By the outside of an event he means whatever it is about an event that can be described only in terms of movements and changes of state. By the inside of the event he means that which can only be described in terms of thought contents. The historian is concerned with actions that present themselves as unities of the inside and outside of an event. Therefore, human actions are movements and changes of state of human bodies, but they are caused by the thought contents of the agent acting. In the rest of nature this distinction of inside and outside does not arise. Collingwood (1974: 25) makes this point when contrasting the historian and scientist on events; "Instead of conceiving the event as an action and attempting to discover the thought of its agent, penetrating from the outside of the event to its inside, the scientist goes beyond the event, observes its relation to others and thus brings it under a general formula or law of nature." In this manner Collingwood crafts a new and more defensible version of Rickert's basic contrast.

Collingwood makes several important points about this proposal. First, he stresses, as did Rickert, that the study of history remains a causal study. But the only relevant type of causation for the historian is that by way of thought contents motivating agents and in that manner bringing about further historical events. Second, the historian's task is to determine by way of reenactment the thoughts of the person studied. Collingwood means literally that the historian must reenact through a reconstruction the same thought content as those agents under study. But this achievement is neither a psychological exercise nor a mere matter of empathy. Rather, it is empirical inquiry by way of the totality of available evidence that allows the historian to reenact intellectually the same propositional contents held by the historical agents being studied. Each historian through asking whether those thought contents would cause those actions under those conditions can then be said to test these results.

Third, Collingwood stresses that not all human events are thereby candidates for historiographic inquiry. Some events may simply turn out to be no more than

psychological or physical events. As Collingwood puts it; “Thus the historian is not interested in the fact that men eat and sleep and make love, and thus satisfy their natural appetites; but he is interested in the social customs which they create by their thought as a framework within which these appetites find satisfaction in ways sanctioned by convention and morality” (Collingwood 1981: 27).

In effect, Collingwood reaches the conclusion that the historian produces reenacted understanding or interpretation of historical actions of others, and these results produce knowledge. For example, Collingwood asserts that to understand a historical event is already to know why it happened. There is this immediate explanatory success since for the historian to understand is for the historian to have already identified the thought content that brought about the event. Once that is grasped as such, there is no further empirical inquiry needed to claim to know that was the cause of the action. Also, this result is not trivial because the historian is responding to the totality of available evidence. Such a result might be misunderstood as trivial, but that would be because explanatory success in historiography proceeds quite differently from that in natural science. In Collingwood historiographic knowledge rests upon the understanding of concepts in a way distinct from the rest of science.

Finally, Collingwood does embrace holism, but only from within the constraints of reenactment. Historiographic explanation requires thinking the same thought contents as a historical actor, not simply describing behaviors or using the same words. But then to grasp the same thought requires reconstructing the context of the thought. If an event has an “inside,” then it has a cultural meaning which requires this broad contextual investigation. But Collingwood does not think this form of holism entails relativism.

Collingwood’s philosophy gained renewed attention through the work of William Dray (1957, 1995). But despite its attractiveness, this view entails a cost. Collingwood narrows the tasks of historiography drastically. He simply dismisses wider historiographic research into the material conditions or non-mental activities of the past as a misconception and he treats psychological and sociological laws as natural scientific laws, thus further narrowing the application of his arguments.

Finally, Collingwood’s mature defense of reenactment seems at odds with his idea of historiography as the study of concrete individuals. Collingwood began his career with views very close to those of Rickert and Windelband. But when he developed his doctrine of interpretation, historiography was reconceived as the study of thoughts and ideas only. Thus the study of individuals is no more than the study of the ideas those people hold. In effect, whether minds are past or present becomes irrelevant to Collingwood. Historiographic facts are but means to reenactment and understanding within the present of ideas imputed to agents in the past. Collingwood therefore does not clarify how historiography could be knowledge of what happened in the past and yet only consist of understanding various thought contents in the present.

Conclusion

The proliferation of different and conflicting uses of the term historicism is not unique. Such intellectual labels as positivism and structuralism exhibit similar confusions and

their use changes. But in the case of historicism there is a question whether any common position or doctrine can ever be identified with this term. This concern has not stopped some commentators, most importantly Georg Iggers (1973, 1995) and Maurice Mandelbaum (1971), from attempting to weave a single story about historicism, bringing together its various uses either by crafting a central common concept or presenting them as a single debate about the meaning of historiography. These efforts, though rich in scholarly detail, face two problems.

First, the idea of a central meaning for historiography turns out to be somewhat anemic. Mandelbaum suggests that it is the viewpoint that concepts or phenomena can only be understood within historical development. Mandelbaum furthermore identifies as a historicist any thinker who treats historiography as important, significant, or capable of rigorous inquiry. But these suggestions cast too wide a net. The problem is that it is hard to exclude any major thinker from this label, and if any are excluded, it seems arbitrary.

Iggers published two surveys of the ideas associated with the term and traced its varied uses. As does Mandelbaum, Iggers distinguishes broadly between cases where historicism stands for a kind of relativism and those where it refers to late nineteenth-century German historiography. Iggers' point about the term as used to identify a movement in historiography, while important, nevertheless, fails to identify a common philosophical position on the nature of historiographic knowledge. Also, in the course of these treatments both Iggers and Mandelbaum use the notions of understanding and knowledge of history as though those concepts were held in common rather contested among those they discuss.

Second, the explicit use of the term as a philosophical concept, dating from the early twentieth century, was clearly as a term of art giving those who coined it permission to shape it to their own purposes. But therefore commentaries on this topic should be cautious and resist finding a single central meaning for what seems more properly a family of problems. The term once coined can then be simply attributed to past thinkers who of course never used it. But if Vico, Herder, and Hegel are historicists without realizing it, the claim of a centuries-long debate about this idea is illusory.

Does my summary leave anything to be said about the concept itself? Historicism is a set of reasons for marking off the study of history and culture from the natural sciences in terms of theory of knowledge and/or the nature of the object studied. However, historicism, even in the version of historiographic interpretation, is not properly characterized as rejecting causal analyses in historiography or explanation by empirical evidence. Therefore, Popper's pessimism that no general interpretation can be tested against empirical evidence is somewhat premature. However, historicism does place adjudication by evidence after conceptual understanding, comprehension, or reenactment.

The rational reconstruction of the objects of historiographic inquiry is also not committed to any strong doctrine of holism. For instance, it does not require holistic entities in history, such as group minds, corporate persons, or world spirits. But it does adopt the position that actions are caused by the intentional mental states of agents. While historians then may speak of collective forces in history, they do so only as shorthand for discussing large-scale historical movements. But this shorthand device does not thereby refer to a supra-individual mind.

It is worth ending this chapter objecting to the two less systematic uses of the term historicism already encountered. First, the use of the word often amounts to an embrace of relativism. Georg Iggers notes this point when he defines one central type of historicism as referring to “the philosophical debates . . . which equated historical knowledge with relativism and saw in relativism an existential problem which needed to be solved if civilized life was to continue” (Iggers 1995: 137). Also Croce’s coining of the phrase “absolute historicism” is intended to signal that Croce’s position is not relativism. Finally, it is the repeated objection to historicism by Popper, van Hayek, and Husserl.

Of course one can criticize a thinker for either encouraging or supporting relativism. H. P. Rickman discusses such charges against Dilthey in the section of his introduction to Dilthey’s writings titled “Historicism” (Dilthey 1962: 51–9). But it would be a mistake to simply treat historicism as another name for relativism. As shown above, the three senses of the doctrine I reviewed do not require a commitment to relativism nor would relativism follow from their claims about historiographic knowledge and objectivity.

The second common misunderstanding is that this position is a form of skepticism. For example, Husserl concluded that “historicism, if consistently carried through, carries over into extreme sceptical subjectivism. The ideas of truth, theory, and science would . . . lose their absolute validity” (Husserl 1965: 125). But such a claim ignores that each version of the doctrine summarized above is about the possibility of knowledge of history. While it is correct that some thinkers, Popper for example, are skeptics about the specific possibility of knowledge of history, they are at least consistent in treating historicism as claiming the opposite. Debates about philosophical skepticism are far from settled, but historicism is properly understood as a family of arguments for thinking knowledge of the past is possible, assuming any knowledge is possible. Even if the view should be criticized for failing to make its case for historiographic knowledge, it still would not properly be skepticism.

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Ethics and the Writing of Historiography

JONATHAN GORMAN

Historians can trace the moral features and concerns of past times. They can write historiographies of moral change, both of practices and of theory. They can themselves judge morally, and approach in evaluative ways, the actions and situations of the past. Their own activities, like the activities of all of us, are subject to ethical consideration.

Why would historians have any interest in the moral realm which would justify our paying special philosophical attention to their situation? If we as philosophers try to stand outside our moral situation and characterize it, then historical change, inevitable plurality, and continual controversy are more striking features than any philosophical or practical success in unification. Historians at least seem well placed to tell us what these changes have been.

Yet how would historians write such historiography? They would need to have in advance some idea of what it was they were writing about. They would need some way of organising the complexity of moral material. They might have to decide, to take just one example, whether it was the (narrowly conceived) moral or the legal which mattered to them, but they would then have to have determined that there was a relevant difference between these. Yet they would know – or discover – that the moral/legal distinction which we make easily today was perhaps not available at all under, for example, a monarch with divine right. Should they, if writing about morality in such a period, make that point? One reason for doing so would be to clarify understanding of the past for their present-day readership. This could be in itself a moral requirement: if historiographic truth is available, and if historians purport to supply it, then they have to do so in terms which are successfully communicable, and this requires tailoring their material to the readership in question. Historians would then need to approach their past material bearing in mind some summary organization of our present-day moral understanding appropriate to their readership. For this they might turn to philosophy, and that would mean turning to what philosophers say about such things, which means – since philosophers typically work in traditions which take full account of earlier work – turning to the history of philosophy.

Moral philosophers have often attempted to organize the burgeoning and historically changing maelstrom of moral meaning and practice in terms of a single overriding approach. We may at first hope at least to be able to identify the realm of the moral, and the usual step here is to suggest, with David Hume, that the moral is to be

understood in terms of a category distinct from the factual: one cannot derive “ought” from “is.” While this fact-value distinction is accepted by many philosophers and historians, there are difficulties. Hume’s reasoning for the position is briefly this: while facts are derived from external experience, “the distinction of vice and virtue is not founded merely on the relations of objects, nor is perceiv’d by reason . . . it must be by means of some impression or sentiment they occasion” (Hume 1888: 470). It is difficult to imagine more fertile ground for philosophical controversy, and so it has proved.

Conceptually, the words “moral” and “values” are currently fairly widely seen by philosophers as comparatively neutral words enabling moral, and moral philosophical, discussion to take place, but philosophers and historians alike know that that is another position not to be taken for granted. The conceptual and the substantive are not clearly to be differentiated: For example, the Greek term *δικαιοσύνη* (“*dikaiōsunē*”), which we typically translate as “justice,” expressed for Plato the master moral concept, thus displaying for us both historical and cultural variation and also philosophical difference from our current understanding. But Kant makes “good” the overriding moral concept, as in “Nothing can possibly be conceived in the world, or even out of it, which can be called good without qualification, except a Good Will” (Kant 1962: 10–11). To say this is to make a substantive moral point. Similarly we might instead, with Jeremy Bentham and John Stuart Mill, make the adjective “right” the overriding moral concept, by making the substantive moral claim “an act is right in so far as it tends to lead to the greatest happiness of the greatest number,” a claim which also operates as a criterion for legislation and related political goals. In further contrast, and choosing the noun over the adjective, the United Nations has declared that respect for human rights is the present-day route to moral and legal unity. The historically located contingency of that declaration is particularly obvious. (The noun “right,” purporting as it does to name some naturally existing moral object, is according to Bentham “nonsense on stilts.”) What, given such theories, then becomes the core of moral consideration? Intentions? Consequences? Character? Human rights? There are thus conflicting moral theories – each typically claiming to be complete and exhaustive of the moral realm – as well as associated conflicting moral concepts. At different historical periods some such approaches have taken priority over others, only to lose influence. Yet, despite controversies and variable details, there does seem to have been some historical continuity through the conflicts, some grasp of the moral realm which has (so far) enabled us to some extent to share our world with others, half-hearted or belligerent though that sharing has so often been.

It is an intriguing question how far morality is innate, how far a matter of “natural” circumstance, or how far a matter of formal training or education. Historians who feel uninformed about ethics can draw on such material as has been outlined above, intended as it is to be informative with respect to our moral understanding as that has been perceived by philosophers. However informed, historians, if they choose, can make moral judgments of individuals, their activities and situations. And many historians have done so. Historians have often been active figures in the public and political debates of their own time, and a moral judgment by Lord Acton in a letter of 1887, written at a time of political controversy about papal infallibility can illustrate this: “Power tends to corrupt, and absolute power corrupts absolutely.” J. H. Plumb observed, “The historian cannot be free from either moral or political judgements . . . but he can do his

best to form both in the light of history” (Plumb 1973: 15–16). Plumb supposes here that history – which can only mean historiography in this context – should inform moral judgment, and plausibly Acton’s famous judgment was so informed.

The moral use of historiography has been explicitly recognized at least since the historiographic parts of the Bible through the tradition of *historia sacra*. For example, Otto von Freising intended (pre-1147, when he took part in a Crusade) “to compose a history whereby through God’s favor I might display the miseries of the citizens of Babylon [the worldly city] and also the glory of the kingdom of Christ to which the citizens of Jerusalem are to look forward with hope, and of which they are to have a foretaste even in this life” (Thompson 1942: 197) or David Hume (wearing a different hat) intending in the first volume of his *History* “to educate the Whigs in political realities” (Forbes 1970: 10).

Yet Acton, whose name occurs more often than that of any other historian as illustrating the subject matter in Herbert Butterfield’s *The Whig Interpretation of History* (1931) is more widely understood to typify the reverse position: that moral judgment should inform historiography itself. That moral judgment should “inform historiography” is vague: there are two main ways in which it might do so. First we may think of historians passing moral judgments on past individuals or states of affairs. Acton certainly did so, arguing for example for severity in judging Pope Alexander VI (Butterfield 1951: 118–19). He also wrote much on how we might judge. Yet he did not practice what he preached, moral judgments are scarce in his historiography. Unlike Burke he did not condemn the French Revolution. In writing “The Massacre of St. Bartholomew,” for example, page after page passes before we find a sentence such as “Mandelot, the Governor of Lyons, the most ignoble of the instruments in this foul deed” which characterizes a figure in obviously moral terms. Butterfield comments that, towards the end of his life, Acton “condemns the ‘exceeding vividness’ of moral judgments in Macaulay and Thomas Carlyle, and in men like Michelet and Taine” (Butterfield 1969: 92). Still, an input of moral judgment is characteristic of his approach. Many historians share this desire for moral input, and Veronica Wedgwood is a contemporary example: “It is therefore for the theory behind the King’s actions and behind Strafford’s that they must ultimately be judged, rather than for the intrinsic merits of those actions themselves” (Wedgwood 1987: 117), and Simon Schama: “Trevelyan, then, was a prig” (Schama 2002: 280). It is characteristic of both Acton and Wedgwood and many other “judging” historians that they hold the belief that there is an eternal truth to morality, and this is an approach shared also by many speculative philosophers of history. Acton wrote in his Inaugural lecture on the study of history (1906): “The men who plot to baffle and resist us are, first of all, those who made history what it has become. They set up the principle that only a foolish Conservative judges the present time with the ideas of the past; that only a foolish Liberal judges the past with the ideas of the present. . . . History, says, Froude, does teach that right and wrong are real distinctions. Opinions alter, manners change, creeds rise and fall, but the moral law is written on the tablets of eternity.” Wedgwood’s attitude shows similar certainty: “She had none of that unnerving impartiality which in more recent handbooks clouds the youthful mind with doubt” (Wedgwood 1987: 93).

Against such judgments, Richard J. Evans says, “what historians are, or should be, engaged in, is explanation and interpretation, not moral judgment. Historians are

simply not trained to make moral judgments or findings of guilt and innocence; they have no expertise in these things” (Evans 2002: 330). Is he right? Or is it correct to think that moral judgments in historiography are appropriate? What kind of “correctness” is involved in judging such appropriateness? Evans’ view as quoted is a *practical* one: one implication (which the rest of his writing suggests he would rather we did not draw) is that it is regrettable that historians are lacking in moral expertise, and that plainly they ought to be trained, and that if they were, then their judging would not be a matter for criticism. Yet on a different approach it is a *moral* rather than a practical problem whether one ought, or ought not, to make moral judgments of past individuals, or choose some moral criteria rather than others in the varying approaches which historians may have. Leopold von Ranke famously sought detachment from moral judgment in his influential approach to modern historiography, but Heinrich Leo merely saw Ranke’s wish to show how things actually were – *wie es eigentlich gewesen ist* – as a “timid avoidance of personal views” (Gooch 1952: 98). Butterfield remarks of Acton, “It disturbed him that Ranke refrained from any condemnation of the Inquisition” (Butterfield 1969: 92). Those historians who, like Ranke, adopt a dispassionate distance from the morally demanding in the name of “objectivity” themselves face a moral risk: we can recognize in terms of our own moral outlook that an attempt to withdraw from the grip of the moral may be itself morally wrong. It is a current fashion to require of historians that they offer moral judgment. Perhaps this is because of the readiness of present-day politicians to ask that they be judged by “history,” when only historians can do this. The avoidance by historians of moral judgment in history may then be seen as a failing of their moral character. Butterfield’s objections to moral judgment were in part practical: “moral judgments on human beings are by their nature irrelevant to the enquiry and alien to the intellectual realm of scientific history. . . . these moral judgments must be recognized to be an actual hindrance to enquiry and reconstruction; they are in fact the principal reason why investigation is so often brought to a premature halt. Yet we do not deny the importance of morality in life any more than we deny the hand of God in history” (Butterfield 1951: 103). “Much of the benefit which is supposed to result from the whole practice is nullified by the deplorable fact that the moral judgments of historians are so often taken at a low level” (Butterfield 1951: 114). But Butterfield also has a moral objection: “since moral indignation corrupts the agent who possesses it and is not calculated to reform the man who is the object of it, the demand for it – in the politician and in the historian for example – is really a demand for an illegitimate form of power” (Butterfield 1951: 110).

It may well appear at this point that different historians simply have different views about the moral or practical merits of moral judgment in historiography, and that ultimately there is no right answer as to whether it is appropriate that moral judgment should “inform historiography.”

There is, however, a philosophically more significant sense of “moral.” Acton’s main historiographic project was to present the progressive, indeed triumphal, history of liberty. This was not a matter of particular moral judgments of individuals, but rather the framing of an entire historiography around some particular moral attitude. The reason why, in contrast with Burke, Acton did not condemn the French Revolution was because he saw revolutions as helping forward the development of freedom. The “Whig interpretation” itself was such a mode of characterising history, where

historical progress was assumed and in particular seen as moving towards the (comparatively) enlightened constitutional monarchy of the late nineteenth century United Kingdom. Once again, Richard J. Evans is against this: "When we allow our appropriation and representation of the sources to be framed by a knowledge or estimation of these political and legal consequences, then we are no longer acting as historians" (Evans 2002: 344).

Are historians like Acton making a moral or a practical error in framing their historiographies in terms of some moral perspective? Historians' moral judgments of individuals are inexpert for Evans and low grade for Butterfield, but the framing of entire accounts in terms of some moral or politically evaluative perspective is a very different matter. While the particular adoption of the "Whig interpretation" is very plausibly mistaken, historians like Acton who do adopt such an approach show considerable skill and judgment and intellectual subtlety in creating their accounts, which is why they have been seen at different historical periods as so persuasive. While it is an unsubtle generalization, late nineteenth-century British moral and political attitudes were themselves framed by approaches like Acton's. Historians were politically informed and politically informative. They were in large part *sources* of the public understanding of morality, not mere recorders of it. Rejecting the "Whig interpretation" invites the response that a *better* moral perspective should have been adopted, rather than no moral perspective at all.

Acton's type of historiography is not an aberration from some objective progress of purely "factual" historiography, for the interrelationships between present-day politics and the construction of historiography have become more rather than less obviously morally and politically informed. This is well illustrated in Edward Said's view that western political attitudes to the Orient, themselves derived from a history of European domination, continue to inform even academic historiography. Yet his is not a plea to avoid such input; rather, he claims that all discourse is inherently ideological (Said 1978). Accepting this, many historians have been quick to recognize the need for ideological transparency in their approaches. Political judgments explicitly inform the European Science Foundation's research program, "Representations of the Past: The Writing of National Histories in Europe," which seeks to investigate "the structures and workings of national histories" because of their importance in "preparing the way for further dialogue and understanding among European nation-states."

As we have seen in Leo's comment on Ranke, withdrawing from moral input by adopting a dispassionate distance may be itself morally wrong, and this applies both to the moral judgment of individuals and to the framing of historical accounts with evaluative presuppositions. Yet this moral complaint can only arise if it is open to Ranke, or to us, to make a choice between adopting a dispassionate distance and not doing so. Instead, to suggest that all discourse is inherently ideological is to suggest that it is impossible to find that "view from nowhere" (Nagel 1986) which supposedly alone enables us to adopt the dispassionate distance sought, and the reasons offered for this can vary from an insistence on philosophical anti-realism to the assertion of our essential historicity. Such approaches typically depend on the assertion of some form of relativism. The implication is that "truth" is less a matter of correspondence to an objective reality and more a matter of social agreement, so that "objectivity" is better understood as supported by the solidarity of the community (Rorty 1989). Such agreement, the view

is, is properly to be understood as a political matter. We may then think of the determination of truth as wholly a function of power, or we may think of it as depending in part on moral concerns. A further view often held at this point of the argument is that moral concerns are themselves wholly a function of power. Either way truth can at best involve no more than a historically contingent agreement about something which is inherently controversial. Whether all discourse is inherently ideological, is an epistemological rather than a moral question.

Nevertheless, it remains morally questionable whether one should engage in discourse if it is inherently ideological. Should the fact – if it is a fact (could it be a fact?) – that discourse is inherently ideological be suppressed, for our moral benefit? Who, otherwise, would take a promise or contract seriously?! If the position is true, moral judgments in historiography or anywhere else are unavoidable. Yet those judgments are themselves subject to relativistic criticism, and perhaps amount to no more than attempts to gain power – a conclusion expressed by Butterfield above.

The error of these relativistic arguments in the present context is in assuming that dispassionate distance requires that some objective “view from nowhere” be available. The claim is that a historian like Ranke needs to adopt the allegedly impossible dispassionate distance in order to avoid making moral judgments, but what is in effect a denial by Said and others of the fact/value distinction prevents him from doing that. Alternatively, it is a familiar moral position to hold that one needs to adopt the allegedly impossible dispassionate distance, not in order to avoid making moral judgments, but rather in order to make moral judgments properly. Whether or not one does need to adopt a dispassionate distance in order to judge properly is itself a moral issue about which one can rightly hold different moral views, but we can readily see that a “dispassionate distance” does not require a “view from nowhere” but merely a view from *somewhere else*.

Says F. H. Buckley, “I feel less angry about the [Maryland 2002] snipers than I did a year ago, much less upset about O. J. Simpson than I did ten years ago. And I feel positively benign about Bluebeard the Pirate” (Buckley 2004). Moral distance, seen as dispassionate, is readily achieved by the contingent discounting of one’s passions over time. We do not need the view from nowhere to obtain moral distance. Historiographic hindsight will do. A degree of impartiality is achievable by this means. This impacts not only on moral judgment but also on Ranke’s own position of avoiding moral judgment. He does not need the “view from nowhere,” but dispassionate distance. The elapse of time alone can often be sufficient for this. Suggesting that historians are well placed to adopt the required “neutral observer” position, when it comes to characterizing the changing complexities of our moral situation over time, is to privilege the distance hindsight offers without committing oneself to a view about relativism.

However, it remains *morally* problematic whether a dispassionate distance should be adopted in order to make a moral judgment. It is not only a philosophical but a moral difficulty, how morality relates to the passions. It is plausible to hold, for example, that the Holocaust was a matter of such evil that it would be morally wrong to adopt a dispassionate view of it. In so far as historiography is seen as requiring a dispassionate distance, then it would be morally wrong for historians to address the Holocaust. Yet perhaps “there is no morally honorable way of

writing about the Holocaust, since all writing about it will enlarge our universe of moral depravity" (Ankersmit 2004: 97).

The range and complexity of possible moral issues for historians is vast, just as it is for everyone else, with the same room for different opinions. But are historians in the same position as everyone else? Therein lies the next philosophical issue: whether the nature of morality is such that its provisions and requirements (supposing ourselves able to specify them) are universal, so that everyone is equally covered by them regardless of their situation? Given the United Nations Declaration, it seems plausible that some duties are equally held by all, necessarily including historians: for example, the duty to respect human rights. Yet it is not correct to think of human rights as equally held by all, since children, for example, may have rights which adults do not have. Perhaps such variability of rights may be illustrated by the dead, whom – at least in English law – one cannot slander. Universality is limited. F. H. Bradley wrote an essay with the memorable title "My Station and Its Duties" (Bradley 1988) and (despite the view of Henry Maine that we long since moved from status to contract (Maine 1959) he wrote at a less democratic time when the view that one's ethical position might vary with one's social status was more widely accepted than it is now. Can our ethical position vary with our status? What is our "status"?

While we might imagine providing an account of our social status or position, and then deriving our moral situation from that prior account, in practice present-day philosophical and theoretical arguments tend to run the opposite way: different people can be in different moral situations, and we can if we wish deduce varying accounts of status or position from that. Leaving aside historical change itself, one very obvious way in which people can differ in their moral situations is indeed through the existence of promises or contracts. My moral duty may differ from yours because I have promised to do something whereas you have not. In the modern western world, it is promises and contracts and other, usually voluntarily entered, commitments which typically set up each individual's status and position. It is plausible to distinguish, with John Rawls (1972: 111–15), between those rights and duties which apply to everybody and those which instead arise by taking up some role or position, such as accepting public office, taking up a profession, or having children. While these rights and obligations commonly arise as a result of a voluntary act such as a promise, they need not do so; thus conscription into military service could have the same effect.

Theories of justice such as Rawls' sometimes seek to specify principles governing the relevant social institutions so that the role-specific rights and duties associated with those institutions are appropriately binding. However, while it is plausible to seek a structure which will limit the impact of role-specific rights and duties on other rights and duties universally held, there may, nevertheless, be not only a conflict between these role-specific moral concerns and our universal moral concerns but a conflict which might be properly morally resolved in favor of the role-specific moral concerns. Historians will have the same universal moral situations that everybody else has. But it is possible that they will also have moral concerns – particular rights or duties, for example – peculiarly in virtue of their being historians. And it is even possible that those role-specific moral concerns might rightly override those moral concerns which are shared with everybody else. This is an issue which historians themselves need to consider.

How historiography is conceived as a discipline governs the rules, including of course the professional ethical rules, of how that discipline should operate. Membership of the profession of historians, says the American Historical Association, is “defined by self-conscious identification with a community of historians who are collectively engaged in investigating and interpreting the past as a matter of disciplined learned practice,” and the Association summarizes that community’s values as: “Historians should practice their craft with integrity. They should honor the historical record. They should document their sources. They should acknowledge their debts to the work of other scholars. They should respect and welcome divergent points of view even as they argue and subject those views to critical scrutiny. They should remember that our collective enterprise depends on mutual trust. And they should never betray that trust.”¹ Teaching, the American Historical Association says, requires accuracy, but no consideration is given to deeper ethical points such as (for example) the risk of students being corrupted by attempting to explain the actions of evil people by empathising with them, or the more difficult questions of self-censorship when historians discover things which impact on the public realm in dangerous ways. Such matters are recognized as controversial, but it needs to be further recognized that historians ought not to be amateurs in discussing such controversial matters. Historians are not yet ready to confront the fact that they make moral judgments (Cracraft 2004: 33). “Historical method, as it is almost universally understood in the west, rests on the common-denominator values that characterize modernity. Historians treat these values as if they were universally valid and rarely acknowledge them” (Ermath 2004: 61).

Many have forgotten that historians were once associated, by presenting moral examples, with those who *produce* ethical criteria rather than with those who *consume* them; they do not yet appreciate the opportunity they have to develop our contemporary moral understanding rather than merely to apply it. In observing that “the *political, social, and religious beliefs* [their stress] of history teachers *necessarily* [my stress] inform their work,” and in not considering the deeper philosophical or moral points, it is almost as if, for historians as the American Historical Association understands them, the political, social and religious were unfortunate arbitrary inputs to historiography, allowable because unavoidable. Ethical dilemmas are there to be “navigated.” Perhaps unsurprisingly, a historical view is not taken, so also missing is any appreciation that historiography’s values change over time. Recognising the contingency imposed by a temporal dimension, in the United Kingdom the Royal Historical Society is by contrast “mindful” of the *trend* to greater concern with ethical issues,² but gives little detail, and directs attention to the American Historical Association’s material. The international Oral History Association has guidelines³ which approximate those which social scientists use, although, in the case of the social sciences, universities commonly put in place ethical committees to govern research. These professional bodies give little more than nods of recognition towards the huge range of moral issues which may affect historians. With or without the possibility of factual objectivity as required by the historiographic tradition that traces its origins to Ranke, and certainly without requiring that all these issues be in some way codified, historians should consider how much further historiography should make of itself a discipline to which moral issues are not foreign.

Notes

- 1 www.historians.org/pubs/Free/ProfessionalStandards.cfm, accessed December 2, 2006.
- 2 Royal Historical Society Statement on Ethics, www.rhs.ac.uk/, [December 2004], accessed December 2, 2006.
- 3 http://omega.dickinson.edu/organizations/oha/pub_eg.html#Oral%20History%20Evaluation%20Guidelines, accessed December 2, 2006.

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Logical Fallacies of Historians

PAUL NEWALL

Fallacies are errors in reasoning, deliberate or otherwise. Some mistakes occur so often that they have become an object of study in their own right and detailed classifications have been provided by philosophers and logicians, although it is doubtful that such an endeavour can ever be complete. Understanding these and how to apply them, or else how to spot fallacies in the arguments of others, is often advocated as a means of promoting rationality through the teaching of critical thinking. A critical person is better able to defend one's self from unscrupulous politicians, firewater salesmen and the fellow on the street corner who explains that the world will end at the weekend.

The most famous treatment of fallacies committed specifically by historians is Fischer's *Historians' Fallacies* (1970) – in effect, a list of fallacies with examples from historiography. Fischer wrote in support of his conviction that the majority of historians either did not appreciate or did not care about the philosophical problems associated with their discipline or the lamentable condition that analytical philosophy of historiography was in. “Those historians who imagine themselves to be emancipated from philosophy,” he declared, “are apt . . . to be the slaves of some defunct philosopher” (1970: xii) He insisted that historians must seek a logic of historiographic thought, if there is one to be found, and begin to apply logical analysis alongside other historiographic methods rather than view it pejoratively or as something to call upon only as a last resort. To this end, he listed a Noah's ark of fallacies, with examples culled from practising historians to illustrate his claim that historiography was a long way from logical.

Types of Fallacy

Many arguments in historiography or elsewhere can be reduced to syllogisms, consisting of one or more premises and a conclusion. The most famous example runs as follows:

Premise 1: All men are mortal;

Premise 2: Socrates is a man;

Conclusion: Therefore, Socrates is mortal.

Here the conclusion is *deduced* from the premises; this might be the syllogistic form of a statement such as “Socrates was bound to die just like anyone else,” for instance. In general, we can often find syllogisms implicit in arguments, even where the author disavows a logical or deductive approach.

There are two forms of fallacy: *formal* and *informal*. A formal fallacy occurs where the arrangement of the associated syllogism is invalid; that is, where the *structure* of the argument is flawed, even if the premises and the conclusion are all true. An informal fallacy, on the other hand, occurs where the *content* of the argument involves errors, such as where a key term or expression is used incorrectly or may have several meanings that alter the implication of the argument.

With this in mind, then, we will now look at some of the most common examples of logical fallacies, including a description of the fallacy, its classification and representative instances from recent historiography.

Logical Fallacies of Historians

Affirming the consequent is a formal fallacy occurring when one or more potential premises are omitted from an argument. For example, “if my rain dance worked then it should be raining; it is raining; therefore, my rain dance worked.” Here other possible causes of the rainfall are left out and the argument fails. Affirming the consequent has the general form:

P1: If A then B;
 P2: B;
 C: Therefore, A.

However, we can add missing premises to show the error:

P1a: If C then B;
 P1b: If D then B;

. . . and so on. For an argument of this form to *not* be fallacious, we would need an additional premise stating that A is the only possible cause of B. (Note that *affirming the antecedent* (or *modus ponens*) is *not* fallacious; that is, arguing “if A then B; A; therefore, B.”)

This fallacy is of considerable importance to historiography because it often forms the basis of models of confirmation and shows why a more sophisticated philosophical apparatus is required. A typical statement might be: “if our model/explanation of x [some event, say] is correct, we would expect to find y [some evidence, records or traces, for instance], as indeed we do.” Thus an account of some historical episode predicts that certain evidence of it will be found, and when this happens the account is confirmed or demonstrated to be correct. However, in this form the account is straightforwardly fallacious as an example of affirming the consequent. For example, if Einstein were an extraterrestrial of superior intelligence, he should have made

incredible intellectual achievements. He did, and therefore he was an extraterrestrial. We can attempt to undercut this objection by arguing that actually the evidence only makes the account *more likely*, but this requires further elaboration before it becomes philosophically tenable, for example by comparing different hypotheses with different prior probabilities (see Tucker 2004, for an example of a Bayesian approach that avoids this difficulty).

Begging the question (or *petitio principii*) occurs when the conclusion of an argument is used to demonstrate it, thereby achieving a circular proof. (Note that this expression is different from its misuse elsewhere to mean “this *raises* [or *suggests*] the question . . .”; here it has a specific, philosophical import.) For example, we might assert that certain intellectuals in the seventeenth century were crypto-atheists because they behaved in specific ways or authored specific sentiments, but when someone asks how we know they were atheists we respond that these sentiments can be identified with their crypto-atheism. Thus a historical figure was an atheist because he displayed particular traits, but someone who displayed those traits was defined to be an atheist; and so we start by assuming what we want to prove. (See, for example, the debate on Hume’s possible atheism in Norton 1993.)

A *false dilemma* is seen whenever only two possible options are given when there exist others. (This fallacy is also known as a *false dichotomy*). For example, a person might insist, “you’re either with me or against me” and hence force the listener to join forces or else be taken as an enemy. In general, we have the following:

P1: Either A or B;
 P2: Not A;
 C: Therefore, B.

The argument *seems* valid but a premise is missing; namely, something like “P3: No other options other than A and B exist.” It is often used for rhetorical effect; after all, if only two choices are available and one is unpalatable then we are forced to choose the other, even if we might otherwise have reservations.

An example of a false dilemma is the choice facing Chamberlain as Prime Minister of the United Kingdom in 1938, where he could either opt for appeasement or war with Germany. Since he chose the former, some historians and commentators have – with the benefit of hindsight – been harsh in their assessments of him and the part he played in leading Britain to war or letting down Czechoslovakia (see Gilbert and Gott 1963 or Namier 1950). However, the argument that Chamberlain failed to address the threat posed by Germany depends on there being no other options, and, after government papers of the time became available, other historians began to point out instead that Chamberlain was worried about the threat posed by the Soviet Union and believed that Hitler would move east, not west, or else that he was struggling to reconcile British interests and responsibilities around the world with its diminishing power (Barnett 1972). Still another factor involved was Chamberlain’s own insistence that he had to be sure that “great issues” were at stake before a declaration of war could be made (*The Times*, September 28, 1938), and this only became clear in 1939. Regardless of the reasons why Chamberlain pursued a policy of appeasement, then, “appeasement is clearly a highly complex issue and the historical debate is ongoing”

(Darby 1999: 83). The claim that his options were “appeasement or war” is thus superficial at best and a false dilemma, one that robs the history of this policy of considerable depth and nuance.

A related informal fallacy is *post hoc, ergo propter hoc* (“after this, therefore because of this”) which holds that if one event follows another then the former must have caused the latter. (Similarly, *cum hoc, ergo propter hoc* involves the assertion “with this, therefore because of it.”) That Chamberlain’s government pursued a form of appeasement and then war followed does not imply that one necessitated the other. As before, the error lies in assuming that no other causes were operating.

An *argumentum ad vericundiam* or “appeal to authority” is a resort to expertise that is often – but not always – fallacious. It occurs where a writer wishes to buttress a claim by referring to an authority who agrees with him or her. A common example takes the form “as X has demonstrated, Y is the case.” The difficulty here is that *other* authorities might have “demonstrated” otherwise and so it is by no means clear that referencing the authority actually achieves what the author hopes. In particular, where controversy exists in a given discipline, appeal to the authority of one of the participants falsely gives the impression that a consensus exists.

In syllogistic form:

P1: A says B;

P2: A is an authority on the subject of which B is a part;

C: Therefore, B is true.

This argument is missing an additional premise, to the effect that A’s view is representative and not subject to significant dispute. However, where A is an authority on a subject it is not fallacious to appeal to his or her opinion; for instance,

Vico’s heroic effort to modernize philology by making it philosophical was a project he shared with many seventeenth-century intellectuals of the most diverse theological views, as Carlo Borghero has shown in his detailed account of historians’ and philologists’ efforts to counter the Cartesian attack on book-based, historical forms of knowledge. (Grafton 2001: 276)

Borghero is an authority on both Vico and Cartesian thought, while Grafton himself is an expert on the period during which Vico lived, so the definitive appeal to Borghero is not fallacious. If new or other scholarship challenged Borghero’s work, however, merely appealing to his authority to justify a statement would become a fallacy. While we cannot go through life doubting the authority of experts, for lack of time and resources, as philosophers or historians when we write of a particular topic, we doubt all expertise, and attempt to follow the lines of argument of the experts to check on their evidence and proofs.

The *argumentum ad hominem* (“argument against the man”) is possibly the most widespread of all fallacies, at least in part because it *works* so well on a rhetorical level. The effect of a remark such as “professor Y offers an impressive argument that almost distracts one from his being a racist” is to discredit an author before his or her disconnected ideas or associations. There are three forms of *ad hominem*:

An *abusive ad hominem* points to a trait or quality of the person making an argument, as in the example above. This gives us:

P1: Professor Y presents argument X;
P2: Professor Y is a racist;
C: Therefore, X is false.

Simply writing out this argument in syllogistic form is enough to show that the conclusion does not follow from the premises and is thus fallacious. Note that this is *not* the same as an outright insult; instead, the fallacy invites the reader to consider a person's opinions on matters *other than* the argument at hand. The point here is that even if someone is racist, sexist, xenophobic or holds any other belief generally considered to be distasteful, this says *nothing* about the logical validity of their arguments.

The second form of *ad hominem* is *circumstantial*, where, as the name suggests, a person's circumstances are brought in as though they are relevant to the discussion. For example,

Just as there are professors of literature who never engage in the actual interpretation of literary works – and even disdain interpretation as an inferior vocation – so there are professors of history who have never (at least to judge by their published work) done research in, or written about, an actual historical event or period. (Himmelfarb 1992)

Here the fact that certain commentators do not or have not practiced historiography (whether or not this assessment is accurate) appears as though it has some bearing on the matter (in this case, Himmelfarb's objections to postmodernist historiography). The effect is to suggest that a person has no business offering an opinion if he or she has no practical experience of it. For example, doctors and psychologists who have no children cannot advise parents, or most pertinently here, philosophers of historiography cannot advise historians. In general, we would have:

P1: A claims B;
P2: A is in circumstances C;
C: Therefore, B is false.

Again, the conclusion does not follow from the premises and there is no logical connection between a person's circumstances (or lack of practical experience) and the truth-value of their arguments.

The last *ad hominem* is the *tu quoque* (literally, "you too/also"), in which an inconsistency or hypocrisy in someone's argument is exposed. For example, "professor Y argues that anyone commenting on historiography should be a practising historian, but he is not one himself." Formally:

P1: A claims B;
P2: A practices not-B;
C: Therefore B is false.

Once again, the conclusion does not follow and the argument is fallacious. However, although the truth (or otherwise) of the claim is not affected by A's inconsistency, it may show that his advocacy of it is hypocritical. This form of *ad hominem* has been important in the debate surrounding relativism in historiography, where one of the main objections against an all-encompassing skepticism is that it is self-refuting, and hence critics of traditional historiography are charged with disdaining meta-narratives while offering new ones of their own in the process.

An *argumentum ad consequentiam* is a fallacy that appeals to the real or supposed *consequences* of an argument to reject it. The error is plain when cast in syllogistic form:

P1: If argument A were true, it would have consequences B;

P2: B is distasteful;

C: Therefore, A is false.

It may seem that this has no relevance to historiography as the fallacy is so obvious that it would immediately be noticed, but recent debates in historiography suggest otherwise. Commenting on the efforts of revisionists (the "minimalists"), for example, Dever (2001: 291) remarked that their programme,

if it could be carried out, would in my opinion see not the advent of a secular Utopian "Brave New World" but rather anarchy, chaos, and ultimately those conditions of despair that have often historically led to Fascism.

For his part, Elton (in Jenkins 1997: 179) asserted that the uncertainty surrounding the possibility of historical truth, "however dressed up, leads straight to a frivolous nihilism which allows any historian to say whatever he likes." Both consequences described here are somewhat apocalyptic so we ought presumably to reject them, but if we recognize them as fallacious (*even if* they are actually true) then we can begin instead to look at the assumptions underlying such characterizations and understand why these authors felt the need to use them.

Fallacies and Historians

The *historians' fallacy* involves the assumption that people in the past experiencing an event would know it exactly as a historian does when studying it today. This error may be behind many conspiracy theories, insofar as historians studying some past event consider it to have been so obvious what was going on that for contemporary actors not to realize it implies a conspiracy of some kind. The best examples of the fallacy are therefore those suspected of involving genuine or possible conspiracies, such as the attacks on Pearl Harbor or the consequences of the Treaty of Versailles. The difficulty lies in historians looking for reasons why some event happened and not identifying the many other factors at work in any given situation that would also have influenced people at the time. Hence, historians assume that their understanding of the event mirrors that of the actor involved, allowing judgments to be made (or conspiracies and motivations to be inferred).

We can put this error in syllogistic form as follows:

P1: Circumstances A, B and C pointed to the likelihood of event D occurring;

P2: Person E knew about circumstances A, B and C;

P3: Event D occurred;

C1: Therefore, person E knew that event D would occur;

C2: Therefore, person E deliberately did nothing about event D.

Written out in this fashion, the problem becomes apparent: there are any number of other premises missing, such as “person E also knew about circumstances F, G, H . . .” and so on, suggesting something other than D, in which case the conclusions do not follow.

An example of the historians’ fallacy is in the debate surrounding the dropping of atomic weapons on Hiroshima and Nagasaki. Some historians have claimed that the Japanese intended to surrender and that the US President knew this, rendering the attacks unnecessary and certainly immoral (Sherwin 2003). However, other historians have pointed out that what Truman and other members of the US administration did or did not know is the subject of speculation, especially given that the contents of meetings covering secret material were not recorded, and that it can in fact be shown from the documents that do exist from both sides that the bombs were necessary (Weintraub 2001; Rhodes 1995). This fallacy occurs when a historian goes beyond studying an event and begins to make conclusions based on how a contemporary actor *must* have reasoned (often assigning moral culpability) when he or she has no way of knowing how that person would have experienced events or interpreted the information they had to hand.

Anachronism is a well-known error in which past events are interpreted according to frameworks that did not exist at the time. For example, the study of the so-called Galileo Affair commissioned in 1992 by Pope John Paul II referred to the “tragic mutual incomprehension” at the time of Galileo’s difficulties with the Church (Newall 2005a) and concluded, amongst other things, that theologians of the time had not understood scripture correctly. However, the methodological approach that Galileo had advocated towards interpreting biblical passages (Newall 2005b), since adopted as standard, was not in use at the time and hence complaints that it was not employed properly are anachronistic. After all, how can we criticize our forebears for not using something that did not yet exist?

This fallacy is of particular interest to historians because it is a universally recognized methodological mistake. Indeed, teaching itself can be a significant source of anachronism as textbooks tend to present subjects (particularly the sciences) as they are *now*, fully formed and seldom with any historical context; in particular, they present a linear development where actually controversies existed and people in the past held ideas we now consider wrong for very good reasons. For example, this was Feyerabend’s (1987: 247–64) opinion of Bellarmine’s arguments against Galileo. Kuhn stated baldly that the purpose of textbooks is explicitly pedagogical and that to include details of how “knowledge was acquired (discovery) and why it was accepted by the profession (confirmation) would at best be excess baggage” (1977: 186).

A nice example of anachronism in the history of science concerns Einstein's work on the special relativity, originally published in 1906. Later that year, in his paper *Über die Konstitution des Electrons*, Kaufman experimentally falsified Einstein's theory (1906: 487), a result that was accepted by Lorentz, Ehrenfest, and Poincaré, amongst others (see Feyerabend 1999: 146–8, for more detail). According to Popper's conception of *falsificationism* (specifically dogmatic falsificationism), then, the special theory had been falsified (i.e., failed the test of experiment) and should therefore have been given up. Although Einstein admitted as much (1907: 439), he declined to do so and it took Planck another decade to discover that there was a flaw in Kaufman's experiment. By the time Miller appeared to have refuted the theory again years later, however, it had proven so valuable theoretically that few physicists took Miller's work seriously, even though it again took a long time (almost thirty years) for Shankland to find the mistake (1963: 167ff).

This example is useful in historiographic terms because it shows anachronism in two distinct ways on the part of philosophers of science. Critics of falsificationism pointed out that if Einstein had followed Popper's methodological advice then the result would have been disastrous; that is, the special theory would have been lost, even though we now consider it one of the best theories we have. Here the critics interpret what Einstein and other scientists should have done anachronistically, using the episode to make a philosophical argument that would have made little or no sense in Popper's terms at that time. On the other hand, supporters of falsificationism responded by saying that although it *seemed* that the special theory had been refuted, actually this was only an *apparent* falsification because the error was later found and so Einstein behaved as a good Popperian should. However, this then set up a distinction between *true* and *apparent* falsifications that could only be employed anachronistically; that is, a refutation can only be discovered to be mistaken considerably after the fact, so concluding anything about how a scientist behaved based on information that became available decades later (or in principle at any later time) is again anachronistic. (See Lakatos 1978, for further discussion of this issue.)

This story is important (and hence worth telling) because we can see that an account of the past is not necessarily anachronistic relative only to the truth of the matter (if there is one) but also when compared to *other* anachronistic accounts. Moreover, in the first part of our example the anachronism is actually useful, insofar as it shows that a requirement formulated long after a historical event would actually have been unhelpful; in short, the anachronism is used in a positive sense. Anachronism of one form or another may be unavoidable in historiography but in its more blatant manifestations should be questioned by historians and philosophers alike.

An error related to anachronism is *ethnomorphism*, which involves “the conceptualization of the characteristics of another group in terms of one's own” (Fischer 1970: 224–5; similarly, *ethnocentrism* involves overstating the *role* of the author's own ethnic group in some historical episode). Ethnomorphism is easy to identify in everyday life, the best example being the assumption that western standards are the norm to which others aspire and from which divergence can be judged. The fallacy is seen in questions such as “why did nation X not develop the concept of human rights?” or “why does nation Y refuse to accept democracy?”

In historiography, the fallacy occurs when the historian judges a past culture or period in terms of how far it had advanced towards his or her own, which presupposes both that history is teleological and that the author's standards are the end result. Mumford (1967: 244) objected forcefully to ethnomorphism in the historiography of technology:

If the historian finds a lack of invention in earlier cultures, it is because he persists in taking as the main criterion of mechanical progress the special kinds of power-driven machine or automation to which western man has now committed himself.

For this reason, instances of ethnomorphism often tell us more about the historian than about the period of history under discussion. An excellent instance of historiography written with this understanding in mind is Cantor's *Inventing the Middle Ages* (1991).

Fischer describes the *Baconian fallacy* as "the idea that a historian can operate without the aid of preconceived questions, hypotheses, ideas, assumptions, theories, paradigms, postulates, prejudices, presumptions, or general presuppositions of any kind" (1970: 4). The demise of the very possibility of historiography on such terms is a particularly good example of the relevance of philosophy to historiography and of the benefits of an interdisciplinary approach. The association with Bacon derives from the traditional empiricist interpretation of his work, which stresses his insistence that science required emptying the mind of any notion concerning what we might find (see Lakatos 1968, for example; however, that this is an accurate rendering of Bacon's philosophy of science is disputed, see Muntersbjorn 2003).

The Baconian fallacy is easily identifiable:

The historian must certainly make one initial choice, of main area of study or line of approach. But after that (if he is worth considering at all) he becomes the servant of his evidence of which he will, or should, ask no specific questions until he has absorbed what it says. (Elton 1967: 62)

Strictly speaking, such an attitude to historiography (or indeed anything) is misguided at best but not a fallacy since it contains no logical flaws; it is simply a philosophical error that was outdated even when Elton was writing. Nevertheless, a version of this sentiment remains popular, particularly when historians are attacked for their supposed bias and retaliate by insisting that a neutral methodology is used in historiography.

The fallacy of *negative proof* occurs when the lack of evidence for something is taken to justify the conclusion that it did not exist. The logical error involved becomes obvious when shown as a syllogism:

P1: No evidence has so far been found for A;
C: Therefore A did not exist/happen.

This is a formal fallacy: what is needed is an additional premise, to the effect that no evidence for A will *ever* be found subsequently.

A good example of this fallacy is found in studies of historical figures or events, especially religious ones, such as the debate over the historicity of Jesus or the existence of Atlantis. Where a person is referred to in stories or sagas, say, but no other evidence of their actual existence is found, should we conclude that the person is fictional? Notice

that there is a considerable difference between claiming that the lack of evidence proves that the person did not exist (i.e., the negative proof fallacy) and asserting that the *likelihood* of their historicity is small; however, an elaboration of how such probabilities are assigned is still required.

The *holist fallacy* occurs when historians attempt to write a particular history while relating it to the whole. The fallacy can be often found wherever a holistic approach is emphasized or a sense of “universal history” is referred to. For example, Elton advised that

all good historical writing is universal history in the sense that it remembers the universal while dealing with a part of it. (1967: 16)

The problem is that if we need knowledge of the whole in order to understand a part of it then the particular knowledge is rendered moot – we already have complete knowledge. Fischer (1970: 65) called this methodology “absurd” because “it would prevent a historian from knowing anything until he knows everything.” A more charitable reading of Elton might take his notion of “remember[ing] the universal” to mean that an account of a specific event must be contextualized by keeping in mind the other happenings that went on around it, but nevertheless the methodological requirement is difficult to implement and reveals a genuine problem: how can we put accounts of the past in context without knowing the past in its entirety?

The *didactic fallacy* concerns attempts to learn from history. Pundits are keen to endorse Santayana’s remark that “those who cannot remember the past are doomed to repeat it (1905: 84); or to point to Hegel’s skeptical conclusion that we only learn from history that no one ever learns anything from history, and there are often good reasons to insist that we ought to try to learn lessons from the past. For instance, Soviet tactics in Afghanistan “ignored not only the experiences of the British in the same region, but also their own experience with the Basmachi resistance fighters in Central Asia” (Binnendijk, in Grau 1998: vii).

Even so, the informal fallacy lies in supposing that since lessons *can* sometimes be learned, therefore they *should* be. In order to learn anything from the past we have to assume that it resembles the future closely enough that any lesson from prior experience can be applied, *mutatis mutandis*, to new situations. Catastrophic examples of this supposition not holding can be found littering the history of warfare, where new technologies and weaponry rendered extant tactics obsolete even as commanders sought to implement what they had learned from tradition and studying the past – so much so that Murray (1997) could write that “[a] battalion commander of 1914 . . . would not have had the slightest clue as to what was occurring in 1918; that was how far military affairs had travelled in the course of four years.”

Clearly, then, the didactic fallacy is important in historiography because it cautions us to modify Santayana, along the lines of “those who cannot remember the past *where applicable* . . .” Trying to learn from the past when there are good reasons to suppose that the contemporary world has moved on thus becomes at least as stubborn a methodology as the refusal to learn that is so frequently lamented.

The *reductive fallacy* involves reducing a complex history to a simple one, often by taking an event influenced by a multiplicity of factors and explaining it in terms of only

one of them. Fischer wrote disparagingly of psychoanalytical historiography, joking that “in some of its forms [it] reduce[s] the cause of the reformation to Martin Luther’s toilet training” (1970: 174). Reduction inevitably occurs in historiography since no account of an event can contain all the factors at work, but taking it too far results in this informal fallacy. A good example is the claim that the US Civil War was caused by slavery, which, although relevant, falls considerably short (see Wagner et al. 2002).

In summary, the study of logical fallacies can help us understand common errors in arguments and utilize the lessons in historiography. Although an appreciation of logic is not sufficient to write well about history, it may increasingly be necessary and certainly a useful tool for the historian hoping to increase the rigor of his or her work. If clarity of thought is aided by the application of logic and philosophy in other disciplines, there is no reason why historians should not do likewise.

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Historical Fallacies of Historians

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Philosophers' fallacies and historians' fallacies

In the language of philosophers, the word “fallacy” usually denotes a mistaken inference, i.e., the conclusion of something which does not follow from the premises from which one drew it. Thus philosophers may criticize historians for committing fallacies when they make unwarranted inferences from their premises or evidence (Fischer 1970). Philosophers, however, can be criticized by historians from making *faulty inferences*, inferences from false premises about the past, a mistake which results in the falsehood of one's conclusions.

Though the class of statements which are eligible to be criticized as historically fallacious may include any kind of claim about the past, here we shall focus on one particular sub-class of such statements: interpretive claims about the concepts, theories, and ideas of past authors. For example, a medieval monk interpreting Plato's dialogues in terms of Christian theological concepts could well stand accused of committing a historical fallacy (even if some of those concepts were in fact derived from Platonism), as might a modern historian of science who reconstructs the theories of Renaissance alchemy using ideas derived from modern chemistry. In both cases we find an interpreter who interprets texts and statements from the past by means of ideas which – we presume – were not fully available to those who authored the texts to be interpreted.

A landmark study in this regard is Robert Livingston Schuyler's *Some Historical Idols*. This title refers to Francis Bacon's four *idola* and therewith belies an awareness of the early modern roots of negative methodology. Schuyler singles out *present-mindedness* as an illusion particularly obstructive to our understanding of the past. It is an illusion in which the past is measured by the present. Accordingly, the individuals of past eras are ascribed motivations which may only be assumed for our own actions in the present, or the actions of the past are interpreted in terms which must have been both foreign and unavailable to them. These and similar types of methodological criticism later came to be adopted by the historiography of political ideas movement of the Cambridge School, of which one noted exponent was Quentin Skinner. In distinguishing the *mythology of doctrines*, the *mythology of coherence*, the *mythology of prolepsis*, and the *mythology of parochialism*, Skinner (1969) avails himself of a methodology similar to that of Schuyler in analyzing various “anachronistic mythologies,” all

of which share the common trait that they make the past a backdrop for reflection on the present.

From the perspective of a methodology of the historiography of philosophy and ideas, then, historical fallacies consist in those procedures which lead to an anachronistic view of a certain historical object, event or action (Fischer 1970). We define an illegitimate historiographical practice as one which ascribes beliefs and intentions that presuppose *modes of presentation* and *types of description* unavailable to a certain agent at a certain time. Such practices are anachronistic, and therefore are to be avoided.

Five Historical Fallacies

In order to illustrate these practices, we shall characterize five common historical fallacies discussed in the historiography of philosophy. Whereas all historical fallacies manifest themselves as some form of anachronism, they may be traced to the following objectionable, or at least occasionally false, premises. These are: (1) the premise of historical teleology (the fallacy of historical teleology); (2) the premise of *philosophia perennis*, or the immutability assumption (the historical fallacy of immutability); (3) the assumption that the conditions of reference for the interpreted author are identical to those of the interpreter (the historical fallacy of reference); (4) the assumption that the historical object of interpretation had access to the concepts of the interpreter (the historical fallacy of availability); and, finally, (5) the premise that the most adequate reconstruction of a historical view is the one which makes it most tenable for *us* (the fallacy of patronization).

The historical fallacy of historical teleology

In historiographic reconstructions of philosophical theories we sometimes find pride of place given to insights into current theories and the current state of philosophy. In such cases, depending upon the positions deemed to be “state of the art” (here one finds some variation), certain historical positions will inevitably be winners, and those incompatible with them will seem to be losers. As interesting as the losers may be, what progress there is in philosophy will be expressed in a historiography of the winners.

This approach lends itself to the construction of a historiography of successful precursors and their triumphant modern theoretical heirs. From the perspective of such a historiography, the winners are always those theoretical precursors which are most connected with, or intelligible to, the philosophical present. The relevance of predecessors is manifold. The line of continuity derived from the concept of a precursor can, on the one hand, be used to “modernize” that very predecessor; on the other hand, it can just as well endow the “successor” theory or doctrine with a fine tradition of eminent precursors. In other words, the precursor concept provides not only retrospective continuity; it may also furnish legitimacy, much like the practice of “reading one’s own views into a sacred text so one can read them back out endowed with authority” (Gilbert Harman’s letter in Sorell 2005: 43–4).

Such practices raise some historiographical suspicion, and justly so. Might they not involve a rather dubious contract between two parties in which the precursor

becomes a “pillar of support” through “hagiographical” anachronism? Such procedures seem particularly problematic when the retrospectively constructed lines of development are backdated offhand as teleologies, and so interpreted as mere anticipations of present trends and theories. Even if a forerunner of a particular theory or discipline is not used as a retrospective anticipatory figure in order to furnish identity and legitimacy, the danger remains that the precursor only be perceived *qua* precursor, i.e., in relation to the particular characteristics which permit the construction of a continuous line to the present. If so, then the precursor’s other “non-teleological” characteristics are disregarded.

The corrective for teleological fallacy in the historiography of philosophy is a *veil of ignorance* regarding the philosophical present. This *veil of ignorance* – which is not to be confused with a less salutary *real* ignorance of contemporary philosophical issues – shuns the reduction of the history of philosophy to an inventory of anticipation and error. From a methodological standpoint, we note that the correlation between anticipation and realization inherent in the concept of precursorship modifies the notion of historiographical explanation profoundly: the precursor can now not only be situated within his own historical context, but can also be placed within the historical contexts of his successors, who have continued on and achieved the undertakings which he had supposedly anticipated.

Such anticipatory histories of philosophy generally assume that the disciplinary framework in which precursors and successors work remains stable. Sometimes “philosophy” is even said to be “the name of a natural kind – the name of a discipline which, in all ages and places, has managed to dig down to the same deep, fundamental questions” (Rorty 1984: 63; here, Rorty criticizes this view of philosophy). Against the notion that philosophy is a “natural kind” it may be pointed out that philosophy is not located in a timeless realm, but has gone through substantial changes since its inception, which itself would be difficult to properly define. The same goes for the disciplines we now call the sciences. Talk about ancient “physics,” “anatomy,” “physiology,” and “geology” is not to be taken for granted. The tendency to endow relatively recent fields of research with long traditions may be seen, for example, in the case of “aesthetics,” which became a recognized topic of philosophical study in the eighteenth century, but is often discussed as if it had already been well established in antiquity.

In contrast to teleological accounts of the history of philosophy, properly historical accounts tend to exclude later developments from consideration. Thus the historian of philosophy is advised to “study the ideas and theories of a period . . . without regard to what came afterwards” (Brush 2004: 256) and to avoid uncritically imposing contemporary philosophical concepts and categories on the theories of the past, lest these appear as mere approximations to those in use today.

The historical fallacy of immutability

Do philosophical concepts have a history? Is the notion of “belief” a cross-cultural category? Can our concept of truth be universally applied? Are there fundamentally different philosophical ways of classifying objects? Are there timeless styles of legitimate reasoning? Can the findings of logic be universally applied? Since when do philosophers think that philosophy is about problem solving? How different are the conceptions of

what philosophical research is? Is there a certain number of timeless philosophical systems, as Bertrand Russell assumed when he spoke of “the great types of possible philosophies,” reassuring the reader that “the philosophies of the past belong to one or other of a few great types – types which in our own day are perpetually recurring” (Russell 1900: xii)?

These questions relate to the type of alleged historical fallacy that consists in the assumption that all philosophical inquiry concerns a single set of theoretical objects. This set is described in different ways. In some traditions it consists of eternal philosophical problems, in others it variously includes philosophical concepts, systems of classification, methodological canons or styles of reasoning. Some argue that a certain number of philosophical systems, philosophical subjects, or even the discipline of philosophy itself can be located in a timeless realm. Here we shall examine the consequences attending just one of these positions: the view that there are timeless, or at least transhistorical, philosophical problems.

Leo Strauss provides an admirably clear example of the problem-oriented approach to the history of philosophy. He claims “that all human thought, and certainly all philosophic thought is concerned with the same fundamental problems, and therefore that there exists an unchanging framework which persists in all changes of human knowledge of both facts and principles” (Strauss 1953: 23–4). Here we notice that the presence of enduring problems is taken as a reason for there being an “unchanging framework.” But have the problems related, for example, to “consciousness,” “virtue,” “reference,” or “death” always been the same? Presumably not; and even if they were, one could still explain this without assuming that the frameworks for all such problems are identical.

Are there, then, eternal or enduring philosophical problems? The question has resurfaced in recent years (Bevir 1994; D’Oro 2004); discussions of it focus on the problematicist accounts of Robin George Collingwood (1939) and John Passmore (1964/5), and tend to ignore the (primarily neo-Kantian) German tradition of *Problemgeschichte* (Hänel 2001). As a comparison of different problematicist accounts of the history of philosophy shows, the historiographical notion of “problem” doesn’t necessarily have to be used in an anachronistic way. Problem-oriented approaches search for a disciplinary contextualization of the objects of investigation. They do this by emphasizing the identity of problems in past and current research and, very often, by translating the original vocabulary of the historical source into the vocabulary of modern philosophy.

If there are indeed no common methodological canons or subject areas of philosophy over longer spans of time, then the historian of philosophy is well advised not to confound his own interests, standards, and concepts with those of past philosophers. He is thus constrained to stick to the standards, concepts, and categories which were available and relevant to the historical author he studies. At the same time, the investigation of just these things has a dialogical character, one which the problem-oriented approach to the history of philosophy engages. Dialogical philosophical interpretation – as conceived by Hans-Georg Gadamer (1960) – follows the maxim that one must try to interpret philosophical texts as relating to philosophical problems which we, the modern readers, may identify as such. In those cases in which the modern historian seems to become “over-familiar” with his historical interlocutor, the application of this

maxim may result in conspicuously anachronistic or simplistic interpretation, a kind of “pen-pal” approach to the historiography of philosophy. Yet it is unnecessary. Engaging a philosophical text philosophically requires, at some level, the assumption of a mutually intelligible, if not identical, framework. A historiography of philosophical problems, when properly pursued, may have the advantage of constructing such a framework while leaving the question as to the immutability of its several components open to historiographic and philosophical research.

The historical fallacy of reference

A central assumption of the problem-oriented approach to the history of philosophy is that the theories and concepts which form the object of historiographic interpretation refer to the same things which we now study. This assumption seems particularly obvious, even unavoidable, in the history of the natural sciences. If, since the beginning of science, the physical world and the regularities which govern it haven't changed, then why should we assume that the objects of natural science have? But the objects which are supposedly simply “given” are in fact embedded in particular historical and theoretical contexts. And these theoretical and historical contexts and frameworks determine not only what may be intelligibly described as a problem; they also determine the meaning of terms within their purview and, what is more, the objects to which those terms refer. When those contexts overlap, there should be no difficulty in referring to, and meaning, the same things as those who theorized before us, even if we may sometimes use quite different terms. But when the theoretical context of the present widely diverges from that of the past, we may refer to quite different things even with the *same* terms. Let us first consider some examples of this before we discuss the challenges such cases pose for a theory of historical fallacy and a methodology of adequate historiography.

We are looking for examples in which a contemporary writes a book about idea *x* from “the beginnings until our time” – in this case, under the (wrong) assumption that *x* refers to what we think it refers for the span of time considered. We need not look very far. The popular science section of the bookstore is well stocked with volumes on the global history of concepts which are particularly current – or of concepts of particular importance for current programs of research. Let us take the example of the atom (Pullman 1998). Contemporary atomic physics is a science which studies basic, though not the most basic, units of material. It is likely that the first ancient Greek Atomists, Leucippus (active ca. 440–435 BCE) and Democritus (ca. 460–380 BCE), developed their theory in reaction to their Eleatic predecessor Parmenides (ca. 540–480 BCE). Parmenides stipulated that “what is” be uncreated and imperishable, one and continuous, unchangeable and perfect. The Atomists postulated a (theoretical) object they called an *atomon*, an “indivisible” to meet these metaphysical requirements.

The atom of modern physics is clearly the product of radically different theoretical constraints. This is apparent in the most striking difference between the modern concept of the atom and that of the ancient Greeks: whereas the *atomon* is *per definitionem* indivisible, the atom has long since been divided into various constituent parts. But the difference between the frameworks surrounding the two terms goes deeper. If we praise ancient Atomists for having discovered the atomic model, we run the risk

of ascribing to them beliefs and knowledge which they could not have had. For the atom as we conceive it is not the thing to which the term *atomon* referred. This is not to say that no part of the phenomena which we now explain by use of an atomic model was grasped by ancient Atomists (or other Greek philosopher-scientists, for that matter). But there is a difference between ascribing beliefs to past thinkers with reference to certain things and states-of-affairs on the one hand, and describing their views as they had them on the other. When we ascribe to others beliefs about things and facts which we identify as such, we make claims about the real objects of their beliefs. In philosopher jargon, this is an ascription *de re* (Latin: “about the thing”). When we describe things and facts as others see them, we make (potentially stronger) claims about what the others understand to be the objects of their beliefs. This is an ascription *de dicto* (Latin: “about the thing said”). If we keep these two kinds of ascriptions separate, it is possible to say how it may be historiographically feasible to ascribe certain beliefs to ancient Atomists concerning what we consider to be atomic phenomena (an ascription *de re*) without claiming that they conceived of these phenomena as we do (an ascription *de dicto*). Of course, even the first claim could be contentious – but at least it does not imply that the ancient Atomist referred to what only the modern historian can mean by “atom.”

The historical fallacy of availability

As we saw, interpretive statements *de dicto* are not just about what others refer to with a certain expression, but what they mean in referring to it (Trout 1994; Brandom 2002). The horizon of things to which a historical author could have referred may be contested, but it is usually not contentious; yet there may be several and very different interpretations of what that historical author may have believed about the things to which he actually did refer. In making interpretive statements *de dicto*, i.e., about what the author believed, it is therefore important to be clear on what beliefs we may reasonably ascribe to him. If there are good grounds to assume that certain states-of-affairs were unknown to the author whom we seek to interpret, then we shall do well to abstain from attributing to that author any beliefs or attitudes concerning those things which he could not have known.

What does it mean for something to be “unavailable” to someone at a certain time? How should the notion of “availability” be explicated to meet the needs of historiographic inquiry? These questions are at the focus of methodological investigations of the so-called availability principle (Barnes and Barnes 1989; Prudovsky 1997; Jardine 2000). The availability principle, also described as the accessibility principle, suggests in its concisest version that no acceptable historiographic interpretation may use knowledge, descriptive terms or classification schemes which were unavailable or inaccessible in the historical time and place which is to be interpreted through the evidence. An interpretation of the accessibility is the maxim that all interpretations which use linguistic and epistemic resources unavailable to the historical authors and their contemporaries should be excluded. The historian can legitimately use only knowledge, descriptive terms and categories that were available to the historical authors whose (verbal) behavior is to be described.

This is clear but problematic for the principle thus described conflicts with a central principle of philosophical interpretation, the principle of charity. In the application of

this principle one often runs the risk of ignoring historical and situative constraints on the views of the interpreted author, thereby assigning to him beliefs which he couldn't have had. In this way, by committing the interpreter to avoid attributing errors to the author, the principle of charity may systematically lead us to produce anachronistic interpretations. Against the principle of charity we may require that those interpretations which can actually explain how the author came to (right or wrong) beliefs be preferred. This would give us the slightly modified maxim: "We should avoid ascribing unintelligible error, but equally unintelligible *insight*" (Glock 2003: 196).

Yet we need not reject the principle of charity as an interpretive maxim; we need only to reformulate it. On this reformulation we resist ascribing true beliefs to a speaker in those cases in which we do not know how she acquired them, or when we even know that she couldn't have acquired them on account of their unavailability. This, we propose, is just good interpretive common sense. Instead of charitably maximizing the true beliefs of the author, one should seek rather to ascribe to the author as much *knowledge* as possible (Williamson 2000). Using this more context sensitive principle, we shall not lose sight of the epistemic position of the author to whom we ascribe certain beliefs and positions. At the same time, we shall commit ourselves to reconstructing the epistemic position of the author by virtue of the charitable principle of maximizing knowledge.

The proponent of the availability principle does however bear the burden of clearly formulating criteria for what and what not may have been available to the author, actor, or speaker whose writings, actions, or statements are to be interpreted. How are we to arrive at such criteria? This question exceeds the scope of this introduction. But one important component of an answer is that the interpreter need not be strictly constrained by what the author and her peers actually wrote and said. In the reconstruction of the horizon of available beliefs and knowledge the interpreter will also have recourse to potential meanings in the framework of the author's particular time and place.

The historical fallacy of patronization

There is, however, a further problem with the principle of charity, even in its modified form. For to this modified form one could still object that the strategy behind the principle is an "imperialistic" one, designed to read not just contemporary beliefs but more specifically the particular beliefs of the interpreter into the statements of the interpreted author. In this vein, some have called the principle of charity a "principle of *patronization*" (Ross 1985: 500). This is a characterization based upon the objection to the excessive use of the principle: "In interpretive terms, an excess of charity means the dissolution of differences between readers and writers, in which the former do the latter an imagined favor by making everything in the text agree with their own beliefs . . . It becomes a virtue to see one's own thought in everything one reads" (Pappas 1989: 257).

The problem, then, in short, is that the global application of even a context sensitive version of the principle of charity could lead to a systematic re-casting of the interpreted author in the interpreter's own image. At the core, the problem with charity consists in an implicit epistemic asymmetry between the interpreter and the interpreted, whereby the interpreter is supposed to occupy an epistemic position

which is superior to that of the author interpreted. Yet in our dealings with other speakers and texts we can never simply establish beforehand that “we” are right. We stand in danger of failing to be able to learn from our interlocutor if we accept a concept of interpretation according to which, from the very start, it is *our* claims to knowledge and standards of rationality which are deemed superior and hence to be assumed for that which is to be interpreted. If we cannot ascribe beliefs to another which plausibly differ from our own (supposedly correct) ones, then we shall not be able to make room for the possibility that we ourselves might be wrong.

It follows from this objection to global charity that we should apply the principle only in those cases in which the interpreted author is taking a position similar to that of the interpreter. Otherwise we simply substitute our perceived truths for the supposed errors of the interpreted author. Further, an over-extension of the principle of charity may lead to the interpreter not even perceiving deviations from the interpreter’s own beliefs, convinced as the interpreter is that she should reconstruct the author’s beliefs under the lights of her own (Lehrer 1990: 133). In extreme cases, this amounts to nothing less than denying logical possibilities which are essential for fruitful inquiry. Consider for example a debate between Galileo and advocates of a geocentric cosmos in which the later astronomers were to interpret the statements made by Galileo. If they applied the principle of charity globally, they could come to the conclusion that the thing which Galileo calls “sun” and assumes to be at the center of a “solar system” in fact means “earth,” and is at the center of a series of concentric celestial spheres. The real substantive difference of positions would thus be negated, and the matter at issue obfuscated.

This last point brings us to a peculiar and apparently unavoidable dynamic in the principle of charity: that those who wield it invariably exercise a certain hermeneutic generosity from an epistemically privileged position. Such a constellation does not even admit of the possibility that the interpreter might have something to learn from the text she interprets. Yet on what grounds could the interpreter insist that the interpreted author’s beliefs and intentions are identical with her own? If she sees the author’s beliefs as different but absurd, there remains at least the possibility that the interpreter herself is wrong. Perhaps more interestingly, the ability to suspend judgment on apparently absurd views of the philosophical past leads us to a possibility which gives the interpretation of historical philosophical texts a good part of its philosophical value – the assumption that the author we interpret could be right.

Historical Fallacies of Philosophers and the Relationship of Philosophy to its History

Contemporary academic philosophy generally grants that the history of philosophy has some sort of interest, but this interest is seldom justified with reference to a genetic account of contemporary philosophical theories. Rather, contemporary philosophers tend to believe that they need not understand the history of their current problems in order to adequately understand the problems themselves. This view that the history of philosophical theories and problems has little or no bearing on their systematic analysis has made for an institutional environment in which the historical approach to systematic

problems has become suspect. It is therefore understandable that the question of contemporary philosophy's relationship to its past is a topic which is frequently passed over.

In his *Lectures on the History of Philosophy* Hegel emphatically states his own philosophical conception of the history of philosophy. Philosophy is, for Hegel, historical in two senses. It is historical in the sense that it progresses through its development in time, and is intimately related to its place and time, though not simply determined by them. Philosophy is, however, also historiographic in its methodology, for in order to understand what is actually true in the philosophical present, one must study its generation in the philosophical past. A philosophical understanding of the current state of philosophy always involves an understanding of the philosophical past. This view of the relationship of philosophy to its past proved highly influential for both historians of philosophy and for philosophers such as Karl Marx and Martin Heidegger, and has been echoed by more recent philosophers such as Alasdair MacIntyre, Charles Taylor, and Bernard Williams. Furthermore, recent work in the "history of concepts" (Hampsher-Monk et al. 1998) suggests that the history of philosophy is valuable because the conceptual possibilities of the present are the result of what was known and thought in the past. Thus, to a certain degree at least, a proper understanding of our current philosophical problems may indeed call for a proper understanding of their past.

It follows from the above that our outline of historical fallacies of philosophers is not meant to imply that those philosophers interested in a systematic approach to problems should disregard the history of their discipline altogether. It is only meant to argue that the methodological framework in which the historiography of philosophy is to be practiced cannot be provided by philosophy alone. By invoking the negative methodology outlined above, we have tried to show that this methodological framework will have to be provided by a theory of historiography. The question whether philosophical research is empty without knowledge of its history remains an open and fiercely discussed topic; what can be asserted in concluding this entry is that history of philosophy without a proper historiographical methodology remains blind. This is particularly true of the philosophy of historiography.

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Part III

Philosophy and Sub-fields of Historiography

Philosophy of History of Science

NICHOLAS JARDINE

Introduction

It was widely held in the nineteenth century that philosophical reflection could articulate the basic modes of progress in the sciences and hence help to provide a general framework for the history of science. Though under increasing attack for much of the twentieth century, this view has persisted. In the first section I consider it as represented in the writings of the rationalist William Whewell (1794–1866), the empiricist Ernst Mach (1838–1916), and the conventionalist Pierre Duhem (1861–1916). The next section looks at the rationally reconstructed history of science of Imre Lakatos (1922–1974), in relation to the strongly opposed views of Thomas Kuhn (1922–1996) and Karl Popper (1902–1994). In the latter part of the twentieth century the relevance of philosophy to the history of science came to be considered in more modest terms: not as a contributor or articulator of grand narrative structures, but as one source of insights into particular historiographic problems of interpretation, explanation and narration. In the final section, I touch on some of these more specific issues.

Please note that in this chapter the term “history” is used in connection with the sciences in the broad sense usual in the writings of the philosophers whose views I discuss. This covers past developments in the science, research into those developments, and writing about them; that is, “history,” “historiographic research,” and “historiography” as defined in the editor’s glossary of terms.

Critical Narratives

In his monumental *History of the inductive sciences* of 1837 Whewell organized the history of each science according to a dramatic scheme of “epochs,” in which bodies of distinct facts were brought together under clear ideas to produce new general truths, such discoveries being at the hands of “the great philosophical names which all civilized nations have agreed in admiring” (cf. Cantor 1991; Fisch 1991). Preceding each epoch was a “prelude,” in the course of which facts were gathered, ideas clarified, and links between ideas and facts established. Following epochal discoveries were “sequels,” in which the consequences of the new theories were worked out,

controversies about them settled, and their contents “diffused to the wider throng of the secondary cultivators of such knowledge” (Whewell 1858: 10). The sequence of preludes, epochs and sequels had by no means followed a steady course; for there had been “stationary periods,” notably in the Middle Ages, an era marked by “Indistinctness of Ideas,” the “Commentatorial Spirit,” “Dogmatism,” and “Mysticism” (1858: 11–12, 182). Moreover, the sciences differed widely in degree of development, ranging from astronomy (“a full-formed matron”) and mechanics, in which substantial bodies of secure knowledge had been acquired, to the “organical” sciences of physiology and comparative anatomy and the “palaeteological” science of geology (a “tottering girl”), which still awaited their Keplers and Newtons to bring about an epoch (Whewell 1831: 390).

In his likewise weighty *Philosophy of the Inductive Sciences* of 1840, Whewell introduced a “Fundamental Antithesis” between subjective thoughts and objective things. Accordingly, he distinguished theories and ideas, in the realm of thought, from facts, which though inevitably laden with concepts are far closer to the realm of things. At epochs the fundamental ideas which define each science – space in the case of geometry, matter and force in the case of mechanics, likeness in the case of natural history (viewed as the science of description and classification of plants, animals, and minerals), etc. – were “explicated” and wide ranges of ascertained facts were “colligated” under them, so that the truth of the resultant theory was established by a “consilience of inductions” (cf. Snyder 2005 and “Colligation,” ch. 12, this volume) Through “inductive ascent,” the laws derived by one colligation of facts under ideas could themselves serve as facts for the next inductive step. Moreover, Whewell now made strong claims about the attainability of certainty in the natural (or, as he called them, “material”) sciences.

Whewell viewed the scientific method as a species of interpretation. He often drew analogies between the roles of skill and ingenuity, conjecture and insight, in the inductive sciences and in textual criticism. Thus, although a general account of the means of attainment of truth through application of clear ideas to distinct facts could be given, and although rules could be laid down for certain special types of induction, the process of discovery, like the finding of the meaning of texts, could not be reduced to rules. For in the preludes to discoveries a substantial role was played by the growth of practical knowledge, in which ideas were implicitly rather than explicitly applied. Moreover, though conditions for epochal discoveries could be laid down – a clear and steady possession of abstract ideas, and an aptitude in comprehending ascertained facts under them – “no maxims can be given which inevitably lead to discovery. No precepts will elevate a man of ordinary endowments to the level of a man of genius” (Whewell 1847: vol. 2, 20–1).

Whewell’s vision of the progress of the sciences was providential. For the fundamental ideas whose clarifications and applications had mediated the advancement of the sciences were, for him, no mere human contrivances; rather, they were divine ideas, realized in the creation and innate, if generally unrecognized, in the human mind. Later nineteenth-century historians of the sciences likewise often treated scientific progress as something inevitable, transcending the particular circumstances of production of hypotheses and theories (von Engelhardt 1979: pt. IV). In stark contrast to such “fatal” histories, Mach viewed the progress of the sciences as the product of contingent

human contrivances in response to specific and local needs and problems. Where for Whewell history revealed the way in which the most advanced sciences had become “permanent” bodies of certain knowledge, for Mach its function was not to celebrate the certainties of the sciences, but to unmask them as historical accidents that could well be replaced by very different principles of equal efficacy in organising the phenomena. For Mach the study of history was essential at every level of the understanding of science: it provided the only way in which the most fundamental physical problems could be grasped; the only way in which the status of the principles of the sciences could be critically assessed; and by far the most effective way of teaching the methods and content of the sciences (Hiebert 1970).

It was in Mach’s *Die Mechanick in ihrer Entwicklung historisch-kritisch dargestellt* (translated as *The Science of Mechanics*) of 1883 that this historico-critical method was most fully developed (d’Elia 1971). All physical knowledge had, he maintained, originated with magical secrets and artisan knowhow. A first phase of development in a science was the fixation of publicly expressed principles based on observation. This was followed by a deductive phase, in which subsidiary principles were developed for the effective solution of the various problems that arise as the range of application of the science is extended. Finally, there was the formal phase, in which “it is sought to put in a clear compendious form, or *system*, the facts to be reproduced, so that each can be realized and mentally pictured with the *least intellectual effort*” (Mach 1893: 516). These phases of development constituted the narrative framework of *The Science of Mechanics*.

For Mach the primary lesson to be drawn from this philosophy of history was the value of criticism of predecessors and, above all, exposure and elimination of their metaphysical assumptions, notably including all that had to do with the arch-metaphysical notion of causation. The task of natural science was to connect the primitive facts, which Mach called “sensations,” and to facilitate the prediction of some from others. For that purpose scientists made up theories. Those theories were never uniquely determined by the facts; many different theories could organize and predict sensations, and the aim of scientists should be to find the most economical.

Mach’s vision of the world, as set out in *Die Analyse der Empfindungen* (*The Analysis of Sensations*) of 1886, was monist. Sensations were neither physical nor psychological, neither subjective nor objective, but neutral. Science, Mach complained, remained split into physics – the study of sensations in their objective guise – and psychology – the study of sensations in their subjective guise. The proper goal, however, was a unified psycho-physics; and he viewed his own extensive researches on psycho-physical phenomena, especially optical illusions, as a first step in the direction of this final science. Mach dreamed of an ultimate science that would dispense with all theories, so that in the end scientists would wield nothing but brute formulae for the correlation of sensations. Thus Mach’s history of science and his philosophy of science were united by underlying assumptions about the progress of science through elimination of metaphysics and theory in the quest for a unified and economical calculus of facts.

Like Mach, Duhem held that scientists should avoid metaphysical questions, concentrating, rather, on the quest for theories that economically classified and predicted the facts (Brenner 1990; Martin 1991). But they had very different motives for wanting to eliminate metaphysics from science. For Mach, this was part of a general program for purging human culture, starting with its educational institutions,

of all forms of superstition and prejudice. For Duhem, the aim was rather to make the sharpest possible distinction between science and metaphysics, so as to show the irrelevance of science to issues of faith and theology. Further, Duhem's notion of economic organization of fact by theory differed markedly from Mach's. In his major philosophical work, *La théorie physique. Son objet et sa structure* (translated as *The Aim and Structure of Physical Theory*) of 1906, the chapter on "Experiment in Physics" opened with the declaration that, "An experiment in physics is not simply the observation of a phenomenon: it is, besides, the theoretical interpretation of that phenomenon"; and shortly after Duhem noted that "this interpretation substitutes for the concrete data really gathered by observation, abstract and symbolic representations which correspond to them by virtue of the theories admitted by the observer" (Duhem 1969: 144, 147). For Duhem facts became relevant to science only when interpreted in the light of theory. There was simply no place in science for everyday descriptions of facts; for, compared with the precise symbolic languages of science, ordinary language was hopelessly vague and ambiguous.

Like Mach, Duhem held theory to be underdetermined by facts. He was radically holist – for him the laws of physics did not engage one at a time with experimental facts. For to derive such facts from theories, whole sets of laws and boundary conditions were needed; hence, when a prediction failed, "The only thing the experiment teaches us is that among the propositions used to predict the phenomenon and to establish whether it would be produced, there is at least one error; but where this error lies is just what it does not tell us" (1969: 185). How was a choice to be made of what to abandon when faced with recalcitrant facts? For Duhem there were two basic guides: the "principle of simplicity," namely, to keep theories as simple as possible, mathematical elegance being the preferred type of simplicity; and the "principle of conservatism," namely, to preserve as far as possible existing theories with their power to order the experimental facts.

For Mach, theories were purely heuristic devices; they were not about a real world, because the only true realities were sensations. Duhem's position was different, startlingly so. For scientists, theories were to be treated instrumentally, as convenient fictions. But the *critical philosopher*, who studied the whole history of science, could discern something more. Successive theories embodied ever more natural classifications of the experimental facts, and in so doing they progressively revealed the real structure of the world. Indeed, according to Duhem, this natural classification was the one glimpsed by St. Thomas Aquinas and was re-emerging, partly through Duhem's own work, following the development of a general thermodynamics. ("The Physics of a Believer," in Duhem 1969)

In *Sôzên ta phainomena* (translated as *To Save the Phenomena*) of 1908, Duhem had traced an astronomical tradition, from Plato to the seventeenth century, in which hypotheses were considered as mathematical fictions designed for the purposes of saving the phenomena. Paradoxically, he concluded, it was Kepler and Galileo, vigorous opponents of this view of the status of astronomical hypotheses, who, in bringing together celestial and terrestrial physics, had prepared the way for the extension of the fictionalist approach to physics as a whole. Duhem's monumental *Système du monde* (*System of the World*) offered far fuller reconstructions of astronomical and cosmological systems from Antiquity to the Renaissance. Throughout the work

Duhem was concerned to trace continuities. In particular, he argued, contentiously given the widespread contemporary perception of the medieval period as the “Dark Ages,” that anticipations of Galilean dynamics were to be found in fourteenth-century Parisian works (Murdoch 1991). For Duhem, unlike Mach, the message of the history of science had little to do with criticism of prejudices, and everything to do with respect for tradition. Moreover, he made it abundantly clear that the true guardians of the scientific tradition had been French *bon sens*, strongly opposed to German dogmatism and English woolly-mindedness, and the Catholic Church, committed to curbing the metaphysical pretensions of science.

Rival Reconstructions

Philosophies of the history of science, some rationalist, some conventionalist, many empiricist, proliferated in the nineteenth century, notable examples being those of Auguste Comte, J. S. Mill, Hermann von Helmholtz, Heinrich Hertz, Friedrich Lange, Wilhelm Ostwald, and Abel Rey. In the course of the twentieth century they were, however, challenged by the insistence – at first associated with logical positivism and with the movement against psychologism in philosophy – on a sharp distinction between contexts of discovery and contexts of justification in the sciences, the former being a matter for historians, psychologists, etc., and only the latter the business of philosophy (Kusch 1995).

Kuhn (1962: 9), like Lakatos, was strongly opposed to the context of discovery/context of justification divide. In particular, both contested Popper’s distinction between, on the one hand, the logic of testing and refutation which demarcates science from pseudo-science and, on the other, the psychology of theory formation. However, Kuhn and Lakatos were themselves at odds on the issue of demarcation. For Lakatos, demarcation of true science, which displayed the workings of reason, was possible, though not in terms of Popper’s simplistic criterion of falsifiability (Hacking 1979; Larvor 1998). According to Kuhn, there was no such domain of rational science. Rather, he distinguished “normal science” from “revolutionary science.” Normal science was governed by paradigms, in which “accepted examples of actual scientific practice – examples which include law, theory, application and instrumentation together – provide models from which spring particular coherent traditions of scientific research” (Kuhn 1962: 10). Revolutions occurred when a paradigm entered a state of crisis and was replaced by a new one. The transition from one paradigm to another was a sudden business, a “conversion” or “gestalt switch.” No universal rational standards governed such revolutions; rather, “As in political revolutions, so in paradigm change, there is no standard higher than that of the relevant community” (1962: 96).

Lakatos too took as his units of appraisal research programs rather than theories. But for him the choice between research programs was a rational matter depending upon their track records. Each program had a “core” of principles, a “heuristic” of methods and techniques, and a “protective belt” of modifiable auxiliary hypotheses. “Progressive” programs were the ones which developed in accordance with their heuristics, yielding successful novel predictions. Degenerating programs, by contrast, underwent ad hoc modifications in response to recalcitrant facts and yielded few

successful predictions. Reason, often with long hindsight, opted for the progressive programs.

For Kuhn the pattern of scientific development was primarily the business of historians, not philosophers. Lakatos' position was more subtle and more interesting. The task of the philosopher was to formulate scientific methodologies and to assess them in terms of their success in providing rational reconstructions of the history of science. According to Lakatos, by this test his own "methodology of research programmes" triumphed over its rivals, conventionalism, inductivism, and falsificationism (Lakatos 1978: 102–38). Kuhn memorably denounced Lakatos' rationally reconstructed history as "not history at all but philosophy fabricating examples" (1978: 192). Lakatos responded vigorously that "all historians of science are *always* philosophers fabricating examples," and that his own fabrications contained more truth than Kuhn's more colorful ones. Further, in reflexive accordance with the core principles of his methodology of research programs, he showed himself prepared to tinker with the details of that methodology in the face of recalcitrant historical data. Thus, faced with the failure of Copernicus' research program to yield novel predictions in its opening decades, he adopted Zahar's suggestion that it suffices for progressiveness if a program "predicts" known facts that played no part in its formulation (1962: 184–8).

Philosophical Problems

Kuhn and Lakatos were diametrically opposed on the issue of the roles of reason in the major transformations of the sciences, and hence on the relevance of philosophy to the historiography of science. But they shared a commitment to grand narratives, whether of the succession of paradigms or of the competition of research programs. Since the 1970s confidence in such "Big Pictures" has abated (Secord 1993). Historiographic and sociological studies devoted to the reconstruction of past sciences from the standpoints of their practitioners have revealed ever more clearly the sheer variety of the presuppositions, goals and methods of the sciences from field to field, period to period and place to place (Dupré 1993; Galison and Stump 1996). Where once it seemed that the history of science and its philosophy had a common concern with the development of science as a whole, their interaction now appears more local. At best the two disciplines may work together to illuminate more specific problems in the domains of historical interpretation, explanation and narration.

An urgent problem faced by historians of science in all their activities is that of anachronism. In interpretation the problem is acute because the sheer otherness of past institutions, practices, and doctrines often forces the historian, on pain of losing contact with present-day readers, to commit conceptual anachronism by describing those institutions, practices and doctrines in terms far removed from those available to the agents themselves. The problem is likewise pressing in relation to explanation, given that so many of the forms of explanation – economic, sociological, anthropological, evolutionary, psychological, even psychoanalytic – that historians of science use to account for changes of the sciences are far removed from any terms in which past practitioners could have conceived their own actions. Anachronism of selection is at issue with respect to choice of research topics and narration of findings, for there is a

strong temptation to attend preferentially to disciplines that gave rise to our sciences, to questions and doctrines ancestral to those we take to be important, to books that are classics by our lights, etc., rather than focus on the disciplines, works and theories most valued in their own periods. These are contentious issues. Some, Quentin Skinner (1969), for example, have taken very strong lines, arguing on Wittgensteinian grounds that past actions can be understood by historians only in terms that the agents themselves could have accepted as accounts of their actions. Others Jardine (2000), for example, have argued for a more liberal approach, one which distinguishes gross anachronisms – those which rest on preconceptions inapplicable to the agents and their institutions – from harmless anachronisms useful for purposes of communication, explanation and so on. As for anachronism of selection, many have followed Herbert Butterfield's *The Whig Interpretation of History* (1931) in condemning its roles in the telling of triumphal stories of progress (Spoerhase 2004). It is, however, hard to see how historians of ancient, medieval and early-modern science can avoid such anachronism altogether; for the alliance of disciplines that we call "science" was consolidated only in the opening decades of the nineteenth century.

In the domain of explanation a host of philosophical and methodological problems arise concerning the legitimacy of the various types of explanation of scientific belief. Can the growth of knowledge usefully be explained, as urged by Popper (1972), in terms of an evolutionary mechanism of trial and error in which "our hypotheses die in our stead"? If so, what are the units of selection and replication: can concepts, or theories, or methods be considered as "memes," analogous to genes in these roles? (Aunger 2001; Wheeler and Boden 2002). Turning to communication in the sciences, awkward questions arise concerning the possible means of communication of tacit know-how (Turner 1994). As for communication of explicit knowledge, what parts can be played by images, models and instruments: are they mere aids to verbal communication, or do they play distinctive parts in their own rights? (Lynch and Woolgar 1988; Jones and Galison 1998; De Chadavarian and Hopwood 2004)

An explanatory issue that has loomed large in recent philosophy of science concerns the legitimacy of invoking a fact to explain a past true (or approximately true) belief in that fact. It has been argued, for example by Latour (1987), that such appeals are inevitably circular, and by Bloor (1976) that they violate a symmetry condition according to which historians and sociologists should confine themselves to explanations of types applicable to both true and false beliefs. Some, Bloor for example, have held that the symmetry condition is compatible with scientific realism; others, Collins (1985), for example, that it entails an outright relativism about scientific beliefs. Lewens (2005) has argued that the thesis is quite neutral on such metaphysical issues, and Tosh (2006) that it is misleading insofar as it purports to offer guidance to historians of science.

Finally there is the issue of the purposes and strategies of narration in the historiography of science. Whewell, Mach, and many other nineteenth- and twentieth-century philosophical historians explicitly addressed their works to critical ends: to the reform of science and its methods, and to education in sound scientific practice. Following the Second World War, with the development of History of Science as an independent academic discipline, disinterested and specialized scholarly approaches flourished, and the more openly committed types of historiographic narration fell under suspicion. But

since the 1980s, with the growing confidence of the once marginal discipline, there has been a notable re-engagement with moral and political issues, and extensive new engagement with issues of gender and ethnicity. Awkward philosophical and methodological questions arise in this connection. In the 1890s Heinrich Rickert (1899) famously argued that in the cultural sciences, including history, unlike the natural sciences, material is inevitably selected in the light of cultural values, and many have since questioned the very possibility of strictly disinterested history. However, once legitimate didactic, moral and political goals are admitted, pressing questions arise concerning the ways in which they can be most effectively pursued without loss of scholarly rigour. For example, are our current attitudes to, and uses of, the sciences best criticized by exploring their most recent histories; or can studies of more remote periods yield the proper “critical distance” from which to scrutinize them?

Theoretically inclined historians of science these days tend to turn to sociology and anthropology for inspiration, not to philosophy. Philosophers of science still dabble for examples in the history of science. But only a few – Hacking (1983, 2002), Daston (1995), Galison (1987), and Jardine (1991), for example – take historical issues to be central to the philosophy of science. This separation of the disciplines is most regrettable, for as Lakatos observed, “Philosophy of science without history of science is empty; history of science without philosophy of science is blind” (Lakatos 1978: 102).

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Philosophies of Historiography and the Social Sciences

HAROLD KINCAID

There is no single answer to the question of how historiography and social theory relate for several reasons. For one, the relation between the two might be contingent and causal or it might be conceptual and necessary. For example, some historians and social theorists may have been intellectually influenced by one and another. That is a different claim than that there is something essential about historiography and social theory such that they are inevitably related. Furthermore, we can also ask if the relation between the two is mutual or if it is only one directional – from historiography to social theory or vice versa. Equally important is the fact that “historiography” and “social science theory” can refer to very different things. What gets called a “theory” in science often varies a great deal and there is no reason theories in the social sciences should be any different; historiography is a complex practice with many different components.

Thus answering the question of the relation between historiography and social theory requires specifying what parts of historiography and social research one is talking about, how one thinks of theories and their place in science, and whether one is looking for historical influence or some deeper conceptual connection. It is also important to distinguish what historians and social theorists say about the relation as opposed to what the relation really is – historians and social theorists, like all scientists, need not be infallible in understanding what they do. My concerns in what follows will generally not be the intellectual influences the two have had on each other except when those influences illustrate deeper conceptual connections. Correspondingly, my interest is less in what historians and social scientists say about the relation than in what in fact has been the relation. In some domains, I shall argue, social science theory is essential to historiography and historiography essential to social science theory and in others there is little relevance in either direction. My goal in what follows is to adduce some evidence about which domains those are.

An Intellectual Historiography

It should be useful now to set the stage and further elaborate the crucial distinctions made above by a short intellectual historiography of the history of historiography, social theory, and philosophy of science. Prior to the twentieth century there was no clear

distinction between social theory and the historiography of society. Montesquieu, Millar, Ferguson, Smith, Marx, and Weber wrote historiographies of society and social theory that were tightly intertwined. That tradition was in part inspired by Newtonian physics as the exemplar of scientific study of nature. However, the Newtonian paradigm was not interpreted uniformly. On the continent it was the mathematical formulation of laws that received priority while the British, e.g., Maxwell, emphasized mechanisms in the sense of a continuous causal process. The Enlightenment social scientists-historians found place for both strands in their vision of a “philosophy of society” (Millar’s phrase), talking about laws of development that identified the deep causes behind social change.

As historiography became professionalized during the nineteenth century around the Rankean approach, large-scale historiographies of society were eclipsed by the use of public sources to triangulate to facts about the past, particularly facts about the state and politics (Tucker 2004). As social theory become professionalized a bit later, as sociology and political science (economics became established earlier) led to the establishment of the social sciences, there was often an insistence on a unique realm of social facts and the relations between them. For historiography, social theory dropped from the scene; social theory in turn became something that did not have to reflect or be confirmed by history nor be explicitly about causes. Indeed, eliminating historical references and causal explanation went hand in hand. As I argue below, inquiring about causes naturally leads to inquiring about history. Social theory emphasized the nomic side of the Newtonian tradition, searching for laws, and that came at the expense of theorizing about causes or correlations or (admittedly ambiguous) functional relations.

Though renegade exceptions always persisted, the pole of attraction by the 1950s for historiography was overwhelmingly the inference of specific past facts on the basis of public documents and more or less commonsense background information devoid of social theory. Social theory in its turn valued law-like relations between variables as its most important output. Typically, social scientists would conduct large-scale surveys that would support regression equations, describing the relation between variables. That vision still predominates in much current social science.

Social theory was aided in that vision by positivist apparitions of science that dominated through the 1950s. Two key pieces of that approach are represented in Hempel’s two main projects of finding a formal account of explanation and a logic of confirmation. Hempel argued that explanations were deductive relations between statements where at least one was a law of nature. Laws of nature were thought to be identifiable by formal criteria such as universal scope. Concerning confirmation, Hempel was after a set of universal criteria, formal in nature, that would tell us when a piece of evidence supported a hypothesis and how much it did so. The social science tradition that took theories to be universal relation between variables and statistical tests as decisive certainly fits Hempel’s prescriptive version of philosophy of science.

Hempel himself applied these ideas directly to the question of how historiography and social science theory relate and most of the explicit discussion of this issue has centered around his contributions. Hempel argued that because explanation required stating the relevant covering law, historiographic explanations always at least

implicitly relied on a law that covered the event or regularity to be explained. Those laws he thought were the laws of social science and thus he thought that there was no non-trivial historiography without social theory. This produced a sustained debate through the 1950s and 1960s about whether historiography actually relied on such laws and about its scientific status if it did not, all framed by Hempel's assumption that explanation is subsuming under laws.

Developments in postpositivist philosophy of science starting in the 1950s rejected Hempel's picture and the arguments that were made are crucial for the points developed in the rest of this chapter. Among the key ideas from the postpositivist philosophy of science that are relevant here we should emphasize that testing is holistic: finding evidence for or against an hypothesis always requires prior background knowledge that is domain specific. Philosophical accounts of science are continuous with, constrained by, and ultimately judged according to the empirical standards of the sciences. There is no purely formal and informative account of laws; laws as universal generalizations are often unavailable in some of the best scientific work. There is no purely formal inductive logic that does significant work in scientific inference. Much of good science concerns finding causes; this can be done by a host of means that usually assume some prior background causal knowledge; asserting a causal claim may entail that there exists some law relating cause and effect, but causal claims can be confirmed without precise knowledge of what that law is. Explanation depends importantly on contextual and pragmatic factors; citing laws is neither necessary nor sufficient to explain. Scientific practice is considerably more complicated than simply testing theories against the data. What gets called "theory" is often differently interpreted even within the same scientific discipline, sometimes not fully formalizable, and a batch of claims varying from law-like statements to much more piecemeal and ad hoc elements. The "data" is information at different levels of prior processing and almost always requires some background theory to interpret, with that theory often being considerably more specific and relatively independent of the theory being tested. Understanding the social processes of science is essential to understanding science and to evaluating its validity; social processes can be essential to objectivity, can produce biases that even good science is unaware of, and can play multiple epistemic roles that have to be investigated case by case.

These developments need to be supplemented with parallel developments in the philosophy of the social sciences. Philosophers and some social scientists have long thought there was a fundamental divide between the natural and the social sciences. They pointed to the alleged fact that the natural sciences are about describing universal laws about a world of brute facts grounded in the certitude produced by the logic of experimental inference and noting that the social sciences are not like that (Taylor 1971). However, that argument presupposes a picture of science that postpositivist developments rejected. Much natural science is about describing causes based on nonexperimental evidence – Darwin, for instance, comes to mind. Moreover, careful observation of "experimental logic" in the hardest of sciences found that experiments need a great deal of interpretation and categories to describe the world that are theory dependent in various ways (Pickering 1984). So brute facts, laws, and purely logical inferences are in general not the provenance of science. Thus the social sciences stopped looking so different from the natural sciences.

Philosophers of the natural sciences learned these lessons in part by adopting the naturalist view that science and philosophy are continuous and that philosophy of the sciences has to look at how real science works. Those morals have steadily albeit slowly percolated into the philosophy of social science as well. Work in the philosophy of the social sciences has often become about specific social sciences, archaeology (Wylie 2006) or economics (Hausman 1992), for example. How those sciences themselves work became a much more important question than it had been previously and there has been a diminished pretension to be able to judge entire areas of investigation as good or bad science based on a priori standards produce by conceptual analysis or armchair and simplistic visions of the “logic” of the natural sciences. Social science research, like all scientific investigation, is seen as a complex practice which must be analyzed with attention to detail and the different components that make up the process of providing evidence and explanations. So, for example, issues concerning functional explanation have gone from conceptual analysis based on intuitions about ordinary language and the question how they fit with the nomological deductive account of explanation to questions about what kind of causal processes social scientists are proposing and of what would count for evidence about them (Elster 1983; Kincaid 2006).

Needless to say, both these developments and the general changes in philosophy of science they instantiate have significant implications for questions about the relation between social theory and historiography. Some are obvious, e.g., that the study of history does not require having general laws on the books before it can explain or that determining the place of social theory requires paying attention to what historians actually do. Other implications are less direct and will be developed in more detail as we go along.

Why and When Historiography Needs Social Theory

Historiography certainly embodies several of the general claims about science mentioned above. For the purposes of this chapter, one important fact is that the practice of historiographic inquiry is a multifaceted enterprise. The various components involved, therefore, need not have a uniform relation to social theory.

Tucker (2004) makes good use of this insight to argue that crucial parts of historiography are quite independent of social theory. Fundamental to scientific historiography is the use of present traces of the past – paradigmatically surviving documents – to argue to the past events that in part produced them. Tucker argues that this is the evidence base of historiography and largely proceeds apart from social theory of the sort produced by the social sciences. It can proceed independently because inferring past events from current traces does not require large-scale social theory. Much more mundane and piecemeal background assumptions can do the job. So to take Ranke’s example cited by Tucker, memoirs are generally less reliable than other kinds of documents. That is because we know from common sense that individuals reporting about themselves are often not entirely objective. No large theory of macrosociological structure and change is needed to know this. Thus a standard and fundamental practice of scientific historiography is logically independent of social theory.

Tucker also argues that scientific historiography should be distinguished from historiographic interpretation, which covers such large-scale issues as the proper narrative, the important causes, and the political significance of some period of history, work which is the usually stuff of historiography books, but not scientific historiographic research in specialized journals. The former frequently borrows or proposes social theories, but it is a mistake to infer from that association to the conclusion that scientific historiography involving present traces and their explanation by past events is essentially tied to social theory.

These distinctions are important and show that there need not be logical connections to social science theory in all aspects of historiographic practice. Yet that is far from showing that historiography never needs social theory, and I think Tucker overstates his case even for the lower-level practice of explaining present traces by past events that cause them. I outline a number of ways that social theory is relevant throughout historiography, albeit in differing degrees and ways (cf. Burke 1992).

Let's start with the low-level piecemeal inference from current documents to their past causes that Tucker emphasized and that seem to be the best case for historiography free of social theory. No doubt commonsense generalizations – “memoirs may be biased” – play an important role. Yet the social and psychological sciences have many useful results that should be relevant here. Experimental work shows testimony by independent parties to be frequently dubious; sociological investigations of collective memory show how value laden it can be. Both these results are just a few of the many sociological and psychological results that ought to be relevant to assessing the value of extant human reports in inferring past events. Of course this work is not “theory” in the sense of universal laws that can be explicitly stated. However, if we realize that major parts of the natural sciences produce no such thing either, then this work is social theory in the same sense as there is theory in the natural sciences.

Constructing historiographic evidence for past causes from current traces also relies on social theory in importing *categories* and *models*. It is a philosophical truism that in describing reality we inevitably must use language and hence must use some general categories or predicates in the process. However, general categories are the stuff of theories and thus theories are to some extent inevitable in scientific historiography.

This is an abstract philosophical argument and we should take care not to give it too much weight as it stands. General categories need not be those of social theory as we know it and a commitment to the use of general categories may not commit us to very much in the way of theoretical claims about those categories. However, it is an open empirical question how much social theory scientific historiography is committed to because of the use of social theoretic concepts. Let me bring one typical case where there is a clear commitment by historiography to social theory: the study of nineteenth-century US slavery. Social categories are involved in ways both obvious and implicit. Slavery is of course a general social category about which there are competing social science theories. Some scientific historiography – for example, determining the nutritional status of American slaves – can perhaps be carried out without taking any stand on those theories. However, much that we want to know about slavery does indeed require a good amount of social theory. Considerable work has gone into compiling data to evaluate the comparative efficiency of slave production compared to production with free labor at the time. Many assumptions from economic theory are

required to do so, and much of the historiographic debate involved debates over whether those assumptions from economic theory actually hold (Fogel and Engerman 1974).

One reason that social theoretic categories are likely to be essential is that the social sciences have shown that every human society is constructed around some kind of division of labor and power according to social roles and statuses. You can disagree on what they are – e.g., on whether “feudal peasant” picks out a useful and defensible category – but there is no good evidence that some human populations have no such divisions of labor or power. So it seems inevitable that historians in gathering evidence of what happened in the past from current sources will have to infer that the past had a social organization of some sort. That conclusion commits historians to using key concepts of sociological theory, if not specific theories.

So far I have argued that historiography has good reasons to invoke social categories and to that extent social theory. Another reason that social theory can be essential follows the historiographic use of models. It imports models when, for example, it is interested in causes of historical events. Scientific historiography in specialized journals goes beyond just asserting that past events happen that explain current documents, etc. It also asserts that past causal relations explain current traces. The work on slavery mentioned earlier tries exactly to do that – to explain the causes in the differences in productivity that the primary sources seem to show. Historians have essential interest in historical causes and do their best to provide evidence based on what we currently think we know to infer what those were. That means theories are involved, at least in the more realistic sense of theories as models of causal processes. These models may not be any more elaborate than some listing of possible causes and their causal direction. This may seem an anemic sense of “theory” but it is precisely the kind of “theory” evolutionary biology uses in investigating the past. This does not require unexceptionless laws as Hempel thought, but then Hempel was wrong about what theories were in natural science as well.

Another important way historiography needs social theory comes from the need to understand the social process of its own investigations. Science is through and through a social process, that such processes need not be an obstacle to good science, that such processes can indeed reduce the reliability of scientific inquiry, and thus that awareness of social process is one component in assessing the reliability of scientific data. Physicists, for example, know that there are “bandwagon” effects when they try to determine the value of the fundamental physical constants and make attempts to adjust for such social biases.

If social processes need to be taken into account in evaluating physics, it should be no surprise that they need to be taken into account when assessing the research and reasoning processes and results of scientific historiography. How important these processes are surely must vary depending on which aspect of historiographic practice one is considering. Much feminist criticism of historiography identifies some aspects of current practices that seem to result in part from current gender roles.

Social influence is most important when historians infer the causes of past events, especially in deciding which causes were more important than others. Current work in philosophy of science shows decisively that the determining causes and causal importance has a large contextual component. What is “the cause” or an important

cause depends on what one is holding constant and what questions one is asking. This leaves plenty of room for unspoken assumptions based on larger societal values to play a role, a role that needs to be understood if we want to judge the reliability of scientific historiography. That understanding is likely to involve recourse to the resources of social theory.

Understanding social factors is necessary even for the seemingly most atheoretical historiography that proposes the occurrence of some past event as the best explanation of our current documents. The historiography of economic affairs can again provide a nice example. We have a variety of documents that we hope to explain by appealing to some historical total national income. But to do so, we need among other things, an understanding of what social and value assumptions influenced past economic actuaries.

Why and When Does Social Theory Needs Historiography?

I turn now to ask when and how social theory might rely on scientific historiography. Following the general themes of the first section, I think there is no all-purpose answer to this question. Yet I also think the dependence is wide spread, much more so than social scientists acknowledge.

Let us recall first the positivist ideal that has dominated much social science research since the 1950s: the purpose of research is the confirmation of theories formulated as laws relating dependent and independent variables. The laws are tested on cross-sectional data by means of multiple regression and related statistical techniques. This approach focuses on laws and hopes to validate them by applying logical, *a priori* rules of inference based on the probability calculus that underlie statistical tests.

A classic instance is Blau and Duncan's (1978) study of occupational mobility. Using large data sets, they estimate by path analysis the relative influence of years of education, father's status, and other variables on occupational achievement. Subsequent studies added further variables such as intelligence, on-the-job training, etc. Another more recent paradigm example is the work by economists studying the causes of growth and growth disparities in the nations of the world (Barro 1998). Gross Domestic Product per capita is the variable to be explained. A large set of possible independent variables such as human capital, openness to trade, and so on are used to construct regression equations. The coefficients of the independent variables measure their relative importance, and statistical significance is the measure of evidence for those variables.

The work in this tradition treats explanation as subsuming under laws. Not surprisingly, given the inadequacy of that notion of explanation, they waive the requirement for subsuming in many cases and frequently abandon talk of laws for talk of causes. Social theory wants to be relevant to policy and to normative judgments and both require identifying factors that are causally responsible for outcomes. However, once taken as providing causal information, these versions of social theory depend on some strong and implausible assumptions, *viz.* that causation can be demonstrated by cross-sectional data alone, that social causation is simple rather than complex in ways that I shall detail below, and that a theory describing causal relations can best take the form of equations relating variables.

The data sets in the studies mentioned above and standardly used across the social sciences are data “at a time”: they measure certain quantitative variables at a moment in time. In this case, given a population of individuals or of nations at a given moment, they measure the quantitative values of various variables for each individual or nation. Then scientists look for correlations between and within the series of measured variables. Of course, correlations of these sorts can be evidence for causal relations. However, there are good reasons (Cartwright 1989) for thinking that any conclusive evidence for causation in these cases requires previous background causal knowledge, e.g., of what the direction of causation can and cannot be. Our background knowledge tells us where to look for causally meaningful correlations, and which correlations are likely to be irrelevant or be screened by another common cause responsible for the correlation. Cartwright’s motto here – “no causes in, no causes out” – embodies and fleshes out the postpositivist claims described above that testing is always via background knowledge and that purely formal or logical accounts of fundamental scientific practices such as causal explanation are unlikely.

The upshot for social theory should be clear: reliable knowledge about causes can be considerably advanced by, and sometimes requires knowledge about the process of change in the factors studied over time, in short, historiography. Increasingly, social theorists wake up to this reality. A clear example comes from work in political sociology that advocates “process tracing” – the detailed outlining of the unfolding of events over time – as essential to confirming causal social theories. Of course, identifying changing events over time forces the social scientist to rely on scientific historiography to infer what happened in the past from traces we have in the present or to do their own such studies. Working with primary sources as part of developing and testing social science theory has gained more of a following among social theorists. Paige (1978) and Tilly (1998) are two prominently influential instances.

The second crucial assumption of the positivist inspired social theory is that causation is simple, not complex. Take the case of economic growth theory that is expressed in equations like:

$$Y = f(K, L, s, n, H, \text{ and } I)$$

where Y is GDP, K and L are capital and labor, s is savings rate, and n population size, H is investment in human capital, and I is a set of institutional factors – rule of law, property rights, size of government, etc.

This picture of causation makes some central assumptions such as (Abbott 2001). There is a fixed universe of individuals with a fixed set of properties that are distributed among them. For example, Blau and Duncan assumed there is a fixed set of occupations. This precludes occupations coming into and going out of existence, merging, or splintering, etc. The factors causally relevant at one time are the same at other times. Blau and Duncan’s model assume that the causal determinants of occupational achievement do not change. Finally, they assumed uniform effects, presuming that the influence of a variable does not vary according to context or background conditions. Interaction with context is a flaw that should be remedied by adding another variable representing the interaction effects of the two variables. In short, context can always be eliminated and each causal element makes an independent contribution to the outcome.

These causal assumptions of standard social theory have to be tested against everything we know about society, and that is one sense in which historiography has an essential place for social theory. From innumerable historiographic studies, conducted both by professional historians and social scientists, we know that social causation is not always as described above. Instead, we have good reason to believe that causation is complex. Social causes are seldom sufficient or necessary for their effects. Instead, their influence is conjectural. Events have causal influence only against specific backgrounds and they have no or different causal influences when those backgrounds are not present. Social causes may also be necessary causes in that they are essential for outcomes without having any independent causal influence at all. Studying these kinds of causes naturally leads to testing theory against history.

Conclusion

There is no single answer to the question of what is the relation between historiography, history, and social science theories. The more complex understanding of science that followed the positivists ensures that there are multiple possible relations which need not have a uniform treatment. Historiography surely is not just applied social theory. Yet some elements of the social sciences play crucial roles in some parts of historiography, and historiography is even more relevant and important for social theory in numerous ways. These relations are thus complex and this chapter at best has sketched some of them. There is much interesting further work to be done putting flesh on these bones and more to learn about both the practice of historiography and social theory.

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HAROLD KINCAID

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The Philosophy of Evolutionary Theory

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Darwinian evolutionary theory is a bastard offspring of Christianity. In that fact lies much about the theory itself as well as its presupposed philosophy of history.

The Christian Philosophy of History

The early Christians were not at all sure about the correct way to regard the Old Testament (Ruse 2001). It was St. Augustine, around AD 400, who finally settled things, arguing that the Old Testament was an absolutely vital part of the canon. But with the full acceptance of the Old Testament came the Jewish view of history. There was a beginning, there is a middle, and in the Christianized version, we look forward to an end.

All Europeans up to the Scientific Revolution of the sixteenth and seventeenth centuries thought that the universe is relatively recent. Archbishop Ussher in the seventeenth century put it at about six thousand years. All Christians thought that the six days of creation were true in some sense, as was the story of Adam and Eve and the universal flood some time after. But none of this was an essential part of history. Augustine, particularly, warned against simplistic literal readings of the Holy Scriptures. When he converted to Christianity, he took on the Old Testament, but he knew all about its problems – the two creation stories of Genesis for instance – and he stated explicitly that sophisticated thinkers of the Roman Empire should recognize that much in the text is written symbolically or metaphorically. It is intended for uneducated nomadic people.

Progress and Evolution

During the Enlightenment, the flowering of knowledge in the eighteenth century, Europeans considered the possibility that their religions might not be true. In the place of Providence came *Progress*. This was the idea that humans through their own unaided efforts – through science, through technology, through medicine, through education, through philosophy – could improve the lot of humankind, perhaps indefinitely (Bury 1920; Frankel 1948; Tuveson 1949; Ruse 1996).

Evolution came in on the back of progress. It was the story of social progress written into the rocks. Consider Denis Diderot, the philosopher and editor of the *Encyclopedia*.

Just as in the animal and vegetable kingdoms, an individual begins, so to speak, grows, subsists, decays and passes away, could it not be the same with the whole species? . . . would not the philosopher, left free to speculate, suspect that animality had from all eternity its particular elements scattered in and mingled with the mass of matter; that it has happened to these elements to reunite, because it was possible for this to be done; that the embryo formed from these elements had passed through an infinity of different organizations and developments . . . that it has perhaps still other developments to undergo, and other increases to taken on, which are unknown to us; that it has had or will have a stationary condition . . . that it will disappear for ever from nature, or rather it will continue to exist in it, but in a form, and with faculties, quite different from those observed in it at this moment of time. (Diderot 1943: 48)

Diderot was simply repeating his thinking about societies and their growth from the simple to the complex, from the savage to the Frenchman. "The Tahitian is at a primary stage in the development of the world, the European is at its old age. The interval separating us is greater than that between the new-born child and the decrepit old man" (Diderot 1943: 152). What happens in society is what happens in the world of living beings. This was likewise the thinking of Erasmus Darwin, grandfather of Charles and much given to expressing his thinking in verse. As he himself put it, life was a progress from monarch (butterfly) to monarch (king).

Organic Life beneath the shoreless waves
Was born and nurs'd in Ocean's pearly caves;
First forms minute, unseen by spheric glass,
Move on the mud, or pierce the watery mass;
These, as successive generations bloom,
New powers acquire, and larger limbs assume;
Whence countless groups of vegetation spring,
And breathing realms of fin, and feet, and wing.

Thus the tall Oak, the giant of the wood,
Which bears Britannia's thunders on the flood;
The Whale, unmeasured monster of the main,
The lordly Lion, monarch of the plain,
The Eagle soaring in the realms of air
Whose eye undazzled drinks the solar glare,
Imperious man, who rules the bestial crowd,
Of language, reason, and reflection proud,
With brow erect who scorns this earthy sod,
And styles himself the image of his God;
Arose from rudiments of form and sense,
An embryo point, or microscopic ens!

(Darwin 1803: 1, ll. 295–314)

He drew an explicit analogy between the upward path of culture and that of biology, the two notions really being but one. The idea of organic progressive evolution “is analogous to the improving excellence observable in every part of the creation . . . such as the progressive increase of the wisdom and happiness of its inhabitants” (Darwin 1794: 509).

Embryological Analogies

The early nineteenth century in France saw Jean Baptiste de Lamarck (1809) starting to expound his evolutionary ideas. In major part, his thinking was influenced by a then-popular idea that goes back to Aristotle: the chain of being, a life-picture that sees all organisms as lying on a ladder from the simple to the complex, with humans at the top (Lovejoy 1936). It was Lamarck’s genius to give it movement, which he did thanks to his thoroughly progressive evolutionary ideas.

In Germany an important thread of the progressivist doctrine was being prepared and woven into the fabric. It was in that part of the world above all that embryological studies were being advanced, and this led – particularly in the thinking of the Nature Philosophers or *Naturphilosophen* – to the famous or notorious analogy between the development of the individual and the development of the race or group. Just as the single organism went through stages from the very primitive to the very complex, so also the group went through stages from the very primitive to the very complex (Russell 1916, 1992).

This was not necessarily a physically evolutionary doctrine. But it certainly lent itself to an evolutionary interpretation – there is some likelihood that the poet Goethe inclined this way by the end of his life – and when in Britain evolutionary ideas started again to appear and to flourish, there were those who took up the embryological analogy with enthusiasm. This was particularly the case of the anonymous author (now known to be the Scottish publisher Robert Chambers) of the evolutionary tract, the *Vestiges of the Natural History of Creation*, which appeared in 1844. Right at the heart of Chambers’ evolutionism was the analogy between the development of the individual and that of the group. Indeed, the two were connected, because Chambers thought that evolution came about when an embryo fails to get born at the proper time and goes on developing in the womb.

It goes without saying that this is a thoroughly progressionist doctrine. Without argument Chambers thought that further development would take one up the ladder of life to ever greater complexity. It should also be noted that Chambers openly identified the progressiveness of life’s history with the progress that he thought he saw in early Victorian society.

The question whether the human race will ever advance far beyond its present position in intellect and morals, is one which has engaged much attention. Judging from the past, we cannot reasonably doubt that great advances are yet to be made; but if the principle of development be admitted, these are certain, whatever may be the space of time required for their realization. A progression resembling development may be traced in human nature, both in the individual and in large groups of men . . . Now all of this is in conformity with what we have seen of the progress of organic creation. It seems

but the minute hand of a watch, of which the hour hand is the transition from species to species. Knowing what we do of that latter transition, the possibility of a decided and general retrogression of the highest species towards a meaner type is scarce admissible, but a forward movement seems anything but unlikely. (Chambers 1846: 400–2)

Charles Darwin's Theory of Evolution

Let us move now to Charles Darwin, the author of *On the Origin of Species*, first published in 1859. Rightfully, Darwin is known as the father of evolutionary theory, but it is important to understand exactly what this means. He was obviously not the first evolutionist, meaning the first to think that all organisms came through a process of natural development from primitive beginnings. What he did however, by amassing an amazing array of biological facts – behavior, paleontology, biogeography, systematics, morphology, embryology – was make the idea of evolution not simply plausible but (in the opinion of his contemporaries and of thinking people ever since) commonsensical (Ruse 1999).

What he also did was propose a mechanism – natural selection. More organisms are born than can possibly survive and reproduce. This leads to (what the English clergyman Robert Malthus had called) a struggle for existence. Those that do survive and reproduce will on average be able to do so because of their superior features – their adaptations – and eventually this process akin to the breeders' technique of artificial selection will lead to full-blown change. It is most important to note that for Darwin – and apart from the use of Malthus' ideas (intended to show how God gets people up and running and working for a living), this is the major reason why I speak of Darwinism specifically as being an offspring of Christianity – evolutionary change is change of a special kind. It is change that creates and cherishes those features that help in the struggle – eyes, teeth, hands, leaves, bark, fins, scales, feathers, and all else. These are the contrivances, the adaptations, that natural theologians like Archdeacon Paley (1802) had seized on as proof of the existence of God. The eye is like a telescope. Telescopes have telescope makers. Hence eyes must have eye makers, the Great Optician in the Sky (Ruse 2003).

But where stands progress in all of this? Socially and culturally Darwin was an enthusiastic progressionist. Why would he not be? His grandfather (and that of his wife) was Josiah Wedgwood who made a fortune in the pottery trade. Charles Darwin was doing very nicely out of the industrial revolution. Biology however caused him more problems. He was very much against *Naturphilosophie* – it had the stench of German Idealism that was not popular in the circles in which he mixed (at Cambridge and later in London). He had no time for the inevitability of the individual–group analogy, where the progress of the group was seen as a kind of momentum akin the momentum that forms the complete adult individual. Yet, Darwin believed in biological progress and wanted to support it. In the *Origin* he wrote:

As all the living forms of life are the lineal descendants of those which lived long before the Silurian epoch, we may feel certain that the ordinary succession by generation has never once been broken, and that no cataclysm has desolated the whole world. Hence

we may look with some confidence to a secure future of equally inappreciable length. And as natural selection works solely by and for the good of each being, all corporeal and mental endowments will tend to progress towards perfection. (p. 489)

In the *Descent of Man* he was even more open:

In the class of mammals the steps are not difficult to conceive which led from the ancient Monotremata to the ancient marsupials; and from these to the early progenitors of the placental mammals. We may thus ascend to the Lemuridae; and the interval is not very wide from these to the Simiadae. The Simiadae then branched off into two great stems, the New World and Old World monkeys; and from the latter, at a remote period, Man, the wonder and glory of the Universe, proceeded. (1, 213)

The trouble is that natural selection is a relativistic process. What succeeds in one situation may not succeed in another situation. Take size. As Rosemary and Peter Grant (1989) have shown of the finches on the Galapagos Archipelago, if food supplies are abundant then small-beaked finches do well or better. If food supplies are scarce, then it is better to have a big beak. Only then can you crack the big nuts that are available. How then can one talk of progress? This demands some kind of scale and value coming as one goes up it. How in particular can one show that humans came out top?

I think the task is impossible. After Darwin, the inevitable direction that was accepted by the earlier evolutionists – that direction that is an inheritance from the Christianity that they were trying to replace – was forever gone. However, this is my opinion, and not one shared by the historical figures. Darwin, who as we shall see was just about the only person who really understood the nature of selection and appreciated its implications, worried about the problem of progress – he worried about the problem in ways that no one else seems to have done – but Darwin was no less eager for a directed world-history than anyone else. To solve his difficulty, he turned to what today is known as an “arms race” metaphor. There is competition and consequently improvement – the prey gets faster, the predator gets faster. Taken overall, this leads to absolute improvement with humans coming out top.

If we take as the standard of high organisation, the amount of differentiation and specialisation of the several organs in each being when adult (and this will include the advancement of the brain for intellectual purposes), natural selection clearly leads towards this standard: for all physiologists admit that the specialisation of organs, inasmuch as in this state they perform their functions better, is an advantage to each being; and hence the accumulation of variations tending towards specialisation is within the scope of natural selection. (Darwin 1959: 222; this was added in the third edition of 1861)

After Darwin

This was Darwin’s answer and in a moment we shall see that it sits well with the thinking of many today. At the time however it was ignored, not because in itself it

failed to convince, but because (as is well known) although Darwin's arguments for evolution were a success, his case for natural selection was much less so. Today – indeed since the 1930s – natural selection is considered the key mechanism of evolutionary change. Back at the time of the *Origin*, most people – including key supporters like Thomas Henry Huxley – were much less impressed. The *Naturphilosoph* analogy – now thoroughly evolutionary – ruled the day. Leading the charge was the German biologist Ernst Haeckel whose “biogenetic” law, claims that ontogeny (the biological development of a single organism) recapitulates phylogeny (the evolution of a species). As we see progressive development in the individual so we see progressive development in the group (Ruse 1996).

In Britain, this kind of thinking was taken in as part of an overall progressivist foundational model to history, in biology, in culture, and in all else. The English general man of letters and science, Herbert Spencer – far more influential than Darwin in many respects – made this very clear. He saw organic evolution as being but one facet of the overall, upwards progress that characterizes the whole world process: from the undifferentiated to the differentiated, or to use words that he made nigh ubiquitous, from the homogeneous to the heterogeneous:

Now we propose in the first place to show, that this law of organic progress is the law of all progress. Whether it be in the development of the Earth, in the development of Life upon its surface, in the development of Society, of Government, of Manufactures, of Commerce, of Language, Literature, Science, Art, this same evolution of the simple into the complex, through successive differentiations, hold throughout. From the earliest traceable cosmical changes down to the latest results of civilization, we shall find that the transformation of the homogeneous into the heterogeneous, is that in which Progress essentially consists. (Spencer 1857: 2–3)

Everything obeys this law. Clearly, humans are more heterogeneous than other animals; Europeans are more heterogeneous than savages; and (only to be expected) the English language is more heterogeneous than all other languages.

What is fascinating is the way in which this philosophy of history laid itself open to modifications and adjustments as the need arose. By the end of the nineteenth century, many were wondering if progress is inevitable. With the rise of militarism, with the slums of the great cities, with the slumps of agriculture and industry, could it not be that the western civilization entered a period of decline? And would not this be reflected in evolution or rather devolution? Such indeed was the conclusion of many, including the novelist H. G. Wells, a sometime biology student of Thomas Henry Huxley. In the *Time Machine*, the traveler into the future learns unambiguously how the human race is set to descend ever lower. *Homo sapiens* has evolved into two separate species: the Eloi who live above ground, and the Morlocks who live beneath the ground. The Eloi are friendly, childlike creatures, who are virtually simpletons.

A queer thing I soon discovered about my little hosts, and that was their lack of interest. They would come to me with eager cries of astonishment, like children, but like children, they would soon stop examining me, and wander away after some other toy. The dinner and my conversational beginnings ended, I noted for the first time that almost all those who had surrounded me at first were gone. (Wells 1895: 70)

Then we learn that the Eloi are kept as cattle by their subterranean neighbours, the Morlocks. These latter do the work and provide the food for those above ground. But just as the Eloi are in a very fundamental sense “degenerates,” so also are the Morlocks. “I lit a match, and, looking down, I saw a small, white moving creature, with large bright eyes which regarded me steadfastly as it retreated. It made me shudder” (p. 101). Continuing: “I felt a peculiar shrinking from those pallid bodies. They were just the half-bleached color of the worms and things one sees preserved in spirit in a zoological museum. And they were filthy cold to the touch” (p. 111). And yet, the Morlocks, no less than Eloi, were descended from us.

I do not know how long I sat peering down the well. It was not for some time that I could succeed in persuading myself that the thing I had seen was human. But, gradually, the truth dawned on me: that Man had not remained one species, but had differentiated into two distinct animals: that my graceful children of the Upper World were not the sole descendants of our generation, but that this bleached, obscene, nocturnal Thing, which had flashed before me, was also heir to all the ages. (p. 101)

The Twentieth Century

Move now into and rapidly through the twentieth century. The big event in evolution’s history, of course, was the coming of genetics – first Mendelian and then molecular (Provine 1971; Bowler 1984). Many think that this too must have spelt the doom of progress. After all, the raw building blocks of evolution, the mutations, are now seen clearly to be random – not random in the sense of uncaused, or even random in the sense of unquantifiable – although one cannot tell when a particular mutation is coming over a group, one can find what proportion of genes will mutate – but random in the sense of not occurring to need. If say a new predator comes, there is no guarantee that a mutation will occur giving a new adaptive camouflage protection, or any like thing. Extinction may be on the horizon.

Surely then there can be no hope of upwards rise? Even if a mutation proves adaptive, it may be regressive in some sense. Suppose a plague kills off the prey of an animal predator. It may pay the predator to turn to other foodstuffs – vegetable matter – in which case that very expensive organ the large brain is redundant and a burden, and selection could put pressure on shrinkage. As the paleontologist Jack Sepkoski once said memorably: “I see intelligence as just one of a variety of adaptations among tetrapods for survival. Running fast in a herd while being as dumb as shit, I think, is a very good adaptation for survival” (Ruse 1996: 486).

Nevertheless, evolutionists – like scientists generally – tend to have a progressive worldview, including biological, social and cultural progressivism, though (compared to earlier evolutionists) they are not overt about it in their professional writings. The non-directedness of mutation is brushed aside as is the relativity of natural selection. Edward O. Wilson, the Harvard entomologist and sociobiologist is typical in writing:

the overall average across the history of life has moved from the simple and few to the more complex and numerous. During the past billion years, animals as a whole evolved

upward in body size, feeding and defensive techniques, brain and behavioral complexity, social organization, and precision of environmental control – in each case farther from the nonliving state than their simpler antecedents did. (Wilson 1992: 187)

He adds: “Progress, then, is a property of the evolution of life as a whole by almost any conceivable intuitive standard, including the acquisition of goals and intentions in the behavior of animals.”

Causally, the arms-race mechanism continues to ride high. Richard Dawkins (1986) is typical, suggesting that not only do such races produce advanced or better adaptations, but that this leads to progress. Eventually, it leads to humans, simply because when all is said and done, brains are the very best of all adaptations. The history of arms races in the twentieth century shows that, increasingly, military strategy depends less on sheer brute force and more on sophisticated weaponry using high-tech electronic equipment. These are analogous to the development of organisms’ on-board computers, better known as brains. Referring to Harry Jerison’s (1973) notion of an Encephalization Quotient (brain to body ratio), this being a kind of universal animal IQ, that works from brain size and subtracts the gray matter simply needed to get the body functioning, Dawkins argues that humans come way out on top: “The fact that humans have an EQ of 7 and hippos an EQ of 0.3 may not literally mean that humans are 23 times as clever as hippos!” But, he concludes, it does tell us “something.”

Others suggest different methods of getting upwards rise. The Cambridge paleontologist Simon Conway Morris (2003) favors jumping up to ever more demanding ecological niches. But is this a universal conclusion? Is biological progress a common hope of evolutionists? Do not some stand against the tide? Yes, some do, notably the late Stephen Jay Gould. He spoke of progress as “a noxious, culturally embedded, untestable, nonoperational, intractable idea that must be replaced if we wish to understand the patterns of history” (Gould 1988). It is a delusion engendered by our refusal to accept our insignificance when faced with the immensity of time (Gould 1996). Making reference to the asteroid that hit the earth some 65 million years ago and that (we think) wiped out the dinosaurs, Gould wrote:

Since dinosaurs were not moving toward markedly larger brains, and since such a prospect may lie outside the capabilities of reptilian design . . . we must assume that consciousness would not have evolved on our planet if a cosmic catastrophe had not claimed the dinosaurs as victims. In an entirely literal sense, we owe our existence, as large and reasoning mammals, to our lucky stars. (Gould 1989: 318)

Even Gould however allowed some kind of upwards direction and thought that if we search the universe we will find the emergence of human-like intelligence. This comes about through a kind of random walk. Just as the drunk walking along a sidewalk bounded on one side by a wall eventually ends in the gutter, so life starting simple eventually ends complex. An idea in fact that brings us right back to Charles Darwin. In a private notebook, in 1839 – just after he had discovered natural selection – he wrote:

The enormous number of animals in the world depends, of their varied structure & complexity. – hence as the forms became complicated, they opened fresh, means of adding to their complexity. – but yet there is no NECESSARY tendency in the simple animals to

become complicated although all perhaps will have done so from the new relations caused by the advancing complexity of others. (E 95)

Note the adamant denial that there is any necessity to the directions of nature – the *Naturphilosophen* are the target here. Then Darwin added:

The Geologico-geographico changes must tend sometimes to augment & sometimes to simplify structures:— Without enormous complexity, it is impossible to cover whole surface of world with life. – for otherwise a frost if killing the vegetable in one quarter of the world would kill all of the one herbivorous. & its one carnivorous devourer.; – it is quite clear that a large part of the complexity of structure is adaptation. though perhaps difference between jaguar & tiger may not be so. – Considering the Kingdom of nature as it now is, it would not be possible to simplify the organization of the different beings, (all fishes to the state of the Ammonoites) Crustacea to—? &c) without reducing the number of living beings – but there is the strongest possible to increase them, hence the degree of development is either stationary or more probably increases. (E 96–7)

Growing Up

We draw to a conclusion. Evolution was part of an alternative philosophy of history to that of the Christian. An alternative, but nevertheless something that took much from the Christian – including the need to offer a solution to the question of origins. An alternative that likewise gave humankind a key place in history – past, present, and future. Darwinism seems to challenge this alternative, and truly one can say that conceptually it does. It has not stopped people trying to read progress out of the evolutionary process, but it does mean that they have to work to do so, and not everyone finds the solutions entirely satisfactory. You may think that large brains are a good thing. I certainly do. In fact, I think you would be pretty odd if you did not think large brains are a good thing. But it is not necessarily something read out of the evolutionary process. It is, rather, something that we read into it.

What of the embryological analogy? With the coming in the twentieth century of full-blooded Darwinism – a Darwinism using Mendelian genetics and giving a major role to natural selection – embryology started to fade from the scene. Simply as a science it was somewhat excluded – the tendency was to go straight from the genes to the mature morphology, and take all in between as given. As crucially, as Darwin saw and emphasized, when you have natural selection, although you can see why the embryological development often parallels group development (early organisms are like the embryos of today and then new stages as added needed), there is no necessity to the biogenetic law (and many exceptions) and in any case the kind of momentum leading to progress is negated. So that kind of progressivism is gone.

Today, embryology – evolutionary development or “evo devo” (as it is often known) – has risen and become now one of the most exciting and forward looking of the branches of evolutionary study (Carroll 2005; Ruse 2003). But now it is joined by natural selection – along with sociobiology, paleontology, and so forth – and although it gives crucial insights into the history of change, it is no longer trying to offer the foundation

for the change. So evo devo is not in any sense trying to refute or deny the way that conventional Darwinism views history.

Ultimately, to use a word that has not been used before, it is all a matter of “teleology” or end-directed thinking. Christianity offers a teleological view of history – from the beginning we are working towards the end, to the Day of Judgment and to the possibility of eternal salvation. Everything, especially the Fall, the death on the Cross, the Resurrection, and justification by faith, is to be understood in those terms. Evolution tried to offer a secular alternative. Progress led to an end in which humans were the major players. Embryology became a crucial part of this story. Darwinians wanted to keep this vision, but with the coming of natural selection (and Mendelian genetics) truly it seems that the inevitability of the end, the absolute teleology, is no more. You can try to regain something of this, but it is doubtful that you can ever really go “home.”

This really is what you might have expected all along. You are replacing a spiritual view of the world with a secular one. You are replacing one with meaning by one without meaning. You can try to keep as much as you can of the old picture, but you should not be surprised if in the end you lose things that were considered absolutely crucial. That is what the move from the sacred to the secular is all about. Some of us call it a loss. Others of us call it “growing up.”

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The Philosophy of Geology

ROB INKPEN

Introduction

Earth science studies the operation of the natural environment such as geology and physical geography, often focusing on the abiotic (non-alive), but study can be extended to include the biotic (life) embracing human interaction with the environment. Such endeavors have long been a focus of study and debate within science. From von Humboldt's survey of the natural world in *Cosmos* through Darwin's epic voyage of collection and rural musings, to contemporary contemplative analysis of Gaia by Lovelock, the study of the systems that make up the earth has involved a whole host of different disciplines, philosophies, and working methods. earth scientists deal with uniqueness. Singular events and entities in space and time form their basic diet of study. Identifying unique events, classifying them and trying to explain them is central to understanding the philosophies of earth sciences.

Assuming that a single overarching philosophy underlies these diverse activities is both optimistic and arrogant. Yet it has often been assumed that labelling these pursuits as science somehow assigns the "scientific method" as their *modus operandi*.

Great philosophical traditions such as logical positivism and critical rationalism attempt to serve as the basis for how practitioners should behave. Whether the practitioners themselves are aware of this requirement is another matter. Often researchers prefer to leave the "armchair" philosophizing to others, whilst they get on with the "real" work of investigating the natural environment. Practice, getting out there in the environment, "walking the section" in geology, is what researchers do. Their studies are guided by tradition and methods that seem clear and obvious, if also tacit, the philosophical basis of these is of little concern, if such practices provide the information to answer the questions they pose.

Such cynicism may be related to the view that earth science and their objects of study as a practical subject, one in which the concern is to answer specific and difficult question based on incontrovertible field evidence. There has also been a feeling of intellectual inferiority associated with the view that earth sciences are a derivative subject, a set of disciplines based on "pure" or "hard" sciences such as physics and chemistry, resulting in "physics envy" (Massey 1999). Earth sciences, in this scenario, are still relatively immature, slowly progressing towards the status of "law-making" sciences like physics.

The “hard” sciences, particularly a caricature of physics, dominated philosophic thinking about the nature of science and its subject matter. A world where readily identifiable entities interact in a regular manner forms the basis of this reality. Careful and systematic observation of these entities provides the information needed to develop (or reject) theories about reality. Only when “laws” can be stated, can a subject claim intellectual maturity and a place at the high table of understanding. Science should focus then on the search for universal laws or rules that provide a basis for predicting how entities will behave. Deriving general rules from the morass of the physical environment has been an implicit objective of earth sciences from their inception.

Earth science does not deny the importance of physics and chemistry; indeed it could not be practiced unless it accepted their findings. Rather, earth science applies to the “real” world outside the laboratory, to situations where control and repetition is not possible, to situations where the observer is a late-comer and not the instigator of phenomenon.

Identifying Unique Events

Physical reality has no natural divisions. Breaking reality up into discrete units for study is a matter of human choice, albeit guided by an experienced dialogue with reality (Wright 1958; van Bemmelen 1961; Demeritt and Dyer 2002). Isolating an event from a spatial and temporal continuum immediately highlights the explanatory framework that a researcher has in mind. A theory always guides the division of reality. Theories define what is acceptable as a discrete entity and what is not. Researchers do not define individual events as isolated unique entities. Researchers are interested in events because they believe they can use them to extract information about the operation of reality. Token events are never classed in the singular; they are always related to a template, a type. Classification as a token of a type immediately ascribes the event with a series of properties that are worth studying, measuring, and which determine how it will behave. Assignment to a kind is shorthand for explanation.

Kinds are not natural givens. Hacking (1991) views kinds as being defined and becoming important when their properties are of interest to the researchers who want to know what entities do and what can be done to them. Only through practice and experience can entities, and by extension kinds, be identified and built into explanation. Goodman (1978) holds a similar view, defining relevant kinds as those relevant for some purpose by the user. Although relevant kinds may match “real” kinds, they may divide reality up in the way it actually is, researchers can never know this as they can only understand reality through their division of it into relevant kinds. Kinds, in other words, emerge through our study of reality, through our dialogue with it. This makes theory a key determinant of what past events, the entities of study in earth sciences, are.

There is an assumption that the physical reality studied is the same whatever techniques are used and whoever studies it (Rescher 2000; Inkpen 2005). Goodman (1960, 1978) notes that researchers “make their own worlds,” following the purpose and objectives of their research. Reality may be a physical whole, but researchers intervene in this reality in different ways and by so doing create different dialogues with

reality. This means that different researchers may view past events, history, differently, both in terms of identifying the event and in terms of gathering evidence of the event. Analyzing past climatic events is not done through a single set of methods or a single piece of evidence. Climatic events, such as the “Little Ice Age” can be studied via intentional human-made evidence such as paintings as well as by analysis of variation in tree-ring dimensions. Although apparently studying the same event, the two types of study have different dialogues with reality. Using historical documents requires tapping into a network of information inscribed by humans and open to vagaries of subjective representation. Evidence will be partial and spatially and temporally discrete and discontinuous. Only specific types of questions can be asked based on the nature of the evidence and its location. Dendrochronology, the science of dating events according to tree rings, has similar spatial and temporal limitations, but additionally has a host of issues concerning sampling strategies and representativeness to consider as well. The questions that can be asked of reality are different because of the nature of the method.

It is common that the locations of collection and analysis of evidence are distinct. Sediments collected in the field may be examined for properties such as color, but detailed analysis of their constituents takes place back in the laboratory. Collection of these samples requires the researcher to exercise their judgment concerning what parts of the environment will provide a sample that contains information about an event, relevant evidence. Such selection decisions will be informed by experience, but also by the theoretical framework of which a description of the type of event is a part. Reconstructing descriptions of events relies on the researcher believing the information gathered can infer accurately a representation of past events. Sampling in the field usually follows a set of protocols or guidelines that ensure that the samples obtained can be contextualized and reach the laboratory “uncontaminated.” By “uncontaminated” the researcher usually means that the samples contain information only of relevance about the event of interest. Additional information, “noise” in information theory, such as chemicals from the hands of the researcher or mixing of sedimentary layers by poor handling casts doubt on the source of the information in the evidence. Extracting the relevant information usually requires processing the sample often to eliminate all substances other than the one of interest. At each stage, the researcher is defining or constructing the environment within which the evidential sample exists. It is the researcher who, based on theory, defines and practices the protocols that produce the final data. In this sense the researcher constructs the event and the data used to infer a characterization of the event.

Representing Events

Past events are not directly observable. Earth sciences have to deal with the traces of past events instead. Events, like personalities and individuals, are complicated and multifaceted entities. The traces they leave do not allow the researchers to reconstruct the exact event in all its glorious and intricate individuality. Instead, the traces permit researchers to classify events in terms of specific properties that the traces indicate they possessed. Presence of an iridium layer of varying thickness between Cretaceous

and Tertiary sediments indicates an event, but recognition of it as a trace of an event required the evidence to be interpreted. Iridium by itself signifies nothing, it is the presence of iridium *and* its role as evidence relative to a theory that provides the key to interpret it as evidence of a specific type of past event. The iridium layer represents the impact of a large meteorite, the only potential source of such a large trace. The distribution of the layer and its variation in thickness indicates the impact location of the meteorite. It is interpreting the layer within the context of theories concerning its source and theories concerning its distribution, that the layer takes on explanatory significance. Viewing traces as signs of past events and all the implications of interpretation of signs that this brings is an important aspect to the practice of earth sciences (Baker 1999).

Only specific properties of an event are preserved as traces and of these only a smaller subset are viewed as relevant within a specific theory and are capable of analysis. No analysis is done outside of some sort of theoretical construct. This guides the researcher to the properties of an event about which information may be preserved and to those properties that distinguish an event as unique or the member of a specific kind. Theory does not evolve in isolation from a research program. What scientists consider a the defining properties of an event can alter as the types of properties that can be measured alter. Iridium would not be measured and used as a one of the distinguishing features of the K–T boundary (the geological boundary between the Cretaceous and Tertiary periods) unless uncontaminated samples could be collected and analyzed with sufficient precision to identify variation in this property of sediment.

This view of theory driven observations or evidence is highlighted by Rhoads and Thorn (1996) in their discussion of the model theoretic view of reality (abbreviated to MTV: figure 28.1). In this model of the scientific method, evidence and theory are interwoven through data models that direct the researcher as to the type of information to collect and the appropriate modes of analysis. Using ideas from van Fraassen (1980,

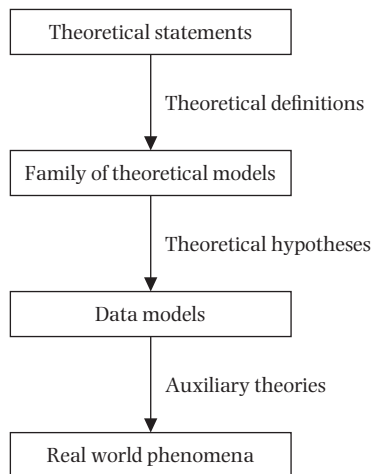


Figure 28.1 Model Theoretic View (MTV)
Source: Rhoads and Thorn (1996)

1987) and Giere (1988) they suggest that measurement of information about the environment, past or present, cannot be separated from the theory that guides instrument design, selection, and use. Any theory has associated with it a class of abstract structures: its models. Such models form a closely related family and are derived from the postulates and laws proposed by the theory. These models are linked to reality, unlike the theory, by the development of hypotheses. Hypotheses are the empirically observable subset of theoretical models. Data or evidence are defined and constructed by the theory through its hypotheses. Data are not, therefore, independent entities waiting for discovery. Past events are not entities waiting for their "true" nature to be uncovered; they are constructs within this view as well.

The use of data models means that it is not the past event itself that is being analyzed, but rather a theoretically laden representation of it. In other words, surrogates of varying quality are being used to represent specific, unique events. Analysis of past events is, therefore, analysis of these surrogates. How such surrogates are analyzed is part of the tradition or practice of a discipline. A diatom, a class of algae, for example, could form part of an analysis of long-term acidification of lakes (Battarbee et al. 1985). On its own it is a piece of information about the past. Classification of it as a specific species narrows down the type of environment it could have existed in, assuming that the species in the past had the same environmental tolerances as its modern counterpart. This identification relies on isolating the diatom from the environment, yet retaining information about the location of extraction for interpretation. This means that a whole raft of procedures and protocols has to be followed to ensure that this information is appropriately recorded. The sediment core needs to be extracted from a location judged to be appropriate for the purposes of reconstruction, in this case supposedly undisturbed lakes in southern Scotland. The core needs to be extracted so as not to disturb the vertical sequence of layering of the sediment. Subsequent treatment in the laboratory needs to preserve either the sequence or information about the sequence. Once a sub-sample of the sequence is selected, then another layer of procedures is involved in extracting, intact, the diatom for more detailed analysis. A single diatom may be identified under either a light microscope or SEM (scanning electron microscopy) but this requires both the equipment and a skilled operator. In other words, behind the identification and classification of a single diatom there are varying networks of relationships that create both the material equipment and training for this purpose.

Although the diatom has been collected according to a host of theories in order to reconstruct the past, it is not the past of the diatom that is the object of study, but the general environment that it represents that forms the focus of study. The diatom is the surrogate for that environment. Singularly, the diatom is not sufficient to represent the past. Instead, the information provided from that single diatom needs to be amalgamated with other information, with other diatoms in the sample. The single diatom is just one piece of information, its value lies in its relations to the other parts of the sediment core. If it does represent the past environment then there should be other diatoms of the same type in the sub-sample. Counting the number of each species in the sub-sample and then producing a species chart (a graph of the frequency of each species found within the core or sample) is the standard practice. This puts the information value of a single diatom into a context or rather into a network of information. It is this network that is represents the sub-sample, via the species graph, and this is how

the past environment is represented. The whole species graph itself becomes the entity that is used to characterize the past environment rather than the single diatoms from which it is constructed. Likewise, it is the amalgamated data of diatom species that are compared statistically to assess if one past environment differs from another past environment.

Unique Events and Explanation in Scientific Historiography

Whether past unique events or past environments are being studied, the aim with each is explanation. Earth science is concerned with understanding why was the past as it was. Trying to develop or “shoe-horn” explanations from earth sciences into an appropriate explanatory framework is not an easy task. Traditional philosophies of science focus on verification and falsification procedures that are more suited to the controlled environs of physics laboratories. Figure 28.2 illustrates the “classic” Popperian method of deductive reasoning coupled to hypotheses that assess the validity of the theory in the “real” world. Explanation becomes a process of elimination. False theories are disproved until a single, unfalsifiable theory is left. Unfortunately, this luxury is one that earth sciences can hardly ever achieve in practice and one that is often inappropriate for the questions being asked. There is a concern with explaining

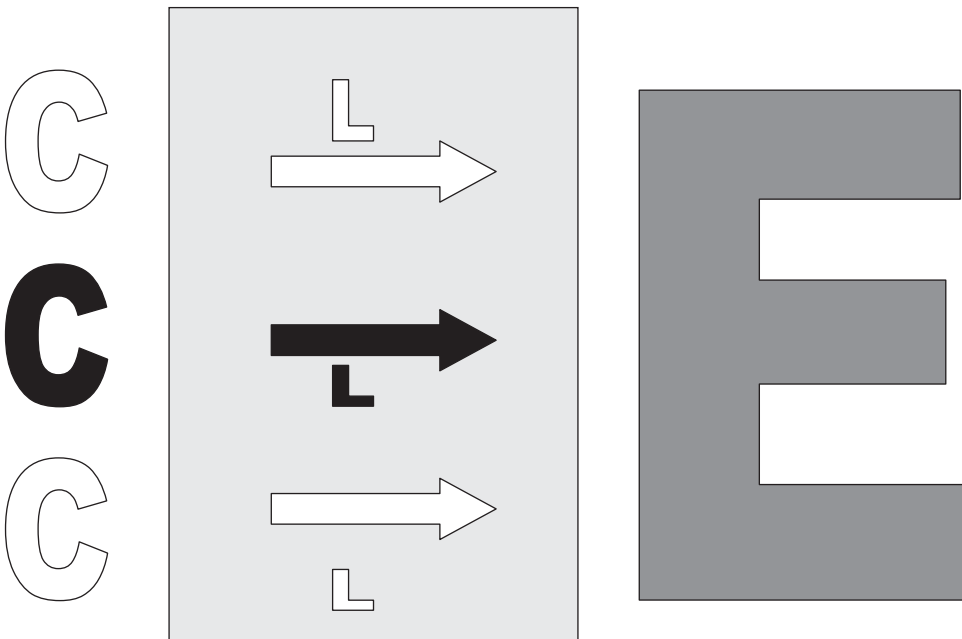


Figure 28.2 Popperian reasoning

Note: Deductive based argument from C, cause or initial state of affairs via application of laws, L to the effect, E. Applying a specific law to each initial state of affairs an outcome can be deduced. Testing (the shading box) these outcomes against reality eliminates all but one of the causes.

Source: Inkpen (2005)

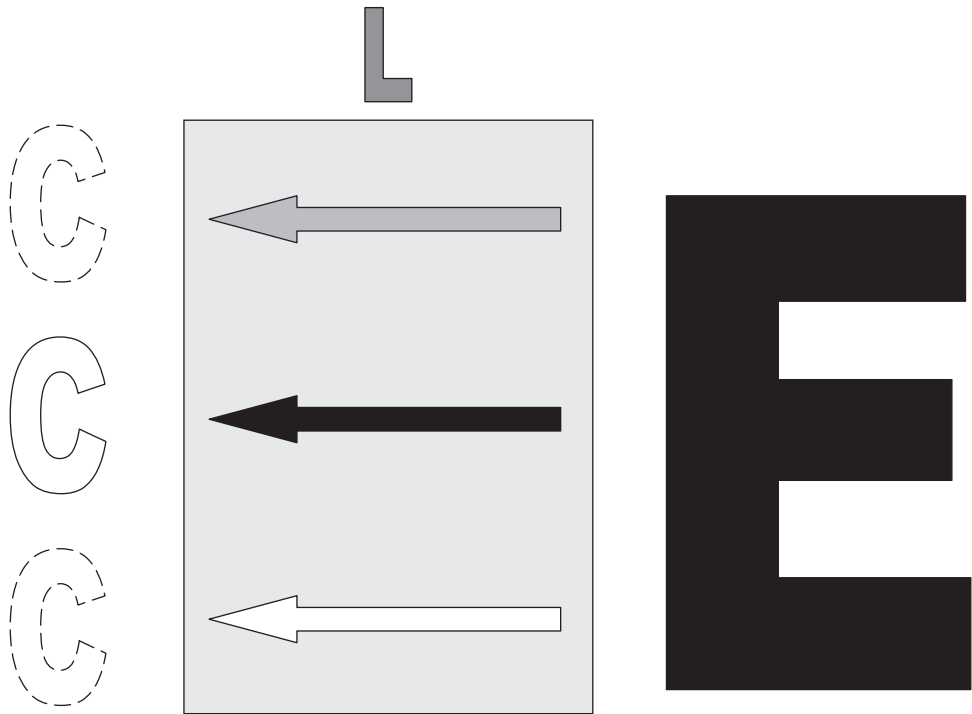


Figure 28.3 Abduction

Note: Effect (E) is known, law (L) is less certain and many may have to be assessed. Argument works from effect to cause. Law is applied and different plausible links are made back to cause, but with differing degrees of confidence (different shadings). Testing (shaded box) can eliminate some of causes, but not necessarily all.

Source: Inkpen (2005)

specific past and present events rather than with generalities, although the explanation of the events can involve and result in generalities being developed in relation to events of that type.

It may be more appropriate to view earth sciences as using the abductive approach or even inference to the best explanation (Lipton 2004; Kleinhans et al. 2005) as its working philosophy (Figure 28.3). Development of this alternative method has its roots in the view that earth sciences are historiographic sciences (Gilbert 1896; Chamberlain 1890; Johnson 1933; Frodeman 1995), often with an assumed inferior status to “proper” science (Anderson 1996). The generalities, the “laws” of proper science will be used in constructing an explanation of events, but they will not be *the* explanation itself. Instead, the “laws” form part of an explanatory framework, a kind of narrative structure that earth sciences deal in.

Scientific historiography deals with the asymmetry of localized events (Cleland 2001). An event, such as the impact of a meteorite, can produce a whole range of effects, but of these only a few are required to identify, unequivocally that the event actually occurred. Any one of these effects cannot, however, on its own, be used to infer that

a specific event occurred. A single effect, just like a single diatom, can plausibly be linked to many and varied possible causes and events. Dominance by a specific species of diatom may be more narrowly linked with only a limited set of environmental conditions. Studies tend to focus on these traces of the past that, when considered together, indicate a specific past event is likely to have occurred. Such entities act as unifying agents, integrating within them information about the past, even if comparisons with the present are required to extract this information (Turner 2004). Although there may be “smoking gun” evidence (Cleland 2001), a single piece of evidence that can be produced by one and only one type of event, it is more likely that there will be underdetermination in earth sciences (Kleinmans et al. 2005). There are two types of underdetermination, strong and weak. Strong underdetermination refers to a situation where the choice between competing theories is impossible no matter how much evidence is available. Weak underdetermination refers to a situation where there is insufficient evidence available to make a choice between competing theories. Kleinmans et al. (2005) suggest that weak underdetermination is common in the earth sciences.

Where theory choice is never going to be definitive then different strategies need to be employed to develop robust explanations, which do not contradict the evidence that is available (Inkpen 2005). Triangulating information, using different methods and sources of information or evidence to assess a theory is vital in the earth sciences. Where there is convergence of evidence, where the same explanation is implied, there is increased confidence in the explanation. Where there is divergence, then either the explanation is at fault or one or more of the sources of information is in error or requires reinterpretation. Alternatively, divergence may point to different theories or data models illuminating different aspects of a phenomena or event. This implies that rather than a single explanation for an event there may be different explanations for different aspects of an event (Inkpen 2005).

Explanation of events requires that they be placed within a context. Simpson (1963) suggested that an explanation should consider the ahistorical or immanent and the configurational aspects of an event. (Configurational could be interpreted as the specific spatial and temporal context within which the event occurred.) The immanent refers to the “laws” of physics and chemistry that explain an event. A landslide, for example, could be explained and modeled using concepts from physics. The configurational refers to the series of states that have uniquely occurred through the interaction of immanent processes with historical circumstance: the context of processes. In other words, the configurational refers to the particular history of an area and how that history has altered that area. This provides the context for the event. Explanation of events cannot be reduced to the immanent. Immanent processes may explain in general why a landslide happens, but it does not explain why it happens at a particular site at a particular time. The configurational explains this aspect of an event. This contextual based explanation has parallels with historiographic narratives that Hull (1989) argues cannot be explained by merely reducing them to a generalization. Such narratives should be explained by integrating them into an organized whole, by consideration of their context and their formative processes.

Consideration of both vertical and horizontal forms of explanation, as outlined in critical realism, means that hierarchical explanatory frameworks are important in earth

sciences. Such explanatory frameworks have a pedigree in biology and ecology (Allen and Starr 1982; O'Neill et al. 1980; Salthe 1985; de Boer 1992) and act as a guide for what can be regarded as an appropriate explanation. Reality is differentiated into distinct strata or levels that interact and influence each other. The entities at an upper level are constrained by processes and entities at a lower level. Landslides, for example, are constrained in their behavior by the material of which they are composed and by the physical relations between their components. In other words, landslides have properties and behavior that are supervenient upon the "laws" of physics and chemistry, but explaining landslide behavior is not possible by reducing landslides to these "laws," i.e., upper levels are not explained by the lower levels. At the level of study there are entities that interact, establish relations that influence their behavior, but which could not be predicted from the entities and relations that exist at the lower levels. Indeed, the upper levels can influence the lower levels by establishing conditions or a context within which the processes found at these levels can operate at a faster or slower rate. Events cannot be explained without reference to a specific level of study. Establishing this level of study establishes the entities and relations that will be seen as acceptable or appropriate within any explanatory framework.

Illustration of Evidence and Events

Vine and Matthews' (1963) article on magnetic strips on the floor of the Atlantic Ocean is often viewed as the key paper in the acceptance of fledgeling concept of plate tectonics. The paper illustrates a number of the points made above. It begins with a generalization of the patterns found in typical profiles of bathymetry (underwater topography or relief) and magnetic field anomaly (variations in the strength of the magnetic field) found crossing the north Atlantic and north-west Indian oceans. The data are distilled and represented as graphs highlighting that it is only specific properties of the events that produced this pattern that will be analyzed in the paper. The authors then use the data to dismiss one potential explanation for the pattern, the association of the long-period magnetic anomalies with bathymetry explained by susceptibility contrasts and crustal configurations. The authors then use data from a recent survey by HMS *Owen* of the Carlsberg ridge to undertake a more detailed analysis of the anomalies. This data was not originally collected for them, so the form of the data is transferable and capable of interpretation within a number of theoretical frameworks. This implies that the theories are closely related or else the data model used to generate the information would not be flexible enough to permit such exchange of data.

In this classic paper, the authors develop the idea of sea floor spreading by creating an entity that, by its novelty, needs a new and novel explanation. The authors use the data and a computer program to generate an intensity and direction for magnetization of two isolated volcano-like features and three profiles perpendicular to the ridge. The resulting graph illustrates their next point, that whole blocks of the ocean floor along the profiles appear to have been reversely magnetized. They hypothesize that the sea floor is divided into 20-km blocks. This means that their graph can then be explained by the differences in behavior of discrete blocks of material. The authors have

created a new entity and an event in one, the 20-km reversely magnetized block, which now require explanation and further characterization. Using their new entity, they identify that about 50 percent of the oceanic crust in the survey is reversely magnetized and so requiring a major idea to explain it.

Rather than suggesting a totally new theory, the authors firmly locate the observations within a set of existing theories about sea floor spreading (Dietz 1961) and periodic magnetic reversals of the earth's magnetic field (Cox et al. 1963). The authors have not suddenly observed the magnetic strips in the data. They have used existing theories to collect and analyzed the data. Theories have guided how they see the information. The "classic" explanation of the stripped pattern is presented with the observed pattern resulting from the formation of the oceanic crust by convective up-currents at the ridge with the crust being magnetized during cooling. Alternate and symmetrical blocks of normal and reversed magnetized crust should be found either side of the ridge. The stripped patterning of ocean crust often reproduced in textbooks is not present in the paper. Instead, the authors use three graphs (figure 4 in the paper) to illustrate the alternating reverse magnetization from the Pacific, Atlantic, and for the Carlsberg ridge. Importantly, they note that figure 4 is not an "accurate" reproduction of the profiles. They state that the graphs, or rather the computations on which the graphs are based, show only the "essential" form of the anomalies. The authors are defining the nature of the kind of which the detailed events are members.

At the end of the article the authors acknowledge the help and data from various organizations and individuals. The web of relations that the authors had to activate to obtain the data and expertise to run the computer models illustrate that the context of the researchers is vital for developing this idea. The authors' ability to gather the information and their access to theories, such as sea floor spreading, that were still developing mean that their interpretation of the data was shaped by their social and professional context as well as by the nature of the information itself.

Conclusion

Unique events and entities are the main focus of study in the earth sciences. Explaining the unique and using the unique to derive generalities about phenomenon in the natural environment is difficult using philosophies of science such as logical positivism and critical rationalism that were inspired by physics and other sciences that operate with phenomena that can be isolated, controlled and made to repeat their behavior. The once only, and contextually bound nature of events in the earth sciences make an abductive mode of explanation more appropriate. Rarely, if ever, do earth scientists deal with past events as a whole. Instead they are restricted to traces of events, preserved echoes of the past, which require considerable effort to extract any useful information from. The type of traces that are useful depends upon the expectations of the researcher that are determined by the theories they work with and the manner in which they are able to interrogate reality. The events and entities any researcher studies are derived through their contextually bound dialogue with reality. This does not make the events any less real, but it does highlight that there are social influences even in seemingly "physical" events.

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Philosophy of Archaeology

BEN JEFFARES

Archaeology constructs accounts of the past based on physical data. Typically, when we think of archaeology's data, we think of the intentional products of human behavior; texts, tools, the remains of dwellings, and the surviving pieces of human material culture that fill our museums. However, the increasing sophistication of archaeological techniques means that the unintentional by-products of human activity are studied as well. For instance, archaeologists can use molecular biology to detect fatty acids on the inside of pottery, providing insights into its use. From various nuclear dating methods to the microscopic analysis of hundreds of tools, archaeological evidence gathering and analysis is an amalgam of scientific methods.

This heterogeneity raises an important problem: Some lines of inference from evidence are very secure, while other lines of evidence are tenuous and suggestive at best. Constructing an account of the past is an act of balancing different forms of inference from evidence, and sometimes adjudicating between them.

This balancing act can be complicated by the fact that archaeological data can become attenuated through time by processes of decay, or re-configured by human, non-human, or geological actions. These post-depositional processes can rearrange the relationships between various data points to create misleading patterns. Archaeologists have to be aware of such processes in order to determine meaningful information signal amongst accumulated post-depositional noise.

The Interpretive Dilemma

The data collecting should assist in constructing a picture of the past. Archaeologists find their data embedded in spatial and temporal contexts. When confronted with spatial configurations of data the archaeologist assumes that physical contiguity represents social continuity, and seeks to interpret those spatial patterns as representative of various features of human social life. In that respect, archaeology is a social science.

As a social science, archaeology engages in anthropology, economics, sociology, religious studies, politics and even interpretive humanities such as art history. Unsurprisingly, given this range of disciplines, archaeology inherits the philosophical problems of the social sciences. As the individuals who are the object of study can never

be observed or questioned, the inference from material remains to behavior is of course problematic, and much theorizing in archaeology can be traced to this problem. Archaeological debates also echo social science debates about methodological individualism, the interpretation of other agents, and functionalism in the social sciences.

Archaeology also has another dimension, a temporal one. Data through time can show change, displacement, increased inter-group contact, settlement, shifts in subsistence technologies, and so forth.

This temporal dimension comes with its own interpretive tensions. Over smaller timescales, changes result from individual initiative, and archaeologists attempt to construct a historiography. However, over longer timeframes, archaeologists frequently look to demographic and environmental causes. Over still larger scales, particularly the archaeology of pre-human evolution, natural selection can play a role.

Should we interpret the end of a civilization as a historical group falling victim to demographic and environmental forces, or as the result of the interplay of active agents constructing their own fate? Archaeology inherits all the debates about human history and historiography found in the rest of this volume, but at a temporal scale that can vary from millennia, to a few hundred years.

To provide context for these dilemmas, we can briefly outline a hypothetical example. Archaeologists, when confronted with a site may systematically uncover multiple objects; pierced shells, bones, what appear to be stone tools, charcoal, changes in pollen types through time, and so on. They may examine these materials with a range of empirical techniques, which may or may not give conflicting results. Should the carbon dating be taken seriously? Or do patterns of change in tool types convey a better chronology? Archaeologist must also decide if the relationships between pieces of evidence are meaningful. Has the pollen been washed through the sediment by rainwater? Has the charcoal for dating been disrupted by later burials?

The archaeologist also has a range of theoretical questions that they can pose. Is this site representative of a regional pattern? Is it representative of a hunter-gatherer adaptive suite? Does the change of tool types represent the evolution of a technology? Do the pierced shells represent a unit of exchange and what economic value did they have? Are changes due to environmental fluctuations? Are these my ancestors, or the ancestors of a different ethnic group?

As noted at the beginning, the theoretical problems are not divorced from the empirical ones. Clearly, they inform each other. The decision to apply the tools of evolutionary biology to artefacts is partly a means to assess the data, but it is equally a contentious claim that cultural change mimics changes in species, with processes of adaptation, drift and the splitting of lineages. Nevertheless, with these background questions in mind, the empirical and the interpretive, we can begin to examine archaeology and philosophy.

Archaeology and Philosophy

Although much theoretical work had gone on prior to the 1950s, (Trigger 1990) as archaeology became more professional, it became more self reflective about its empirical endeavour and its status as a science. Building on the earlier work of people like

Julian Steward and Leslie White, the American archaeologist Lewis Binford articulated a view of archaeology that became known as the “new archaeology,” and later processual archaeology (Binford 1962, 1965, 1972). Binford argued that the “new archaeology” was anthropology, and anthropology’s goal was to

explicate and explain the total range of physical and cultural similarities and differences characteristic of the entire spatial-temporal span of man’s existence. (Binford 1962: 217)

Binford was not fooling around here. Archaeology was anthropology, and had a crucial role to play, as it was concerned with the entirety of the human past.

Archaeology also needed to be scientific. Binford took Hempel’s hypothetico-deductive model of science as the template for a scientific archaeology. This adoption of the Hempelian model was clearly problematic. Archaeology possessed nothing that looked even remotely like the covering laws that are needed for the hypothetico-deductive method to work. Despite this, Binford’s adoption of a philosophical position achieved two things. Firstly, it prompted archaeologists to ask meta-theoretical questions about their discipline. These questions centered upon the aims of being a science, how this was to be achieved, and whether this was good aim to have. Secondly, it gained the attention of a small number of philosophers. The resulting mix between archaeology and philosophy became a “meta-archaeology.” On the philosophical side, the contributors included Merilee Salmon, Peter Kosso, Patty Jo Watson and Alison Wylie (Watson et al. 1971; Salmon 1982; Kosso 2001; Wylie 2002). This self-reflection was on the whole productive for theorizing about the empirical issues of archaeology.

Middle Range Theory

As Peter Kosso notes: “Evidence in Archaeology, since it is an informational link between the unobservable past and observable data in the present, must be accountable to justification that the link is secure and accurate” (Kosso 1993: 163). Before archaeology engaged in anthropology, it had to secure the relationship between observations of data and past human behaviors. The theorizing, and the theory building, designed to deal with the empirical aspects of archaeological practice came to be known as Middle Range Theory (Binford 1977; Raab and Goodyear 1984; Kosso 1993).

Middle Range Theory (MRT) is a term borrowed from the sociologist R. K. Merton. In its original sociological context, it meant low level, or localized theorizing about social phenomena. Rather than attempts to create high-level theories that applied to all people at all time, Merton advocated building general theories from localized theories (Raab and Goodyear 1984). In practice, much archaeological theorizing takes this form because the theories that link data with human behaviors are usually context specific. Middle Range Theory in a local context looks like a localized ethnography.

However, MRT can include more than anthropology. Geological and other post-depositional processes can add noise to signals of the human past, so separation of signal from noise is an important process in bridging from data observation to claims about the human past. Consequently, MRT can include theories about non-human causes of archaeological noise, theories that link physical remains to human behaviors, and

localized anthropology. Moreover, because MRT attempted to identify *regularities* in the relationship between data and past behaviors, it made possible experimental archaeology.

Kathy Schick and Nicholas Toth in *Making Silent Stones Speak* (1993) outline some fine examples of experimental archaeology. Controversies in the study of human evolution made it important to discriminate between marks made by animals, and marks made by early stone tools on animal remains. Schick and Toth engaged the services of their pet dog to gnaw on bones, and under a microscope, compared these marks with marks made on bones from an animal butchered with stone tools. This “testing” of alternative hypotheses helps ensure the reliable interpretation of archaeological remains (Schick and Toth 1993). At its most extreme, experimental archaeology manifested itself as “Garbology,” the cataloguing of humans’ relationship with their refuse. Michael Schiffer in particular attempted to document the relationship of people to their material culture in a way useful for archaeology (Schiffer 1976; Schiffer and Miller 1999).

Whatever we may think of searching for laws of archaeology and anthropology as the goal of these sciences, the new archaeology of Binford made archaeological inferences much more explicit, rigorous, and in some cases, reproducible. Still, difficulties emerged concerning the range of human behaviors archaeologists wish to make claims about. It is one thing to infer that an animal has been butchered by humans and not dogs, it is quite another to infer an artefact’s religious significance. As early as 1954, Christopher Hawkes had outlined this problem (Hawkes 1954).

Hawkes suggested that inferences about the physical facts of artefacts are very secure, and reliable. As we saw above, such inferences can in some cases be tested. He then suggested that economic-subsistence facts, while slightly less obvious, are still reasonably inferred from physical evidence. But as archaeologists try to infer facts about past political institutions, and then on to ideologies and religious beliefs, inferences become increasingly difficult and open to question. Hawke’s hierarchy, as this range of inferences became known, captures nicely the difficulties of archaeological inferences from remains to a range of human behaviors. In many ways, Middle Range Theorizing, in its initial sociological formulation, was supposed to attack all these levels. In archaeology, MRT became somewhat stuck at the lowest levels, working with the empirical problems of evidence, and basic claims about artefacts and subsistence technologies.

The Science of Archaeology

Despite the practical success of Middle Range Theory in raising the empirical bar for archaeology, there are still no laws of archaeology that the Hypothetico-Deductive approach demands. There is no set of general theories for dealing with archaeology’s empirical issues, or its interpretive problems.

One solution that philosophers offered was to abandon the flawed law-based approach to archaeology. Merrilee Salmon suggested that archaeologists could use statistical induction. She also suggested that this inference could be Bayesian (Salmon 1982). While Watson et al. (1971) could still argue for positivism in the early 1970s, by the late 1980s, the project fizzled out. Philosophers of archaeology who had worked in an era that had dealt with the fallout from the radical critiques of Kuhn and

Feyerabend attempted to evaluate archaeological practice with a view to understanding how it worked, rather than proscribing how it should work.

Two philosophers in particular were well positioned to do this. Peter Kosso co-wrote articles in meta-archaeology that utilized the practical experience of archaeologists in the field (Kosso 1993; Kosso and Kosso 1995; Kosso 2001), and Alison Wylie was a trained archaeologist, as well as a philosopher (Pinsky and Wylie 1989; Wylie 2002). Both were uniquely positioned to contribute to the interdisciplinary discipline of meta-archaeology.

Alison Wylie's description of archaeological practice is one where initial hypotheses are proposed, and then confronted with evidence. The hypothesis is not then "tested" as such, but, rather, modified to accord with the evidence, and then re-examined. She refers to this process as one of "tacking" between hypotheses and evidence, gradually refining hypotheses over time. Hypotheses are modified in response to failures and successes in accounting for the data (Wylie 1989, 2002).

This approach is echoed in the work of Peter Kosso, although he comes to this position from working with historical archaeologists. According to Kosso, theories should cohere with the evidence and background theories and vice versa, in a coherent network of information and understanding. We should abandon or modify theories, or re-examine evidence, when this coherence is lacking (Kosso 2001).

Despite differences, in the end, meta-archaeologists seem to have come to the conclusion that the best archaeology engages in some form of reflective equilibrium to generate reliable claims about the past. There are details to be worked out here of course; details about which claims take priority or possess more "epistemic weight" (Kosso 2001: 174), and when a hypothesis modification is legitimate, as opposed to ad hoc. However, one suspects that the refinement of this basic idea will be on a case-by-case basis, for the hybrid nature of archaeological data gathering is important, and some contributing sciences to the archaeological project seem more reliable than others.

This reflective nature of archaeological practice informs both the empirical level and the interpretive levels. But despite the general convergence on reflective equilibrium as an account of archaeological practice, there are still debates to be had within archaeology. For if theory and evidence really are intertwined in important ways, then which theories one chooses, and where hypotheses come from, matters. Archaeology is not solely driven by its data. It is also driven by the questions archaeologists wish to ask. It is this theoretical side of archaeology that now generates the most debate.

Where Do Hypotheses Come From?

It is one thing to acknowledge that theories are capable of modification, but there remains a question about which theories and hypotheses are appropriate to archaeology.

The processual archaeology of Binford introduced a thesis about what cultures are that meshes well with the highly empiricist and positivist viewpoint he was trying to promote. For processual archaeology, the mental lives of past peoples are unobservable, and hence out of bounds for study. The view of culture promoted by the processualists was to see a culture as a system of integrated adaptations to the environment (Binford

1962). The advantage of this view is that it allows the study of past cultures through their material remains. While some of the material remains of a culture would decompose and not be available for study, the integrative view of culture makes it in principle possible to reconstruct the whole from fragments.

However, this view of culture is at odds with a view that sees cultures as ideational; a shared set of beliefs, language and so forth. Processual archaeology's focus on material culture as an adaptation ignores this ideational component in culture. Archaeology's professional alliance with anthropology aggravated this concern over a lack of ideational components in culture. In anthropology, there is a continuum from physical anthropologists that look to the biological sciences, to the ethnographers and cultural anthropologists who look to the humanities. Within cultural anthropology, the ideational, ideological, and belief systems of cultures play a crucial causal role in change and response to external events. Consequently, archaeologists exposed to these ideas have been asking questions about the causal roles of people's beliefs, ideologies, symbolic systems and intentionality that processual archaeology was not in a position to answer.

The most overt response to this problem was "interpretive archaeology," which attempted to elucidate agency and meaning in the past. The full range of options explored within a broadly defined interpretive archaeology cannot be explored here. Interpretive archaeology has utilized not only ideas from cultural anthropology, but also hermeneutics, semiotics, Marxism and the various strands of post-structuralism (see Hodder and Hutson 2003).

One of the key workers in this area has been Ian Hodder. Hodder has actively engaged in building a post-processual archaeological theory that pursues the recovery of human agency and meaning, and has also been proactive in trying to implement this as a methodology in the field (Hodder 1991, 1996, 1997; Hodder and Hutson 2003).

This move in archaeological theorizing has engendered some justifiable self-doubt about how much of the past can be reliably known. There is a natural path here to relativism and skepticism about archaeological knowledge. As Stephen Shennan points out, it can lead to a certain "loss of nerve" (Shennan 2002).

Despite this, it is worth noting that interpretive archaeology has on the whole avoided a full-blown relativism with its anti-science overtones. This is partly because archaeologists in their data gathering are so intimately connected to the sciences. Ideological relativism denies too much of their practices and discipline. They naturally perceive their practice as useful, reliable, and independent of any ideological or cultural bias. One cannot probe the meaning and intention behind archaeological data if one remains ideologically dubious about the science that produced that data in the first place. Nevertheless, there remains a residue of suspicion about western meta-narratives, and "neo-evolutionary" approaches to archaeology.

Cognitive Archaeology and the Archaeology of Cognition

A second strand of post-processual archaeological theory also took cognition seriously. Archaeology is a crucial discipline in understanding hominin evolution and the evolution of cognition, but theorists have rarely individuated this work from

the archaeological project as a whole. Meta-archaeology has typically focused on the archaeology of *Homo sapiens*.

The philosophical interest in this area of archaeology is twofold. Firstly, archaeologists investigating the evolution of human cognition have adopted particular cognitive frameworks to make sense of the past. For example, Steven Mithen utilized a modified version of Jerry Fodor's modular mind hypothesis in "The prehistory of the mind" (Fodor 1983; Mithen 1996), and Thomas Wynn adopts a Piagetian framework for his work in understanding stone tool technology (Wynn 1995, 2002). The utilization of psychological models in archaeology adds yet another potential problem in archaeological thought, for these hybrid works cross the boundaries of psychology, biology, neuroscience and archaeology. Consequently, these syntheses potentially import errors from their contributing disciplines. It is important that archaeologists do not use cognitive science models uncritically.

Clearly, this work can be seen in light of Wylie's "tacking" version of archaeological science. In these cases, the starting hypothesis is not an anthropological or behavioral claim, but a psychological one. This leads us to the second philosophical interest in this work: Theory refinement and modification need not move in a single direction, with cognitive science informing archaeology. As Peter Kosso notes, we are looking for coherence between hypotheses and evidence, so there emerges the real possibility for cognitive science that archaeological evidence can add an extra empirical constraint on its speculation. Is the emergence of art in the upper Paleolithic really the result of the emergence of consciousness? (Mithen 1996, 1998). If so, what have we just learnt about consciousness?

Utilizing the cognitive sciences and their models need not confine itself to the evolution of cognition. Cognitive science can be useful to archaeologists reconstructing past human behaviors in general. The possibility of "cognitive archaeology," research that explores archaeological questions utilizing appropriate tools from the cognitive sciences, has in fact been actively discussed. (Renfrew and Zubrow 1994; Shennan et al. 1995; Renfrew and Scarre 1998; Whitley 1998). As yet cognitive archaeology remains somewhat tentative. However, its development promises to be one of the more interesting areas of archaeological theorizing.

Darwinian and Biological Archaeology

Two further strands of post-processual archaeology embraced a richer Darwinian approach than that provided by processual archaeology. The first attempted to utilize the tools of behavioral ecology, which analyses an organism's behaviors and their adaptive costs and benefits. (Krebs and Davies 1997; Winterhalder and Smith 2000) To an extent, behavioral ecology in archaeology is a natural successor to the view that cultures are adaptive. The distinction is that behavioral ecology looks at individual behaviors, as opposed to an entire culture. Behavioral ecology when applied to human behaviors is clearly a social science, and it overlaps with economic approaches, which take subsistence and economic problems seriously.

The second strand of Darwinian archaeology is more problematic. The long-term records of human material culture show change, stasis, optimality, and cultural

lineages analogous to biological lineages. The idea that processes similar to natural selection can play a role in changes in technology is clearly tempting.

Natural selection acts on a population. In biology, the debate over what counts as a population is in part a debate about different levels at which natural selection can operate; genes, individuals, groups, or perhaps species. Archaeology faces an analogous debate: What is the relevant unit of selection? There are various prospects on the table: One is memes, the replicator analogy of Richard Dawkins. (Dawkins 1989; Cullen 1996) Human individuals are an option, but so are entire cultures, and the tools themselves are also potentially Darwinian populations.

Robert Dunnell argues that natural selection operates on artefacts (Dunnell 1971, 1986, 1989; Embree 1992; O'Brien 1996). Like processual archaeology, the "selectionist archaeologists" who embrace these ideas downplays human intentionality, claiming cultural changes to be the result of adaptation or drift. They have also utilized phylogenetic techniques to investigate cultural lineages (O'Brien et al. 2001).

However, this view is at odds with the behavioral ecologists, which take human behavioral flexibility and intentionality seriously. Consequently, there has been heated debate among these "Darwinian archaeologies" over what counts as a "unit of selection" for archaeological investigation (Maschner 1996).

The debate between the various Darwinian approaches is far from settled (Jeffares 2002, 2005). It is also a reflection of a broader debate about the role of Darwinian explanations in the social sciences. (See Laland and Brown 2002, for a good overview of the options in the social sciences.) Nevertheless, Darwinian explanations are explanations that claim history matters. Consequently, archaeology plays a crucial role in assessing the worth of Darwinian social science.

Environmental Archaeology

Concerns about the future, stemming from our understanding of what we are doing to our world now, have driven an area of archaeology also not previously given much theoretical thought. Past models of human-environment interaction have typically been shaped by the notion of adaptation to external pressures. The model of culture advocated by the processualists embraced this approach. However, this sits uncomfortably with environmental historiography, which emphasizes the omnivorous and highly flexible technologies of human beings, and a somewhat damning recent history of over-exploitation of resources.

Human interactions with their environments and other organisms are likely to be very complex, and inter-related. We are not passive adapters to an unchanging environment; we actively shape our world, and have for thousands of years (Jones 1969; Barton et al. 2004). This shaping changes the organisms that live in these environments with us. And while we have clearly exploited some organisms, the exploitation has not been purely one sided. Both domesticated and parasitic organisms have hitched a highly successful ride with us, benefiting from our modifications of areas of the world. A myriad of organisms have followed human migrations across the planet.

An alternative set of models for the interactions between human beings and their environment and their cohabitant organisms is an emerging project for archaeology.

This means that the target for archaeological research expands to include not just human beings, but their physical and biological world. This project also ties into the concerns of the humanities investigating the boundaries of human relationships with the natural world (Cartmill 1993; Schama 1995).

Archaeology as Social Science

However, the core debate within archaeology still remains; is it a science and if so of which kind? Its sophisticated techniques of data recovery make it look like a science, complete with test tubes, white lab coats and the complete paraphernalia of the laboratory. Despite this, the notion that human beings, their behaviors and beliefs, are capable of being scientifically studied remains an anathema to some, and simply incomprehensible to others.

In many ways, the problem for archaeology is the appropriateness of model acquisition from other disciplines. Because of the hybrid nature of its explanatory project, there are many options available. As we have seen, interpretive archaeology looked to the humanities for explanatory strategies and tools. Cognitive archaeology has looked to the cognitive sciences. Darwinian archaeologies look to an adaptive account of long-term changes, and economic accounts of short-term proximal biological goals.

Are these views necessarily incompatible? In some cases, they may be, but one suspects that much of the time, there are simply different archaeological questions, and these shape the appropriateness of a particular starting hypothesis. The sheer scale of some archaeological questions – the evolution of human cognitive capacities, the spread of agriculture, the impact of human migration on environments – are such that they are probably beyond the scope of individual or even group agency. The flipside of this is that the archaeologist can be confronted with the achievement of an individual; a spectacular piece of cave painting, or even something as mundane but as elegant as a well made pot. Some remains need to be seen as the acts of individuals engaged in the process of constructing meaningful things, individuals who made choices we wish to make sense of, choices driven by various beliefs and desires. Sometimes we want both descriptions at once; the economics of an artefact and its function, the beliefs and meanings associated with its decoration, embellishment and use. There is a continuum of options and explanatory targets in archaeology, and the boundaries are not yet set. So despite this compatibility, or perhaps because of it, as a social science, archaeology finds itself on the front lines of a debate on what it is to be human, and just how we should go about studying that most weird of animals, *Homo sapiens*.

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Reductionism: Historiography and Psychology

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One cannot approach the question of how historiography relates to psychology and social science without assuming that historiography is related to the study of human minds, society, human action and, more broadly, social events. This does not unduly restrict our topic, as most human actions of historical interest have social implications.

How can and do historians explain social actions and events if they are neither psychologists nor social scientists? We proceed on the assumption, for which there is ample evidence, that historians do, at least sometimes, engage in explanatory tasks, citing causes of significant events. They are concerned with answering questions such as: why did the English Revolution occur? What brought about the First World War? What caused the potato famine in Ireland?

Must such explanations be derived from, or depend on, the theories and laws of the disciplines in the social sciences and psychology? The notion of “deriving” or “reduction” involved here is ambiguous. It sometimes alludes to ontological reduction, where what is at stake is the commitment of some theory or discipline to entities of a distinctive kind. Ontological reductionists about the mind are typically committed to the view that the mind is a physical entity, not a distinctive kind of entity in its own right. Likewise, ontological social reductionists claim that groups imply only an ontology of individuals; groups or social wholes are redundant entities. Such ontological reduction is compatible with explanatory non-reduction, so one can, in philosophy of mind, be a non-reductionist physicalist; one can claim both that the mind is physical and that physical explanations cannot reduce psychological explanations (see, e.g., Davidson 1970).

“Reduction” can also be interpreted as *explanatory reduction*, sometimes called theoretical reduction, and this is the sense that concerns us here. In positivist philosophy of science it was typically understood as taking place when the laws of one theory are reduced to the laws of another theory via bridge laws linking the theoretical terms of each theory, as was proposed as the relation between historically successive scientific theories. This is a strict notion of reduction that had to be relaxed to include the “amendment” of the theories in light of the reduction. We prefer the term “explanatory reduction” because we do not wish our discussion to rest upon the availability of any historical laws for which the question of theoretical reduction could arise. Rather, we ask whether the putative historical explanations must be *dependent*

on sociological, economic, or psychological explanations, and so dependent on terms borrowed from those disciplines. If the answer is “yes,” then reducibility implies the absence of *sui generis* historiographic explanations. Historiographic explanations are reducible, on this way of understanding that term, if they are simply a species of, or are essentially dependent on, some other type of theoretical explanation, in this case sociological, economic, or psychological. This way, we avoid begging questions concerning the relation between explanation and the presence of laws and so remain noncommittal as to the nomological concept of explanation (cf. Hempel 1965).

Another preliminary point that needs to be made concerns the modality used in couching our question. The issue is not whether historiographic explanations *can* borrow from the social sciences (to be understood hereafter as including psychology); clearly they can and do so while retaining a robust sense of *sui generis* historiographic explanation. There is an obvious sense in which social scientific explanations may well be useful for historiographic inquiry: they may sometimes be required as supplementary to the otherwise incomplete historiographic explanations. Even so, there could still be room for a unique, historiographic, type of explanation, even if this were necessarily only part of the full explanation of historical events. The principal concern is with whether there can be such a unique type of historiographic explanation, one that does not depend for its explanatory force on explanations from other disciplines. Our discussion will inevitably yield conclusions about the extent to which historians should consult the human sciences in their search for adequate explanations.

1

It is tempting to begin our inquiry with an account of what explanation is, and to move on from there to consider how putatively unique historiographic explanations fit that account. The *locus classicus* of such an approach is Carl Hempel (1942), who applied his analysis of scientific explanation to the case of historiography. That analysis is disjunctive: an explanation is legitimate *qua* explanation if and only if it provides the means to deductively derive a description of the event being explained (the explanandum) from a statement of a law and a description of some initial conditions, *or* if the law-statement and description of initial conditions makes the explanandum highly probable. This clearly makes the presence of a law essential to all scientific explanation, hence the designation of it as the “nomological account” of explanation.

Can we allow such an analysis to dictate whether there are unique historiographic explanations, or should our account of explanation be based on actual explanations in historiography? This is an issue concerning the acceptability of any proffered analysis of explanation, particularly one which may rule out some explanations that practitioners in some disciplines may deem acceptable. Hempel himself seemed to vacillate between ruling out some putative explanations on a priori grounds and amending his own analysis in light of current scientific practice. The rejection of “particularistic” or “singular” explanations (explanations that do not cite any general laws, universal or probabilistic) manifests the first tendency, the extension of the analysis to include probabilistic explanations manifesting the second. The dilemma posed by these two possibilities is that if, on the one hand, one stipulates in advance

that only some explanations count as data for one's analysis, then the disqualification of some putative explanations will appear to be entirely arbitrary. If, on the other hand, one tailor-makes one's account to fit all putative explanations ever offered, then one will end up with no useful notion of explanation at all. This dilemma is present for every analysis that claims to possess both descriptive adequacy and normative "bite."

There is no simple way to move between the horns of this dilemma, but progress can be made by inquiring into the rationale behind Hempel's requirement that laws be present, implicitly or explicitly, in every bona fide explanation. That requirement reflects three thoughts: that in explaining why an event occurred one must render the event less sheerly contingent than it otherwise would have appeared to be; that the only way of doing this is to provide a description of the prior circumstances that necessitated (or made highly likely) the relevant event; and that such necessitation requires laws to be present to "cover" the antecedent circumstance and subsequent event (relevantly described).

The first two thoughts seem unproblematic, though the degree to which an explanation must make less contingent, or "probabilify," the explanandum is unclear. What is crucial is the third thought, that necessity (or a decrease in contingency) requires laws, or at least generality. It is here that a historian may demur, as many historians have claimed that what they are after is an understanding and explanation of why *unique particular* events occurred, and so the generality required for explanation in physics or chemistry is not pertinent to historiographic explanation. On this view the uniqueness of historiographic explanation, its non-nomothetic character, becomes a consequence of the uniqueness of the events historians explain.

Setting the scene in the manner above allows us to concentrate on two issues: Whether there are special, irreducible, historiographic laws, and whether uniquely historiographic explanations require such laws. The notion of "law" at issue here is not particularly restrictive; it need not depend on some close resemblance to laws in the physical sciences (what we can call "nomothetic law-likeness"). What is essential is the guiding idea in Hempel's requirement on any explanation, that the explanandum be rendered, to some degree, non-contingent, and this leaves it open whether there are diverse (non-nomothetic) ways of rendering an explanandum non-contingent.

We consider next the arguments for law-based historiographic explanation, and so the possibility of explanatory reduction, and then the possibility of non-law based explanation.

2

An outline of an argument purporting to show that there could be no historiographic laws can be found in the work of Karl Popper (1957). Popper connected the existence of laws of history to prediction: knowledge of laws plus initial conditions should be sufficient for prediction about the future, but there can be no such prediction in human affairs. Future states of the social world depend on the knowledge available to agents in that world. In order to predict what such future states would be like we would need to have access to the knowledge possessed by those future agents, given that agents

act on the knowledge available to them in changing their social world. We could not have access to such knowledge in the present if that future knowledge rested on, say, new discoveries or new technologies. Given we are not in a position to predict what new discoveries may be made, nor what new technologies may be invented (otherwise it would not be new), we are not in a position to predict what future states of the social world would be like. Hence there can be no historiographic laws.

The argument generalizes to any laws that could be used in predicting a future whose features depend on the knowledge available in that future state. Economic, psychological, and social laws are likewise ruled out. (For further discussion of this nihilistic conclusion see Macdonald 1995.)

The difficulties attached to the notion of there being genuinely *historiographic* laws can be illustrated by two examples. Jack Goldstone (1991) examined periods of social and political turbulence in the histories of England, France, the Ottoman Empire, and Qing China. He found significant similarities between those regions immediately prior to the turbulence, such as demographic change (rapid population increase, high number of young people), state financial crises, elite competition, and a mobile population. On the basis of these similarities he advances a theory of revolutions, which he claimed is supported by the evidence from the period. The underlying trigger is demographic change, a population explosion producing a high ratio of young to old, thereby causing inflation, elite competition, and “mass mobilization potential.” The latter two are also exacerbated by inflationary price rises, leading to state fiscal distress as the state’s revenues fail to match its expenditures. What Goldstone calls the “political stress indicator,” *psi*, rises rapidly, foreshadowing the turbulent times ahead. The result is a loose law relating the various factors to the *psi*, which is then used as an indicator, and explainer, of political turbulence.

The historiographic examination of whether there is a common explanation for the fluctuations in the fortunes of empires offers another example. Peter Turchin (2003) studied the dynamics of territorial expansion and retraction, taking as case studies the oscillations in size of the French and Russian polities (primarily between 500 and 1900 CE). On the basis of these studies he suggested that there is a common dynamic pattern to such expansions and retractions in size. Both Goldstone and Turchin are clearly interested in the histories of their target areas and periods because they are interested in whether there is something significantly common to these historical periods, whether there is a pattern to be discerned. Their interest in particular cases would appear to be exclusively as evidence for the theories they are advancing. Are they providing law-based *historiographic* explanation? (Any such laws will be *ceteris paribus* laws, but this is to be expected, and is not uncommon.) Perhaps Turchin’s description of what he is doing as “historical sociology” best fits the enterprise, but the question we face is whether such explanations are reducible to other theories in the human sciences. *Must* there be lower-level laws to which these historiographic “laws” are reducible? It would appear that there need not be. At the very least an argument would have to be given that there *must* be such reduction-facilitating laws, and it is difficult to see what form that argument could take.

There is, however, a slightly different route the reductionist might take, and that is to claim that the formulae generated by historians such as Goldstone and Turchin remain mere historiographic generalizations, and must remain so until the mechanisms

underlying the disclosed patterns are discovered. Once these mechanisms are known, they will provide the details required for the reduction. Thus, for example, Elster (1985) argues against the view that one could have Marxist-style explanations of economic change without providing any mechanism responsible for such change (Cohen 1978). Elster later expanded this argument to encompass all explanations in the social sciences, as Wesley Salmon argued about explanations in general. Rosenberg (2001) develops the same position *vis-à-vis* reduction in evolutionary biology. Cohen argues that the likely required “mechanism” will describe people, their mental states (cognitive and conative), their particular circumstances, and their interactions. The mechanism should give rise to the patterns historians discern and debate.

The suggestion, then, is that historical generalization will become law-like only when underwritten by psychological (“individualistic”) explanations. For instance, in the Marxist methodologically individualist no-nonsense explanation of a certain historical pattern, a description of the relevant mechanism is generated by considering the question: why is it that the technological forces (the “base” in Marxist terminology), tend to change in the direction of increased productive power? One plausible answer is that in conditions of scarcity rational people will exercise their rationality in pursuit of more efficient ways of producing the goods necessary for their survival, and this will engage them in inventing new technologies, thereby yielding the increased productivity of the productive forces of the society in which they live. The direction of historical change will be derived from a fairly simple psychological explanation, this providing the reduction for the macro-individual law.

This strategy generates two further questions. One, which we must set aside for present purposes (but see Macdonald 1986, 1992, for a negative answer; Rosenberg 2001, for a positive one), is whether knowledge of the underlying mechanisms is required for any “historiographic generalizations” to play an explanatory role, and if so, whether this provides sufficient support for reduction. Another is whether there can be irreducible historiographic explanation that is not law-based, and this raises important issues that connect with our second main topic. The possibility of such explanation is thought to depend on the historian’s interest in explaining the particular rather than the general. What is at issue, then, is the kind of knowledge of individual behavior that must be available to the historian when explaining particular historical events and actions.

3

The particularist who believes in the uniqueness of historical events and historiographic explanations stresses the importance of particular *token* events for the historian. Theoretical sciences may be interested in past events, but they would be interested in those events as tokens of *types*, not as bare *tokens*, and their explanations will be couched in terms appropriate to the explanation of types, and so be inherently general. The explanation given by a historian will always be an explanation of (some of) the properties exemplified by the token event. Despite this, the particularist will insist that it is the specificity of the historical event(s) that is the principal concern of the historian, firstly in discovering the detail of what occurred, and secondly in placing that detail

within the context of the period in such a way as to illuminate how what happened came about.

The particularist is surely right that an essential feature of historiography has been its focus on discovering what happened, when it happened, and why it rather than some other event occurred (this being phrased contrastively simply because it is often in the light of the expectation that a different course of events might have occurred that one seeks an explanation of *this* sequence of events). To what extent must such explanations depend on the theoretical deliverances of the human sciences (psychology or, more generally, social science)?

This question is not about the extent to which these explanations *mention* psychological and social characteristics of the people and societies with which the historian is concerned. It would be easy to show that historians must rely extensively on the human sciences if all that required was that their explanations cite psychological and social properties of the characters and events mentioned in the *explanans*, since it is difficult to find any historiographic explanation devoid of any such mention. The historian who explains why Charles I of Britain acted in the way he did must cite the social milieu, the political situation, the economic background, the aims of his adversaries (the Earls of Bedford and Warwick, for example), the place of the monarchy in the constitution, Charles' beliefs about both that role and what his adversaries were trying to do to change it, and so on. All of this makes ample use of psychological and social properties, but there is no obvious dependence here on any psychological or social *theory*. What is employed in this type of explanation is what has been called "folk psychology," and, as the name suggests, it is distinctive of folk psychology that it is pre-scientific, available to all, and used in common-place explanation of human behavior (Horgan and Woodward 1985). Folk psychology embodies a (typically implicit) common understanding of ourselves, so no special knowledge of or expertise in any science is required in using it. Insofar as historians rely only on such common understandings, their dependence on the deliverances of research in the human sciences will be minimal. But can we safely rely on the veridicality of the hypotheses concerning human action that are embedded within our common understanding of ourselves? More needs to be said about the "folk" in question, and about the psychology presumed.

Folk psychology consists of a set of assumptions about mentality employed by us in our everyday activities and interactions. We attribute to each other beliefs, desires, emotions, hopes, and the like, as well as enduring character traits (generosity, stubbornness, kindness, etc.), on the basis of which we are able to understand and explain the actions of both ourselves and others (D'Andrade 1987; Sousa 2006). An action *qua* action is explained as arising from those mental states which show why that action was the *rational* thing to do, given the circumstances and the psychology of the individual. Such a dependence on folk-psychological assumptions underpins Hempel's rationalizing gloss on historiographic explanation. But this dependence makes his account vulnerable to at least two distinct charges: first, that the presupposed psychology is too parochial to be reliable: it presumes the truth of the EA (European-American) folk model. Just as the anthropologist has to struggle to avoid cultural imperialism in interpreting other cultures, so ought the historian be suspicious of treating other times as though the people inhabiting them were "just like us," Hume's famous mistake in proposing to observe the English and the French to understand the Greeks and

Romans. The folk psychology of other folk may be different. If it is different we need to be aware of how the difference impacts on our view of the way others behave, given that our self-understandings permeate our social interactions. Second, the account of rationality that is embedded within folk psychology is too simple, and credits us with more explicit knowledge of why we act in the ways we do than is warranted by the evidence (Dray 1957, 2000). Not only is it too simple, but it is actually false. Using folk psychology in explanations of historical actions will result in an inevitably flawed explanation.

These charges suggest that we cannot bypass serious empirical investigation into the causes of our actions, since only this will provide reliable information on what *really* makes us act in the ways we do. If so, then the historian cannot rest content with folk-psychological platitudes about the sources of human action. Let us first examine the charge of “too parochial,” before moving on to the charge of “too much rationality.”

4

Historians have been aware of the danger of what T. S. Eliot called “temporal provincialism,” imagining the past too much like the present. One quick way to deal with the problem is to mount a transcendental argument to the effect that folk psychological conceptions of the self are necessarily universal, conditions of any understanding of others. (See Davidson 1974, for an argument of this sort.) We think that the best this can deliver is the conclusion that any understanding of others is predicated upon taking as universal some minimal set of basic psychological properties, principally those of belief and desire.

But there might be a more substantial set of psycho-social properties which can be presumed to be universal. An example of a more substantial assumption concerns the type of rationality evinced by those whose actions were in part responsible for the demise of feudalism and the arrival of capitalism on the world scene. One explanation of this transition claims that the psychological mechanism in play was fairly simple: rational people could see that new socio-economic ways of organising production were being made available by advances in technology, that these new ways would be beneficial, so they struggled successfully against defenders of the status quo to bring about the necessary changes (Cohen 1978). Some complain that this is anachronistic in making the rationality of the successful agents too much like ours. In particular, it ignores the class-specific nature of rationality, and so attributes to some of the principal agents (peasants) an already “capitalist” mentality, concerned with an interest in optimising output by raising levels of production through using new technologies (“developing the forces of production”). An alternative explanation insists that prior to losing their property, feudal peasants had no such goal in mind, being more concerned with using the land at their disposal to eke out the living they needed (see Aston and Philpin 1985, for an extensive review of the debate).

This alternative explanation still takes place within what we can understand as a (broadened) folk psychological model, one that still presupposes the psychological properties and propensities of our familiar self-conception. Other examples, however,

involve differences that are slightly more dramatic. The people of Ifaluk have terms in their conceptual repertoire similar to our folk psychological terms, but there are differences in the way they connect with each other. It is, for example, a matter of some debate whether our model of moral behavior and understanding allows for the possibility that we can understand that *f* is the “good” thing to do while not acting in a manner appropriate to that understanding, i.e., knowingly doing the bad thing. This is not possible on the Ifaluk’s understanding of the connection between moral understanding and action (on one construal of it): if one understands what the correct thing to do is, one of necessity does it (see D’Andrade 1987: 144, reporting work of Lutz 1985).

It is also well-documented that the western folk model tends to understand actions as being caused by people, or, more specifically, personal traits, whereas some other cultures (Saudi Arabian, Indian, Chinese) stress the action as being produced by the situation the person is in (Lillard 1998). This has repercussions in how it is that people are likely to treat one another when something “bad” happens to them. Western folk theorists tend to blame the actor, situation theorists blame the situation. (For an empirically based anthropological view that there are some universals see Wierzbicka 2006.)

Empirical knowledge of the variants available to different folk can alert the historian to possible sources of misunderstanding in the study of historical periods culturally remote from our own. This knowledge will play a supportive role in historiographic explanation, and so will not normally require wholesale revision in the nature of historiographic explanations; the model applied will still use relatively “unscientific” concepts, relating as it does to commonsense, pre-theoretical “knowledge.” Perhaps empirical inquiry from anthropology can make the historian aware of various pitfalls in the unreflective use of a western folk psychological model. The matter will be different if the historian has to do away with such pre-theoretical understandings, either in part or altogether, replacing them with explanations derived from theories in the human sciences. It is to this possibility we now turn.

Irrational action seems clearly to be part of the subject matter of historiographic research, so a paradigm of explanation in which only rational actions are explicable will often fail to be of much use. Historians need to locate irrational behavior *within* the domain of the rational, as behavior that can be understood and explained by appeal to folk-psychological states that are governed by the norms of rationality, but where the behavior departs in some way from those norms. Depending on the distance from the ideal of rationality, the historian may well require guidance from experts in the field. How much guidance depends on the nature of the case. Consider the famous example of George III, who descended into madness in his later years. It may be that historians are interested in the nature of the illness that overtook him, as to whether it was a form of porphyria or inadvertent arsenic poisoning, but one does not need to know a great deal about porphyria (if that is what he suffered from) to explain his actions toward America, or France, or his dealings with William Pitt. The illness itself eventually incapacitated him, but again the historian only needs to know that fact to explain why it was that in the last decade of his reign the role of the monarch was assumed by the Prince Regent. The illness itself did not seem to change the meaning or purpose of his (relevant) actions, so the dependence on any science here is minimal, limited in

the main to the provision of information (the particular form of the illness) and not to substantive explanation.

The case is somewhat different when we consult a less physical or medical cause of psychological disorder. The more the disorder conditions the actions perpetrated, the more reliance there will be on theories from the human sciences. A single example will suffice: in exploring the identity of Jack the Ripper, the notorious serial killer of London in 1888, the historian will inevitably be drawn to the question of *why* whomever he was acted in the manner he did, and the historian will be hampered in this task if the guiding assumption was that such an agent must have been rational. A recent account given by the historian Charles Van Onselen (2007) suggests that the Ripper was one Joseph Lis (alias Silver), who spent some time in Whitehall during the late 1880s. It is essential to the argument that Lis' psychological condition makes him a suitable candidate for being Jack the Ripper; Lis was, on this account, psychopathic, syphilitic, and possessed by an enduring hatred of women. Van Onselen mounts a credible case that such a psychological type was quite capable of serial murder, and that the targets would very likely be prostitutes. In doing this he makes use of the relevant psychological literature on psychopaths and syphilitics, and his account would not have been plausible without that support.

Such guidance is, we suggest, necessary in cases where the agent suffers from a psychological condition that renders their actions clearly irrational. Dementia, phobia, obsessional disorders, and so on, are all part of (some of) the historical material facing the historian, and when countenancing these characters the historian will, of necessity, turn to experts for guidance. In this, rather limited, sense the historian will inevitably be dependent on research done in the human sciences, but, again, overall the dependence is supportive rather than constitutive.

There is an irony in the trajectory just sketched. We began the present section by suggesting that insofar as the historian's explanations of particular events are committed only to a folk-psychological account of human behavior, the historian need not rely to any significant extent on theories derived from psychology or sociology. But we now see that the more agents' psychological states give meaning and direction to their actions, the heavier the reliance on experts becomes. This indicates that rationality itself is not immune to scientific probing, and that insofar as it is the subject of scientific research the historian will be advised to take note. This is not to suggest that historians must slavishly follow the latest fashion in cognitive science or cognitive anthropology; the lesson intended is that it would be prudent for historians to be aware of the burgeoning interdisciplinary work being conducted in the fields of cognitive science, cognitive anthropology, evolutionary psychology, and cultural evolution. This work is often highly speculative and sometimes tendentious, but it also often has the virtue of subjecting our pre-theoretical understanding of ourselves to empirical scrutiny. A historian who, for example, *assumed* that the persistence of religious behavior was primarily dependent on cultural causes, and modeled their account on that understanding, may well be surprised to find that other investigators view such behavior "as a phenotypic analogue to the linguistic capacity that is our evolutionary birthright . . . this means that religious activity is no more 'cultural' than stereopsis or bipedalism" (Day 2005: 86).

We have looked at the possibility of law-based reduction of historiographic explanations to other human sciences, and argued that there was no sound argument requiring such that such a reduction be available. The plausibility of any such argument would depend on the requirement that individualistic psychological features must provide the mechanisms underlying any putative historiographic generalization. We recommended that such a requirement be rejected, but then looked in more detail at these presupposed psychological features. Insofar as historians rely only on a common understanding of ourselves, our folk-psychology, we suggested that theoretical reduction was not available. We then went on to examine this folk-psychology in more detail, arguing that historians need to be aware of relevant research that extends or subverts folk psychology. Reliance on a pre-theoretic understanding of individual action may undermine historiographic explanations offered in ignorance of this research. The picture that emerges is one where historiographic research is seen to be enriched by an engagement with findings in, among other sciences, cognitive science and cognitive anthropology. This enrichment is not reductive; it is enhancing.

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Historiography and Myth

MARY LEFKOWITZ

Some Basic Definitions

Modern notions of history and myth, indeed the very words themselves, have their origins in ancient Greece. Herodotus, writing in the second half of the fifth century BC, called his account of the wars between Europe and Asia “a public statement of his inquiry” (*historiē* 1.1, 7.96). It was through inquiry, by asking or listening, that one learned about the past. The inquirer’s narrative was a *logos*, a word that can mean speech or account. By nature *historia* was far more likely than *mythos* to refer to actual events and real persons because it was derived from actual events that people had witnessed or had heard about directly from other people.

A *mythos* was also a type of *logos*, a narrative with a definite shape, and a beginning, middle, and end. But myths almost always had some religious content, in that they were concerned, directly or indirectly, with forces beyond human control, and the deities who exercised control over those forces. Myths were traditional stories, handed down from generation to generation, composed without resort to *historia* or inquiry. No one knew who created the myths, or exactly when they came into being. Mythical narratives described a distant past about which no one living could have any direct knowledge.

If the myths that have come down to us preserve any factual information about the past, it is only in the form of place-names such as Athens, Thebes, Sparta, or Mycenae. But not all places mentioned in the myths can be discovered; no one yet has found the homes of the gods on Mount Olympus, and no living archaeologist has seen the Lower World, where the souls of the dead are said to reside. It is possible that some personal names in the myths may also have belonged to living persons, but there are no early historical documents or inscriptions against which they can be verified.

Despite their remoteness in time and their references to places which no one had ever seen, as long as they believed in the existence of the gods, they were not prepared to accept that myths, even when unrealistic, were without serious value. Thus myth in antiquity could not have the meaning that it has since acquired, of a narrative that is purely fictional or fanciful, as distinct from historiography.

Myths differ from fiction also because their plots cannot be significantly rewritten or altered. Like narratives obtained by inquiry or *historia*, their plots were known to

many people in a wide geographic area. One could not substitute Oedipus for Achilles in the narrative of the *Iliad*, any more than one could claim that the Persians were victorious over the Greeks in the naval battle at Salamis in 480 BC, because many people knew the stories. Writers of mythical narratives, such as epics and dramas, were free to give their own versions of their characters' motives and to put words into their mouths. A poet could portray Oedipus as being aware of what he was doing when he killed his father and married his mother, or not, if he wished to give the story a different emphasis. But however he told the tale, the poet had no choice but to make Oedipus commit both crimes, because those were the known "facts" of the myth. Similarly, a historian would need to report that the Greeks drove off a Persian armada at Salamis in 480 BC, and give credit to the Athenian general Themistocles for the devising the strategy that won the battle.

Historiography and Myth in Ancient Greece

Despite the differences in the ways in which mythic and historiographic narratives were created, in practice *historia* was not always clearly separated from *mythos*. The first extended accounts of historical events were written by dramatists, who portrayed, or in modern terms, re-created for Athenian audiences some of the tragic events of the Persian wars. These dramas were performed at a religious festival honoring the god Dionysus.

The one surviving example of this genre is the *Persians*, produced by the poet Aeschylus in 472 BC, some eight years after the battle of Salamis. Aeschylus probably fought at Salamis and had plenty of opportunity to hear about it from other Athenians. But although he would have had no direct way of knowing how the news of the Greek victory was received in Persia, he describes in the drama how the news of the Persian defeat was received by the mother of the Persian king, and depicts the king's remorse for his failure and the deaths of so many of his men. By setting historical events into the framework of a myth about human ambition and failure, Aeschylus constructed a drama that was simultaneously a celebration and a sober warning to the victorious Athenians.

Myths were in fact the only narratives about the remote past available to writers of *historia*. Herodotus began his historiography of the conflict between Greeks and barbarians not with a discussion of what he had learned by inquiry, but with a number of stories from the epic poems that would have been familiar to his audience.

In one of these short narratives he related how Paris from Troy in Asia Minor came to Greece and carried off Helen, the wife of Menelaus, the king of Sparta. To make the story sound more like an account of an actual event witnessed by an ordinary human being, Herodotus omitted all references to the role of the gods. He observed that he did not necessarily believe in stories told by poets. But credible or not, he nonetheless included them (1.5.3).

Although Herodotus did not indicate what role the gods might have played in determining the course of human events, his understanding of the workings of the universe has its origins in Greek mythical narratives, such as the *Iliad*. He made it clear from the outset that his inquiry would show that all human achievement is transitory, and

that human beings always would always get things wrong, while true and complete knowledge was reserved for the gods.

Herodotus starts his inquiry into the causes of the conflict between Asia and Europe by describing the rise and fall of Croesus, King of Lydia in the sixth century BC, the first barbarian to conquer some Greek cities. The Lydian King enters the narrative at the height of his powers, confident that he is the most fortunate man on earth. But soon afterwards he loses his son and his kingdom. Only at the last moment does he avoid being executed by his conqueror, King Cyrus the Great of Persia.

Many specific details of Herodotus' account can be verified from other sources. There was such a person as King Croesus of Lydia, a man of great wealth and power, who sent lavish gifts to the sanctuary of the Greek god Apollo at Delphi. But other important aspects of Herodotus' narrative cannot describe historical events. Herodotus describes a significant conversation that Croesus had with Solon of Athens, another historical figure. But the two men could not actually have met at the moment when Herodotus brings them together, sometime during the ten years after Solon's archonship in Athens in 594/3 BC (1.30) because (as Herodotus tells us) Croesus did not become king of Lydia until 561–560 BC (Lloyd 1975: vol. 1, 55).

Herodotus appears to have created the story of an encounter between Solon and Croesus for illustrative purposes. It is in effect a *mythos* that warns his audience of that it is both dangerous and foolish to assume that human wealth and power will endure. King Croesus, rich and successful, asks Solon who in his opinion is the most fortunate man on earth, supposing that Solon will name himself. But instead Solon chooses some Greeks who died before anything bad had happened to them, and warns Croesus about the transitory nature of human happiness. At the time that Herodotus read his historiographic narrative to audiences at Athens, that city was at the height of her powers, behaving as arrogantly and arbitrarily as did Croesus in his day.

As Herodotus tells the story, the encounter with Solon not only allows Croesus to understand why his good fortune could not last, but the wisdom he gained from his suffering also happens to save Croesus' life. Croesus recalls Solon's words when he is about to be burnt alive after his defeat by Cyrus. Cyrus wonders why Croesus calls out the Athenian's name, and has the flames extinguished. Herodotus also tells what he calls the Lydian version of the story: when Cyrus' men failed to put out the fire, Croesus called on the god Apollo, and in gratitude for his many gifts, the god sent a rainstorm and rescued Croesus (1.86–7).

In refusing to choose between the two stories about Croesus on the pyre, Herodotus lets his audience decide which (if either) might be true. But even if neither version is strictly factual, both advise his Athenian audience about a fundamental truth about human experience. Here it is important to note that *alētheia*, the ancient Greek word that we translate as “truth,” in ancient Greek literally denotes that which is “not forgotten.” Herodotus says in his opening lines that he wrote historiography so that the deeds of both Greeks and barbarians would “not become extinct (*exitēla*) in the course of time.”

The Greeks made a distinction between *alētheia*, that which by definition will endure in human memory and that which is simply *etymos*, verifiable or provable. Because myths in the ancient world were thought to convey *alētheia*, they were integral to any attempt to write about human achievement, including works devoted to *historia*.

Thucydides' historiography of the Peloponnesian War is no exception to this rule. He was in a good position to write an accurate account of this war because the events he described had happened during his own lifetime. He had been directly involved in some of them, as a general in the Athenian army. Thucydides composed his narrative not long after Herodotus wrote his historiography of the conflict between Greece and Persia, but he is critical of writers who like the epic poets embellished their narratives with exaggerations, and of writers of stories (*logographoi*) such as Herodotus who chose to write "what was more enjoyable to hear than what was more worthy of remembrance" (*alēthesteron*, 1.21). He explains that he made a strenuous effort to learn from eyewitnesses, and to distinguish among the different accounts they gave of what they saw. He was well aware that his version might give less pleasure to his audience because it was not the account of a story-teller (*to mē mythōdes*, 1.22; Marincola 1997: 117–18).

These observations, especially when translated into English, make Thucydides' approach to historiography appear almost modern. But despite his determination to write a work more serious and lasting than the entertainments produced by the story-tellers, in the absence of reliable first-hand information or documents, he was forced to use some of the story-tellers' techniques. Like the epic poets and Herodotus, he wrote the speeches that he believed the principal characters in his narrative might have given. He differed from his story-telling predecessors only in that he made sure that his audience knew that he had done so.

Thucydides, however, says nothing about the way in which he uses the story-teller's techniques of structuring his narrative. He creates a dramatic contrast between accounts of two Athenian expeditions by juxtaposing them in his narrative, omitting reference to any events that happened in between. First he describes how in the summer of 416 BC the Athenians, at the height of their power and self-confidence, conquered the island of Melos, killed the men and sold the women and children into slavery (5.84–116).

Thucydides then relates that in the winter of that same year the Athenians decided to send an expedition to conquer the island of Sicily, a much more ambitious effort that ended in catastrophic failure, which eventually caused them to lose the war that they had long fought against Sparta and her allies. By contrasting the two Athenian campaigns with one another, and disregarding the less significant events that fell between them, Thucydides makes a point similar to that made by Herodotus in his story about the meeting of Solon and Croesus. Like individuals, city-states can forget that even the strongest human power cannot last, and that the conqueror will eventually be conquered. Thucydides sought to interpret the outcome of the war in this way, because this was what his religion had taught him about the nature of mortal experience.

Only by the use of the narrative structures of traditional myth could an ancient historian make his work truly memorable, or as Thucydides described his own work, a "possession for all time" (*ktēma eis aiei*, 1.22). Historians sought to show their audiences where events fit in the enduring patterns of human experience. As Aristotle was to say in the *Poetics* (1451b) because myths as conveyed in epic poetry and tragedy deal with universals, and historiography with particulars, poetry (*poiēsis*) is a more philosophical and serious matter than historiographic inquiry (*historia*). In order

for historiography to acquire a dimension that was both philosophical and serious, it needed to include some reference, even if indirect, to the truth (*alētheia*) conveyed in myth.

Mythical Historiography in Antiquity

If historiography could acquire depth and dimension by assimilating some of the structures of myth, it follows that myths could also acquire credibility by assimilating some of the detailed information conveyed by historiography. The genre of mythical historiography can be traced back to the fourth century BC, to the dialogues in which Plato included a mythic narrative as a complement to theoretical discussion. These stories were told as if they were traditional (Brisson 1998: 55–62). Plato does not say explicitly that he composed them, but, rather, attributes them to sources that no one living could verify. For example, at the end of the *Republic* (614b), Plato has Socrates give a long and complex description of life after death and the possibility of rebirth. Socrates says that this information had been conveyed at some unspecified point in time by a certain Er, an otherwise unknown soldier who had been presumed dead but then recovered.

No one now supposes that the “myth of Er” could be historical, because no one believes in the existence of the Underworld. But another Platonic myth is often regarded as historical. This is the story of the lost island empire of Atlantis, which (according to Plato) the Athenian statesman Critias learned from his grandfather, who in turn had heard it from Solon. While Solon was visiting Egypt, he had learned about Atlantis from an old local priest (*Timaeus* 22a). Critias explains that the names in the story are Greek, because Solon learned their meanings, translated them, and recorded the story in a manuscript. Critias claims that he read the narrative many times as a child (*Critias* 113a–b).

Critias provides such a plausible description of Atlantis, complete with details about its topography, customs, and governance that people have been searching for it ever since (Brisson 1988: 14; Nesselrath 2002). Yet anyone who had studied Plato’s theories of reality would have known that a second- or third-hand account was unlikely to be true, especially if it had been narrated by one old man to another. In this way Plato suggested to his audience that the story of Atlantis was a myth that conveyed the eternal message of the impermanence of human achievement. But not all composers of historical myths are willing to admit to their audiences (or themselves) that their stories are fundamentally fictional.

Other mythical historiographies were created to fill large gaps in the ancients’ knowledge about their own past. No one knew who Homer was, or indeed where he came from. Stories were told about his life and the lives of other poets, based on the only source materials available to ancient biographers, the works of the poets themselves. The composers of these biographical myths supposed that Homer, like Odysseus, had traveled widely in the Mediterranean, and that the fifth-century dramatist Euripides was torn apart by dogs, a death modeled on the violent end of a character in his drama the *Bacchae*. The biographies of poets often preserved in outline the narrative structure of the myths of heroes who fall out of favor after the moment of

their highest achievement. So whatever the reality, the biographers portrayed the lives of poets as ending in exile, with a demeaning form of death (Lefkowitz 1981).

Another form of mythical historiography consists of what might be called celebrity letters. These epistles appear to have been written by famous figures, expressing their views on the events of their own time, and providing new details about their lives, but their style and frequent anachronisms indicate that they were composed centuries after the events which they purport to describe at first hand. For example, in the letters supposedly sent by the tyrant Phalaris to the poet Stesichorus in the sixth century BC, sums of money are given in Attic drachmas, a currency that at the time was not used outside of Athens. But until the eighteenth century AD many people supposed that the correspondence was genuine.

Mythical historiography was a form of entertainment, comparable in some ways to the success enjoyed in more recent times by “historical fiction,” which also describe imaginary events in the distant past. But there are some significant differences between the two genres. A distinguishing feature of historical fiction is its self-consciousness; its authors make sure that readers are aware that the narrative they are reading was composed by themselves, in modern times, and that the stories they tell are largely fictional. They include frequent reference to actual events, and provide accurate information about the conditions of life in the times that they describe.

Mythical historiography, by contrast, is presented always as if it were the real thing. Its authors usually prefer not to reveal that they are the inventors of their narratives. Either they are anonymous or, like Plato, attribute the story to someone else, preferably to a dead person or foreigner who cannot be questioned, and set it in a remote or indefinite past, like the authentic myths which it mimics. Composers of mythical historiographers often cite documents that are lost or cannot be easily traced as the primary sources of their information. If they succeed in persuading audiences of the authenticity of the works that they have created, it is not only because their narratives have been crafted carefully enough to avoid detection, but also because their audiences are too credulous and eager to believe what they are told.

Myth vs. Historiography

Mythical historiographies are still in circulation and are sometimes mistaken for scientific historiography, particularly when they purport to describe a distant past or a region of the world readers are unlikely to have seen, and (perhaps most importantly) some aspect of the narrative appeals to the imagination of its potential audience. In effect, if documentation is lacking, as is often the case in the study of the ancient world, writers can always draw on myth to fill in the gaps in their narratives.

A mythical historiography written in the eighteenth century, yet still influential today, is *The Life of Séthos*. Its author was a French priest and professor of Greek at the Sorbonne, the Abbé Jean Terrasson. Terrasson claimed that he had translated into French a manuscript that he had found in a foreign country. Its author was supposedly a Greek living in Alexandria in the second century AD, whose text was supposedly “the private memoirs of the Ancient Egyptians.” According to its “translator” the narrative was an

ancient work of fiction that nonetheless offered considerable authentic information about the ancient Egyptians and Phoenicians.

First published in 1731, the book was soon translated into English and German, and was reprinted in new editions until the early nineteenth century. Any student of antiquity could easily have seen that the work is full of anachronisms, including some comparisons with eighteenth-century French education, which the memoir's supposed author could hardly have known about. But nonetheless the narrative was regarded as a reliable guide to the history and customs of ancient Egypt by the many people who read it, because of the many references to ancient sources supplied in the notes by its translator.

What captured the imagination of many readers was the description he gave of the Egyptian Mystery System, which according to his narrative combined higher education with religion and required a series of initiations which were both emotionally and physically demanding. This Mystery System was supposedly the source of all the philosophy, mathematics, and science that were later said to have been invented by the ancient Greeks.

The notion of initiations into higher wisdom had a wide influence in Europe. Freemasons incorporated some of the rituals described in the book into their own initiations, and adapted versions of the Masonic rituals still survive in the rites of present-day societies and fraternities. Most famously, the libretto of Mozart's opera *The Magic Flute* preserves in outline the narrative structure of the Egyptian Mystery System described in *The Life of Séthos*.

For almost a hundred years, the notion of an Egyptian Mystery System in *The Life of Séthos*, along with the book's account of Egyptian education and religion, was regarded as basically historical, and several editions of the book were published. But once hieroglyphics had been deciphered in 1824, and it was possible to learn about Egyptian religion directly from Egyptian texts, scholars discovered that nothing like Terrasson's idea of an Egyptian Mystery System had ever existed.

Nonetheless, the notion of an Egyptian Mystery System survives, despite a large body of evidence that dispenses with it, and it is still treated as historical by many people. When people choose to believe in the existence of something even though there no proof that it ever existed, the narratives that support those beliefs take on the function of myths, with a quasi-religious value that is characteristic of the genre.

One of the attractions of the myth of an Egyptian Mystery System consists of its plausibility. Because it involves initiations that are connected with education and moral development, it seems both plausible and familiar, particularly because many modern rituals ultimately descend from it. Actual Egyptian religious beliefs, on the other hand, are much harder to understand, because of their complex notions of divine metamorphosis and the varied metaphors their language employs to describe theological concepts.

But the notion of an Egyptian Mystery System also survives because it attributes the development of philosophy and science to ancient Egypt, an African civilization, and not to ancient Greece. That idea has inspired pride among people of African descent, and encouraged them to hold in contempt the ancient Greeks who ordinarily are credited with the discovery of philosophy and of science.

Repeated attempts have been made to demonstrate that the myth is historical. It is estimated that some one hundred thousand copies of *Stolen Legacy* have been published,

a book that presents arguments for the existence of an Egyptian Mystery System, from which the Greeks took all the ideas that were thereafter attributed to them (James 1955). The myth was developed by other writers to include new “facts,” such as the notion that Aristotle went to Egypt with Alexander the Great and stole books from the Alexandrian Library and put his own name on them (Ben-Jochannan 1988: 406, 493–4).

These books have a pseudo-scholarly apparatus of footnotes and bibliography, but many of the works cited do not in fact support the claims made in the book (Lefkowitz 1997). It is absurd to claim that Aristotle stole his philosophy from the library at Alexandria, because the library was built only after his death, and in any case there is no ancient evidence that he ever went to Egypt (Lefkowitz 1997).

Although few academics outside of the field of African-American studies had known of the existence of this account of the development of Greek philosophy, the “Stolen Legacy” myth was briefly given serious consideration on some American campuses. after it was written up and presented by a professor at an Ivy League university. In *Black Athena: The Afro-Asiatic Roots of Classical Civilization*, Martin Bernal, a sinologist in the government department at Cornell, argued that in the nineteenth-century European scholars because of racism and anti-Semitism had systematically sought to cover up the extent of the contribution of Egyptian and near eastern ideas to what is normally thought of as western civilization (Bernal 1987).

Bernal’s arguments impressed academics whose expertise lay outside of the field of ancient studies. He had bona fide academic credentials and his book had been published by a university press. His arguments were presented with considerable ingenuity, with many bibliographic references. Still more importantly, Bernal was *au fait* with the prevailing theories of the time. These included the notion that scholarship and historiographic research was in itself a means of colonizing, in that imposed its own values on what it sought to explain and exploited the past for its own purposes.

If (as Bernal and many others at the time supposed) all narratives could be thought of as fictional, the best one could do was to choose among competitive plausibilities. Once historiography was regarded as a form of fiction, ancient studies could acquire a present-day political relevance. If the narrative of Egyptian priority could be a means of combating racism, anyone who tried to defend the traditional view that philosophy had been developed by Greeks, could be supposed to have been motivated by prejudice or chauvinism.

It took some time for scholars in the several fields Bernal claimed to profess to explain why it was that they could not accept his central thesis. They had to show that they themselves were not victims of a blind orthodoxy, but interested in and willing to challenge both their own assumptions and the evidence upon which they were supported. They had to point out that the central thesis of *Black Athena* had been in circulation since the publication of *The Life of Séthos* and had been preserved anachronistically in Masonic ritual long after the advent of modern Egyptology (Lefkowitz and Rogers 1996).

Bernal characterized himself as a heretic challenging an established orthodoxy, but his role was rather closer to that of a proponent of Intelligent Design defending the biblical myth of Creation against a heterodox and diverse group of Darwinians. He sought support rather than discussion, and refused to believe that his critics were motivated

only by a desire to represent what happened in the past as accurately and dispassionately as they could. As scholars, historians know that their work should be open to discussion, revision, and criticism. They realize that historiography will always need to be rewritten, not only because new information may come to light, but because present-day events may provide new insight into what happened in the past.

The *Black Athena* controversy helps to illustrate some of the characteristics that distinguish myth from historiography. Although both mythical and historiographic narratives are recorded and constructed by human minds, historians will admit that they have given shape to their narratives, and will try to alert readers to the gaps in their knowledge or the possible holes in their arguments. Believers tend to be advocates and are determined to assert their moral authority. They can make myth resemble historiography, in effect, by using the characteristic apparatus of historiographic writing, such as chronological tables, footnotes, and citations. It is to their advantage to keep their readers in the dark about the limitations of their theories.

In discussion the more scrupulous historians will always start at a disadvantage, because myth naturally has more power to persuade than historiography. It is less cluttered and complicated; it can more easily be adjusted to fit the needs of an argument. But history cannot be so easily shaped and organized. Historiographic narratives are often messy and inconclusive because they need to take account of the facts. For that reason they are usually less serviceable than fiction in promoting a particular cause.

Myth is created, but history happens. Historiographic narratives can be shaped by writers, but only up to a certain point. The possibilities open to historians are limited by the evidence. Writers of myths are free to use their imaginations and to provide the connections and motivations that are missing from the historiographic record, from what can be plausibly inferred from extant evidence. Myth provides a shortcut to the understanding of human experience. That is why mythology is always easier to remember and to understand than historiography.

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Historiography and Memory

MARIE-CLAIRE LAVABRE

The question of “memory” is certainly not a new one. It is closely associated with the questions of time, imprints, traces, and traditions and constitutes one of the recurrent themes in both philosophical and anthropological thought (Ricoeur 2000). When understood as an individual function, it remains at the heart of psychological and psychoanalytical investigation. In its purely historiographic dimension – or more specifically in its collective or social dimensions – it constitutes a well-used political tool for the promotion of cohesion amongst nations and peoples (Finley 1975). Nevertheless, the connotations linked to this notion today – when it is defined as collective, social or national – have emerged only recently and are associated with the question of individual or shared identities (Lavabre 1994).

The word memory itself has a history and this history varies, depending on intellectual and national contexts. The theoretical traditions in which the concept has evolved also have an impact on what is understood by memory. In Germany, for example no discussion on memory can take place without referring to the work of Jan Assman whereas little is known of him in France where Maurice Halbwachs and later Pierre Nora remain the reference. Finally, the objects of memory themselves vary depending on time and context, i.e., Vichy France, Nazism in Germany, the question of the disappeared in Argentina, etc. “We are not yet accustomed to speaking of a group memory, even as a metaphor”: This observation, which preoccupied Maurice Halbwachs’ thoughts on “collective memory and historical memory,” underlines at the outset the fact that the concept of (collective) memory itself has a history (Halbwachs 1992 [1950]). This is a concept that is both fluid and polysemous. The question of memory remains at the heart of the discussion whatever the subject may be: Chile, Argentina or post-communist Europe, the war in ex-Yugoslavia or South Africa, France and Vichy, Spain and the civil war, colonialism or slavery; it is debated in Germany every time Naziism is evoked or interpreted. “Memory” is being evoked whether the subjects under discussion are genocides, dictatorships and mass prejudices, the tragedies and fractures of twentieth century history, the disappearance or dying out of rural and working-class areas, local heritage, or regional and national identities, or biographies and autobiographies revealing the historical experiences of renowned or anonymous actors of history.

According to Saint-Augustin, memory is, “the past’s present” (Ricoeur 1983). Nevertheless, the shared preconceptions, which preside over usage of the concept, can barely stand up to the complexity and heterogeneity of the phenomena unanimously called “memory.” Remembrances of past experiences, commemorations, archives, and museums, political movements in history or “the invention of tradition” (Hobsbawm and Terence), monuments and historiographies, conflicts of interpretation, and also memory gaps, symptoms, traces of the past, deliberate re-writing of history, and falsifications: “memory” clearly has many different connotations, it may well be a metaphor (Lavabre 2001). What is memory in the contemporary – and apparently shared – sense of the term? What is collective memory? Is it a trace of the past, the continuity of ancient worlds and traditions? Or is it more frequently the still open wounds of past murders, massacres, and social and political fractures? Does it evoke the past, the glorious or the dark legend, the collective narrative, or political manipulations of the past? Is it an effect of the present or an effect of the past? What is the subject of this memory we call “collective”?

A general synthesis would be unwieldy. Today, it appears that three main conceptual approaches to memory co-exist. The first is that of “realms of memory” (*lieux de mémoire*), a term coined by Pierre Nora; the second is that of “the working through of remembering” (*travail de mémoire*), associated with Paul Ricoeur. The third is that of “frameworks of memory” (*cadres de la mémoire*) which emerged from Maurice Halbwachs’ thoughts on the social conditions of the construction and evocation of memory. Although these three approaches co-exist and often overlap, they were nonetheless elaborated at different periods, are anchored in different disciplines, and focus on different objects.

Maurice Halbwachs can be considered a pioneer in the field. Halbwachs was also a traditional sociologist of social structure, of the standards of living and of the causes behind suicide. His work on memory was rediscovered in France and elsewhere, largely following the trend towards interest in memory studies in the 1970s. Pierre Nora understood memory in terms of realms, distinguishing historiography and history from memory. Realms manifest themselves in discussions of the political uses of the past, of traditions and of national identities (Gildea 1994). They have given rise to specific versions in Germany, Italy, and the Netherlands. Paul Ricoeur examined the working through of remembering, a notion derived from psychoanalysis, most thoroughly. Just like grieving, the process of remembering must be undergone by both society and the individual so that the right memory, the long forgotten event and reconciliation both with oneself and with others might be reached. Although this clearly has a normative dimension and stems from philosophical-political thought (Todorov 1995), it is not alien to Henry Rousso’s previous approach to Vichy (Rousso 1987) or Benjamin Stora’s to Algeria. This approach witnessed an increase in influence in the 1990s as a reaction to the emergence of the equally invasive “duty to remember” (*devoir de mémoire*). In many respects, this approach overshadows that of “realms.” On the one hand, it deals with the demands for recognition from various victims’ groups. On the other hand, it can be harnessed to serve a political desire to settle past scores and to find material and symbolic solutions for conflict resolution. The third approach is sociological. It deals with memory as a social phenomenon and concerns the frameworks of memory. Political uses of the past and strategies for remembrance are sometimes based on the

belief of political and social actors that memory can be influenced. Memory refers here both to memories of experiences which have been lived through or transmitted by people who experienced them, or to collective mythologies. The frameworks of memory reflect on the interactions between uses of the past and memories and examine what the shared representations of the past actually are. They therefore attempt to answer one essential question: can memory be shaped? This question is central to much contemporary thinking on memory.

Apart from the works of Maurice Halbwachs (Halbwachs 1925, 1992 [1950]) and Marc Bloch (Bloch 1925) on socialized forms of the presence of the past and transmission – traditions, memories, “notions,” “teachings,” and “symbols,” which constitute “collective memory” – there are very few articles or books in historiography prior to the mid-1970s containing the word “memory.” At the end of the 1970s, Moshe Finley pleaded that the “political uses of the past” which he sees as legitimate objects of study be taken into consideration (Finley 1975). In 1970, Roger Bastide, a French anthropologist inspired by the theories of Maurice Halbwachs, published his important contribution to the theory of collective memory “Mémoire collective et sociologie du bricolage” (“Collective Memory and the Sociology of Patchworking”) (Bastide 1970). His 1965 study of African religions in Brazil, which contains long passages dedicated to “collective memory” and to Maurice Halbwachs’ thesis in particular, is simply entitled *African Religions in Brazil* (Bastide 1965). When Pierre Nora and Jacques Le Goff – who were soon to become the pioneers of a renewal of French historiography in which memory would be the “spearhead” (Nora 1978) – published a collective three-volume work in 1974, *Faire de l’Histoire (Making History)* on new problems, approaches, and objects in historiography, not one chapter was devoted to “memory.” Another example is Philippe Joutard’s book about the legends and traditions of Protestants in southern France published in 1977. It was inspired by British oral history and was attentive to tradition, to the transmission of memory and to the living presence of the past but it focused on “sensitivity to the past,” not on the study of “memory” (Joutard 1977). France is no different from any other country in this respect. Though the “cult of anniversaries” and the obsession with commemoration have become widespread (Johnston 1992), for a long time, the works of Maurice Halbwachs on collective memory were of interest exclusively to philosophers. Even this was largely because of his controversy with Bergson and the radical character of his thesis which affirmed the logical and chronological priority of collective memory over individual memory (Lavabre 1998).

The historiographic study of memory began to emerge in France only towards the end of the 1970s. Two articles by Pierre Nora from 1978 and 1979 mark this rising interest. The first article offered a definition of collective memory: “As a first approximation, collective memory is a remembrance or series of remembrances, conscious or not, of an experience which has been lived through and/or mythified by a living collective identity of which history is a component part.” The distinction between historiography and memory, or between “historical memory” and “collective memory” allows an independent concept of memory to be formulated. This article underlines the strategic use which historians sometimes make of the notion of memory which itself is “vague and ambiguous.” It heralded his next major work on the *Realms of Memory* in which Pierre Nora introduced a new way of doing historiography. This new way considers conflicts of interpretations, the relativity of historiographic knowledge and

the political uses of the past. Pierre Nora's first article on "collective memory" in 1978 more or less provided a definition of the notion of memory for the following twenty years, particularly in France.

The second article, "*Quatre coins de la Mémoire*" ("The Four Corners of Memory") describes the duel between communist and Gaullist remembrance in France from the Liberation to May 1968. The article offers the following definition: "In France, a memory is what justifies the claim to political strength for those in power, it is an instrument of power in the hands of political manipulators and as such constitutes in itself, a tremendous source of power." The concept of memory, which has both national and political connotations, includes all forms of the presence of the past apart from "critical" and resolutely "contemporary" historiography. Historians came to recognize that historiography is both of the past and of representations and uses of the past (Roussou 1987; Confino 2006). How and why did this approach to memory manage to become dominant? Its success is all the more surprising as the proposed analyses centered on political uses of the past, which often depend on a conception of memories that were handed down from the powers that be and barely took into account the socialized production of memories (as in Halbwachs' work), or society's influence on memory.

The 1970s were marked by social, economic, and political change as well as by a certain nostalgia for a way of life that was disappearing. They were marked by a quasi-militant sensitivity towards those who had been dominated in history, by what was called the awakening of a Jewish consciousness, by the end of existing communities and traditional allegiances in Europe, and by the rise of the post-war generations. In the 1980s, the number of publications on memory in the social sciences beyond historiography exploded. This reflects the desire to analyze living memory, recollection, and transmission, to carry out an audit of the past and to lay down the conditions for reconciliation. All of these elements came together to create a new concept of memory outside conventional historiography, a concepts that is marked by a different approach to history. Historians affirmed that it was their vocation to criticize memory at the very moment when society itself became impassioned by its own past. Social and political movements and interests developed a stake in the historiographic debate about memory. Consequently, the exact meaning of "collective memory" has become contested.

Though the first volumes of *Lieux de Mémoire (Realms of Memory)* contained a certain nostalgic celebration of the past, Nora was explicitly opposed to commemorations. This is demonstrated clearly by the radicalization of the notion of "realms of memory" when it is applied not to the obvious symbols of the French Republic or of the nation but to anonymous places such as the "forest" or the "sea front." This gradual move towards a strictly genealogical meaning of "realms of memory" reveals the essence of his idea. However, it was the use of the concept of memory, with its national and political connotations on the one hand and individual memories on the other, which assured the success of Nora's ideas, though this came at the cost of their misinterpretation (Lavabre 2000). Most notably, the legitimization of a (national) identity or even the very idea of (national) identity remain at the heart of the discussions in France, Germany (Francois and Schulze 2001), and wherever else the concept of realms of memory took root. For the question of identities is indeed present in all inquiries about memory, whether the emphasis is placed on the effects of heritage and history, or on

the individual or social functions of selective reference to the past (Strauss 1992). This is also compatible with the suggestion that the increase in interest in memory since the 1980s does not indicate the vitality of memory but, rather, exposes an anxiety about the future and a crisis in national identities as determined by history (Johnston 1992).

The concept of memory was ambiguous at the beginning of the seventies and today it has become worn out by its multiple uses. The various controversies which have troubled historians over the past few years, together with the type of political conflicts where memory becomes an issue such as in Argentina or Poland, have caused the opposition between memory and history to become apparent once more. In cases like this, there are two possibilities. Either memory is considered misleading and militant by the yardstick of scientific historiography that purportedly offers knowledge of the past, or emphasis is laid on the “duty to remember” and the struggle not to forget what historiography cannot give a true account of. This is problematic because the struggle not to forget, by definition, can only occur when events have not yet been forgotten, the insistence on the “duty to remember” suggests that historiographic knowledge is likely to remain unheeded, unless it becomes memory, which includes shared recollections and representations of the past that cannot be rationally controlled. Conversely, the specifically political pre-occupation with reconciliation or the concern that a “common memory” bears witness to the peaceful resolution of past conflicts are most often expressed in contexts where the memories of the experience are sufficiently strong to resist any attempt to synthesize different interpretations of the past.

Current thinking on “the abuses of memory” (Todorov 1995; Ricoeur 2000) or the obsessive fear of the past are reminiscent of the denunciation of the “excessive historical sense which the present is suffering from” in Nietzsche’s second *Untimely Meditations: on the uses and disadvantages of history for life*. Today as before, the thesis according to which too much consciousness of history is harmful to the present suggests that the value of forgetting should not be overlooked. Nietzsche’s demonstration, however, starts with a reminder of the reasons for referring to the past, and hence why we need historiography: “history belongs to the living individual for three reasons: because he is active and ambitious – because he likes to preserve and to venerate – because he suffers and needs relief” (Nietzsche). Nietzsche writes that to the extent that it is possible to distinguish between them, the three forms of historiography – monumental historiography, traditional historiography, and critical historiography – correspond to this triple relation to the past, to these three useful forms of the presence of the past. These senses of historiography and of the ways of using them merit more attention than other reflections on the disadvantages of the abusive presence of the past and the virtues of forgetting. Indeed, in many ways, these senses cover the meanings of “memory” in its contemporary uses.

Monumental historiography, which has great pedagogical value, is a remedy to resignation. It forms a foundation for believing in cohesion and is based on a heroic vision of a civilization throughout time. It brings together things which are unrelated, generalizes them and declares them identical. In this sense, monumental historiography violates the actual reality of the past and can even be a mere mythical fiction. In this case, Nietzsche argues, history, the past itself, suffers.

Traditionalist historiography belongs to the individual who “looks faithfully and lovingly at his/her origins.” Traditional historiography owes a debt of gratitude to its past. It moves its consumers from individual historiography to collective historiography, an identification with a mythical home, family, or town. However, traditional historiography also recognizes all that is ancient and outdated as equally worthy of respect, and discredits all that is new. Once again, history, the past itself, suffers and is in danger of never forming pure fact. Finally, critical historiography judges and condemns, provides “the strength to break and dissolve a fragment of the past in order to ensure survival.” Although critical historiography, “is always merciless and always unjust as it never stems from pure fact”; it undoubtedly serves the “interests of the living” and, for that reason is favored by Nietzsche. But there is nonetheless a risk that the reality of the past be judged by the yardstick of what is true in the present. Critical historiography can then become illusory and obscure identity, for “as we are the fruit of generations past, we are also the fruit of their mistakes, their passions, their errors, and even their crimes.”

Monumental historiography corresponds closely to Halbwachs’ definition of “memory” as a table of similarities between past and present and, conversely, of historiography as a table of differences between past and present. It echoes back to memory as a “totemic” historiography, in the same way that Pierre Nora opposes it to “critical” historiography. Traditionalist historiography corresponds to what is called memory today, particularly when this notion signifies the preservation of patrimony, and of traces of the past, of museums and archives, of the veneration of all things local and of roots. Just like “memory,” traditionalist historiography finds its rationale in individual and collective identities which have been enhanced by the past. Finally, critical historiography is echoed in contemporary controversies on the uses of historiography, either “judgmental” or “understanding,” and indeed the roles historians adopt for themselves, as suppliers of memories or producers of knowledge. To put it briefly, the three forms of historiography identified by Nietzsche describe different uses of the past. All of these are part of what we now call memory. This means memory as distinct from historiography which if it is not pure knowledge, is at least an intellectual process which strives to render the past intelligible and to avoid anachronism, in other words to create a distance between the past and the present (Lavabre 2000). Memory therefore appears ill-adapted to describe most phenomena expressing the presence of the past, and indeed the “memory moment” as Pierre Nora called it has been so overused that there is an actual desire to do without it. However, memory is holding its own. As a concept, it can hold its own against polysemy, the absence of shared definitions and even confusion. Moreover, as a social phenomenon it holds its own against irate criticism. As a certain level of fatigue with this passion for the past has set in, the “historiography—memory” dichotomy, which was dominant in the first years of the phenomenon, has been substituted by the “memory—oblivion,” dichotomy which is no less problematic – albeit in different ways. It is therefore clear that in spite of criticisms, the phenomenon of memory is unavoidable. The concept of memory must therefore be contextualized and historicized.

At this point, the concepts of the “collective” and the “social” should be re-introduced into the discussion and “memory” specified as an object of study in Sociology or Historical Sociology. Halbwachs’ theses and more importantly the criticisms levied at

these by Bloch and Bastide, provide, if not a perfectly coherent theoretical body, at least essential elements for a sociological and dynamic definition of collective and social memory (Lavabre 1998). The contrast between “cultural memory” and “communicative memory” (Assman 2002), often used in Germany, echoes similar preoccupations. It traces the notion of “communicative memory” to Halbwachs where emphasis is placed not on public narratives of the past or on major collective myths, but on communication between individuals, that ensures the transmission of information from the past. Halbwachs’ original statement contains three propositions. Firstly, the past cannot be preserved, but is reshaped from the vantage point of the present. Secondly, because the isolated individual is a fictional being, the past can only be remembered within the social frameworks of memory where conversely, individual memory only attains reality as a constituent part of collective memory. Finally, memory has a social function. From the outset of Halbwachs’ thinking in his early work, *Les cadres sociaux de la mémoire* (the Social Frameworks of Memory) these theses justify the notion of “collective memory.” Consequently, the definition of collective memory wavers continually between the idea that stresses the group as such, and the idea that, on the contrary, stresses the individuals who make up the group and embody the collective memory. There is a certain paradox in the fact that contemporary uses of the notion of memory have often only retained Halbwachs’ first idea of collective memory. They thus have only considered the political uses of the past which themselves reveal a political desire to organize representations of the past and nothing else. Bloch however, in his vigorous critique of the “finalism” and “anthropomorphism” of Halbwachs’ first statements, drew attention to the “communication factor between individuals” that constitutes collective memory (Bloch 1925). Bastide notes that even if Halbwachs never managed to separate himself from “the idea of a collective conscience both exterior and superior to individuals,” the texts that make up *On Collective Memory* clearly demonstrate the “interpenetration of consciences” and allow memory to be seen as a “meeting point.” The change of direction that took place within the notion of collective memory resolutely privileging the group’s point of view rejects the idea of collective memory as transcending individual memories. The opposition between the individual and the collective thus becomes a kind of reciprocal influence between groups and the individuals who compose them. Memory is said to be collective not because it is the memory of the group as such, but because the collective, or the social, is the state in which individuals exist. It should be added that any reflection on this interaction must take the multiple loyalties of individuals into account.

This broad outline indicates that beyond the often-highlighted gaps in the “collective memory” theory and beyond Halbwachs’ sometimes-dated observations, *On Collective Memory* is infinitely more valuable than the caricature frequently made of it in fossilized uses of the theory. “Collective memory” is not necessarily expressed in more institutional or political uses of the past. However, the question of the impact of social conditions on the production of shared representations of the past, or public or authorized historiographic narratives, which often give meaning to individual memories, remains pertinent. Moreover, one could argue that it is precisely the question of remembered experiences – and their transmission – which at the end of the day is posed when the question of memory is raised whether this be to demand justice or to express concern for reconciliation.

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Historiographic Schools

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The Concept of “Schools”

The present and past of historiography is often presented in the literature as a pattern of clusterings of writers and their key concepts into what have become known as “schools” or “traditions” or “discourses” or “networks,” or “approaches” of thinking about how to write historiography. Within this literature these terms have appeared in a somewhat unexamined way. The complex relationships between history, philosophy, and historiography has, at least in many and various accounts by historians of historiography (such as Thompson 1942; Collingwood 1946; White 1973; Breisach 1983; Kelley 1991; Iggers 1997; Bentley 1997; Burns and Rayment-Pickard 2000), given rise over time to a dense, changing pattern of clusters of thought.

Clusters that are variously called “schools,” “traditions,” “discourses,” “approaches,” and “networks” of thought (hereafter all called “schools”) seem to be ubiquitous in the history of ideas generally (not just historiography), at least as revealed by students of the history of ideas (cf. Collins 1998). That is, intellectuals apparently rarely have been isolated individuals without some sort of group affinity that situates and influences their thinking. Indeed, it’s a truism that intellectual thought (indeed, all thought) always depends to a large degree upon prior and related contemporary thought. The history of thought is an *evolutionary* process. But it is a further step to argue that intellectual thinking more or less always occurs within “schools” of one sort or another. The reality, nature, and cohesiveness of “schools” as evidenced in the history of historiography is an open question, which is the topic of this chapter.

In the history and sociology of ideas there is, then, a (usually implicit) meta-claim that a pattern of clustering is the consequence of some sort of deep structure within critical and creative intellectual thought. There seems to be reason to think that the deep structure is one that necessarily produces a non-linear pattern over long periods. The pattern seems to be the consequence, at the deeper level, of the forms of affinity or disaffinity that thinkers and their ideas have with others and as such is an integral part of the structures of all social relatedness. That is, the history of ideas cannot be separated from the history of societies in both the senses of the mutual interconnectedness of intellectual and social relational structures and the historical processes that all such social structures undergo.

Therefore, the historiographic study of ideas is really the Historical Sociology (or Social History) of ideas or knowledge. This has been argued, at least, for some time, especially since the influence of Marx and Weber became strong in the late nineteenth and early twentieth centuries and further extended by writers influenced by both of them, such as Karl Mannheim, Thomas Kuhn, Michel Foucault, and Randall Collins through the twentieth century. The question for Mannheim was about the social conditions or causes of knowledge production. Later in the twentieth century, social epistemology diverged from this kind of work to ask a slightly different question: is knowledge to be understood individually or socially? That is, is all knowledge socially relative or somehow transcendent of all specific social contexts of its production? (Schmitt 1994: 1). Thomas Kuhn (1962) famously argued, influenced by older Quinean notions, for the power of paradigmatic frameworks of presuppositions within which thinking, theorization, and research always takes place. Kuhn's view was that there was a progressive transition between pre-science and sciences in certain fields as a consequence of revolutions in thought that bring about paradigmatic frameworks for knowledge. But his revolutions were of course premised by a history that made the revolutions possible. Imre Lakatos articulated a similar concept of progressive research programs as the hallmark of sciences as distinct from non-sciences (Lakatos 1970). Foucault's view seems to have been (at least at one time) that there are marked epistemic breaks between discourses and no history of development or evolution between them. The strong program in the sociology of knowledge in the 1970s and 1980s, associated with Barry Barnes (1974, 1982) and David Bloor (1976), argued for the social relativism of all knowledge and therefore for the social determination of the history of ideas. The constitution of schools of thought and the evolution of ideas, including revolutions in ideas, have to be traced, in this view, to the social relations of intellectuals and thinkers of all kinds. Karl Popper's argument, on the other hand, was for a semi-independent realm of thought that, somewhat akin to the Platonic concept of ideal forms, undergoes an evolutionary process of its own as an exosomatic form of human creativity and development (Popper 1972; cf. Fuller 1988, 2003).

Two main organizational principles or contentions seem to be central, then, in the literature on the history of historiography: first, that ideas and intellectuals have been and are clustered and this is a consequence of deep social and intellectual forces; second, that these clusters or intellectual constellations or schools have undergone a history. Not so obvious and not universally shared is a third contention, that the emergence of the modern physical, biological, and geological sciences from the late nineteenth century, in symbiosis with powerful technologies of scientific inquiry and engineering that enabled the capacity to uncover the hidden causative structures of the natural world, began a wholesale break from the "normal" history of ideas of all previous thought. Within this contention, the "mature" sciences, once they are "mature," are viewed as having dispensed with the chaotic non-linear history of "schoolization" and entered an altogether different trajectory of historical development. But even in the mature sciences there are forms of intellectual affiliation between scientists that are not entirely linear, not developed solely within empirical scientific research. That is, social networks and social power structures retain a degree of significance in the sciences but these networks and structures are not strongly determinant of the clusterings of affinity *à la* schools in scientific thought generally.

To state it another way, schools are not a phenomenon of the history of sciences because certain fundamental philosophical issues and modes of reasoning are agreed upon, unlike in non-sciences. Disagreements in sciences are about detailed aspects of theory, research techniques, and significance of research results, and not about fundamental issues of philosophy. These disagreements do not arise from prior clusterings of scientists into schools. There is a research “community” within particular sciences unlike in the history of ideas in all non-science discourses. One of the hallmarks of science is the subjection of beliefs about the fundamental nature of being and knowledge to self-scrutiny. Of course, not all scientists do this as a matter of course and basic assumptions are widely taken for granted but such self-awareness of basic commitments is part of the fabric of science in a way that no other forms of discourse have. Ideally science has a bootstrap approach to establishing, criticizing, and building upon its own foundations. Those foundations do not come from some external discourse or authority (cf. Shapere 1984). The equivalent in historiographic inquiry would be for historians collectively to self-criticize their understandings of the nature of social relations, the nature of human thinking and motivation, the structure of human communication, and the nature of social evolution. Instead, we find widespread ignorance about the significance of these issues and a falling back on the timeworn cliché of “common sense,” which is an *a priori*, untestable, and ideological commitment.

Thus much of the history and philosophy of ideas of recent times has indeed concerned the differences between scientific and non-scientific traditions. This is a discussion that was intimated by Vico, Kant, and Hume in the eighteenth century and then taken further by Marx and Mill in the mid-nineteenth century, extended by the Neo-Kantians in the late nineteenth century and given further powerful impetus by Empiricist Positivists in the mid-twentieth century. The terms of the current debate were largely set by the Vienna Circle of Logical Positivists and other loosely associated thinkers in the 1920s and 1930s. A key issue throughout has been whether it is possible to ground indisputably any epistemology on which to build any system of ideas and approaches to the study of any phenomena, historical, physical, intellectual, or human. Can historiographic inquiry be grounded somehow definitively? Should we understand the history of historical writing as reflecting such a long-running debate about knowledge? If so, the fundamental nature of “schools” is to be found in the sharing of common elements of a philosophical kind that cannot be grounded indisputably. These elements are implicit *a priori* ontological beliefs, epistemological assumptions about the knowledge-generating process, and corresponding methodologies of inquiry. Schools sharing these sorts of commitments have indeed been found in the histories of many discourses, such as biology and geology in the late eighteenth and early nineteenth centuries, when proto-sciences began to abandon biblical received wisdom, throughout the history of economics, and throughout the history of social inquiry.

Historiographies of historiography (representations of the history of historiography) are similarly typically constructed around such an organizing principle of real “schoolization.” The works of Collingwood, Thompson, Breisach, and Iggers shows this clearly. Differing presuppositions of a philosophical and methodological kind, deriving from socio-religious and nationalist ideologies and increasingly supported by rationalist and idealist epistemologies, prevalent in western civilization at different epochs, have given rise to this history. However, in historiography and other areas of humanities

there is an added dimension to the debate, which concerns the role of humanistic interpretation. In the sciences and proto-sciences there developed a shared emphasis upon the development of objectivity and the removal of subjective interpretation. In historiography the nature and role of interpretation of evidence and of what counts as evidence is a central focus around which points of view have long coalesced. The complex relationship between meta-concepts, the nature of evidence, the subjectivity of evidential searches, the adducement of evidence, and the interpretation of meaning of both evidence and of the place of humanity in the socio-historical and universal schema, are all still central to historical writing. Of course, many humanistic scholars argue that no field of the socio-humanistic studies can be objective in the way that the natural sciences claim to be and that interpretation remains the central methodological task for all the humanities. Against that, historians who are strongly influenced by and use general theory and quantitative data series see no difference in philosophical principle (but differences in practice and results) between the social sciences and the natural sciences. This is the big divide in historiography of modern times. The question of theory is central to this debate but “theory” is used in two distinct ways – one denotes the idea of “theory of knowledge,” that is, the problem of how historians know about and understand the past; the other denotes the idea of conceptual generalization about reality and causation as found in the generalizing social sciences. While this difference persists, the debate is bound to be somewhat unengaged (cf. Lloyd 2005).

The existence of *schools* of thought in the classical Mediterranean, medieval Islamic, and medieval European worlds (and probably within other ancient and medieval civilizations) was in varying degrees the consequence of the establishment and maintenance of real physical schools in the sense of monastic-like establishments in which students were taught and inculcated into the ways of thought of the prevailing outlook of the particular school as passed on through successive generations of teachers (cf. Collins’ 1998 large-scale study of this phenomenon). This phenomenon of “real” schools has persisted in important respects to the very present. Even today we find the ascription of “school” to clusterings of thinkers and writers associated with particular locations, especially particular university departments and city precincts. The modern phenomenon of schools in which groups of historians began developing “professionalized” approaches to historiography based upon detailed explicit understandings of how to be a member of the profession with well developed training processes and accreditation of professional standards emerged in the late nineteenth and early twentieth centuries. Guild-like organizations developed to institutionalize and maintain standards. The twin phenomena of professionalization and schoolization grew together in places such as Berlin, Cambridge, and Harvard.

In the early twenty-first century, as global communication in all its forms has dissolved most of the barriers to intellectual contact, the question of schoolization has taken on a new significance. The maintenance of “real” schools seems less and less significant while the persistence of philosophically based “virtual” schools seems enhanced. Recent debates and “historiography wars” about postmodernism, post-structuralism, critical realism, culturalism, general theory, forms and use of evidence, the use of historiography in nationalistic disputes, and so on, have involved many of the features of school-like commitments of a “pre-scientific” or “pre-disciplinary” kind and have ranged across the whole globe. Individuals are inculcated into and become

committed to particular affinity groups, based upon such commitments, via communication with people locally and in widespread locations. Historiographical debates are more alive than ever.

The clustering of thinkers into schools, then, is a consequence of philosophical disjunctures, assumptions, and commitments, and social networks or structures of power. The possibility of disagreements about fundamentals makes schools possible. Once that possibility disappears – once sciences emerge out of the hegemony of particular powerful and successful research programs that produce undisputed results via empirical and/or technological and social applications and when sciences have developed highly successful general theories about which fundamental disputes are no longer possible – then schools can no longer exist within such fields. Schools are possible, even necessary, when beliefs about the chief causal characteristics and possible understandings of the nature of the world are indeterminate, lacking any undisputed foundation of warranted true belief.

The process of teaching, learning, and mentoring is perhaps the fundamental force making for school formation. Forming or joining a school of thought resembles conversion or the joining of a religious community, followed by an inculcation process in which commitments are made based not mainly upon seemingly irrefutable evidence or tried and trusted shared techniques (although these might play some role) but upon a mixture of rational choice, persuasion, *a priori* beliefs, aesthetics, and affiliations of power and social relatedness. Lying beneath these is the pre-existing configuration of modes of thought inherited within each socio-ideational era. There are both a “path dependency” and a social process of influence and commitment that determines the development of schools. When we examine these contentions and the schools that contain the different approaches we see that they are indeed based on presuppositions of a non-consensual, non-scientific, often pre-rational, ideological kind (Lloyd 1993). Such prior commitments determine and drive the debates between schools.

Main Schools of Historiography

Modern historiography in the sense of the careful, analytical examination of its own presuppositions and the emergence of consensus, beyond local traditions that were firmly embedded within local cultures, began in Germany at the beginning of the nineteenth century. German universities were the most advanced in the nineteenth century in the senses of training of scholars via the Ph.D. degree and the development of research culture as part of both the liberal university ideal and the emergence of a system of scientific research that became the norm in all western countries in the twentieth century. Arguably, the first modern school was the famous Berlin or Rankian School that powerfully coalesced around, and influentially spread out from the work of Leopold Ranke in Berlin to encompass the whole historiographic profession internationally. The Rankians were a genuine “school” in the sense of the commitments, locations, connections, and lines of power and influence between the members and across generations (Thompson 1942). The success of this school arguably turned it from a nineteenth century school to a twentieth century paradigm since Rankians founded their approach upon a commitment to the use of and privileging of publicly

available documentary sources and national historiographies. These principles became orthodoxies. The neo-Rankeans developed an explicit nationalist political project of identifying historiography with political historiography, international relations, and the life and times of great statesmen and great states.

The social sciences or studies in most forms, except for economics, were most developed in Germany in the mid to late nineteenth century so it was no accident that philosophical and methodological disputes about the proper methodologies for the emerging social sciences and by implication Social Science History were strongest there. The *methodenstreit* began first in economics over the issue of inductive and historiographic methodology (as evinced by the German Historical School of economists and historians) versus deductive and abstract methodology as supported by positivist economists. The Neo-Kantian opposition (associated most with Dilthey) to positivist abstraction and scientism emphasized the necessity of the method of *Verstehen* – empathetic understanding – for the human studies in order to discover the inner feelings and motivations of historical actors. On the other hand, the Marxists argued for a different kind of scientific approach than those of either the Rankians or the positivists – one that strove to discover the deep and historical causal structures of human social structuring agents in a manner analogous to the new biological and evolutionary sciences of the mid to late nineteenth century. Max Weber attempted to bridge the divides between the generalizing theorists of the Historical School, the abstract economists, the *Verstehen* methodology, and the historical materialism of the Marxists, by emphasizing the necessary roles of both generalization and interpretation.

The socio-human studies bifurcated after the debate into two broad streams – one trying to establish in various ways the scientific credentials of their approaches, the other emphasizing the unavoidability of interpretation, hermeneutics, and common sense. The attempts of the French *Annales* School and the Weberians to construct new socio-historiographic syntheses achieved widespread support in the 1950s and 1960s but they too failed to achieve the unification of the socio-human studies. The works of Marx, Weber, Foucault, Bourdieu, Habermas, Geertz, and postmodernists such as Derrida have all recently provided powerful frameworks that have offered persuasive approaches to historiographic inquiry but no consensus has emerged. In the early twenty-first century we see a panorama of historiographic approaches and “schools” clustering within a field in which the attractors are provided by six axes, as in figure 33.1, all of which draw upon more than a century of philosophizing and theorizing about historiographic and social methodology. At a higher or more meta-level we can see that the whole field tends to be divided into two broad “traditions” or “historical outlooks” as indicated by the clusters above and below the central horizontal line.

Looking down more closely at the field we can make out several loosely defined “schools.” The objectivist/structuralist side was dominant for most of the twentieth century. The most “organized” and coherent school has been that of the Marxists who cluster around such fundamental concepts and methodological postulates as “social class,” “revolution,” “ideology,” “structure-agency causation,” “material interests,” and “Historical Social Science.” Similarly, the *Annales* historians (most notably Braudel and Le Roy Ladurie) strove to unite concerns with structure on a grand geographical and historical scale with small-scale local human agency in order to uncover the multiple hidden layers of social structure and *mentalité* – to write “*histoire totale*.”

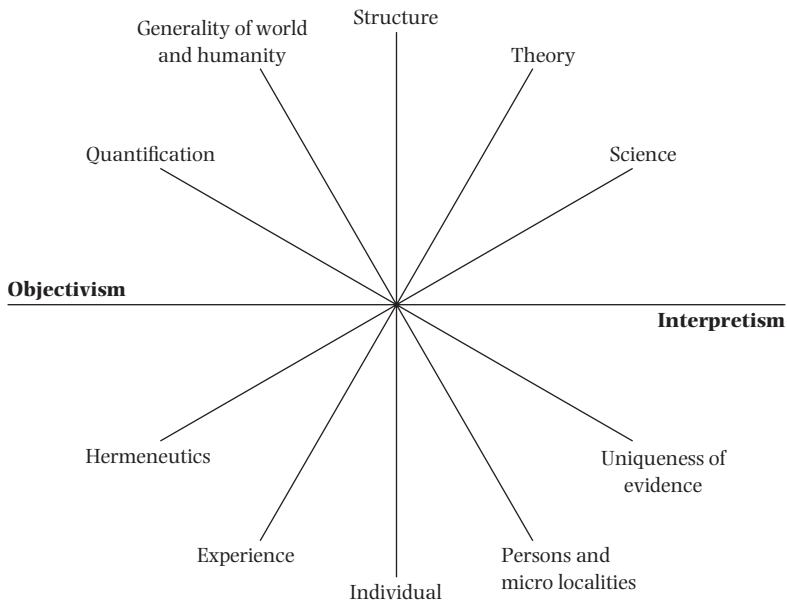


Figure 33.1 The two broad traditions of historical inquiry

The importance of regional and national mentality was given a central place in their work and this became very influential on later historians who did not necessarily share their concern with grand structural continuities beneath the surfaces of everyday life.

The “Historical Sociology” school of the anglophone world, at its height in the 1950s to 1970s, was most influenced by Weber and to a lesser extent Durkheim in its research into the history of social structural change, such as modernization and the rise of industrial societies. Like other similar schools, it pursued a close relationship between theory, research, and quantified data and consciously attempted to build a unified historiographically oriented social science.

This is also the case with Economic History, which has a long lineage from the work of Adam Smith in the mid-eighteenth century and then the adoption of evolutionary ideas in the nineteenth century in Germany and England. Marx became influential in the early twentieth century. However, a distinct new school of Economic History emerged in recent decades out of a convergence between modern orthodox economic theory, quantitative methodology, and the desire to explain the present by examining the past. The older school of Economic History, now in the guise of “Historical Political Economy” influenced by Marx, Weber, and institutionalist theory, has effectively been separated. These “outsiders” have recently combined with other streams of thought to form a new school “Historical Political Economy” that unites Evolutionary Theory with Marxism and Institutionalism.

In the 1960s and 1970s, especially in the anglophone world and those areas most influenced by it, including Germany, there arose a new, relatively coherent and synthetic school of Social History. Influenced by Marxist and Weberian ideas, by Historical Sociology and Old Economic History, and in opposition to orthodox nationalist political historiography conducted “from above,” the New Social History strove to

re-orient historiography to a “bottom-up” approach. They aimed to rescue all the forgotten social classes and to restore the significance of everyday life. Likewise, the women’s history movement, an offshoot of New Social History, aimed to feminize the concerns of all historians. Quantification and emphasis upon social structure played important roles in many aspects of the New Social History, at least to begin with (cf extensive discussion in Sewell 2005: ch. 2).

The most recent developments within the objectivist/structuralist side of the great divide are those of Environmental History, World History, and Big History, drawing upon influences from all the prior structuralist social science approaches and from the natural sciences. These practitioners see their task as explaining synthetically the whole history of societies and environments over very long time scales, using a variety of theories and concepts. No clear philosophical and theoretical foundation, apart from intellectual desire to explain the whole of history as structural change, holds these historians together.

The 1970s and 1980s debates over structuralism, post-structuralism, historicism, and postmodernism, sparked partly by the work of Foucault and other theorists of the centrality of language in social life, but also having deep roots in anglophone historiography stemming from the power of English so-called “common sense” as the sort of “received wisdom” of the English empiricist tradition and opposed to European grand theorizing, had several consequences for historiography. Perhaps the main one was the crystallization out of Social History in recent decades of a broad “Culturalist School” of historiography around themes of “bottom-up” *interpretive* inquiry into numerous neglected and marginalized areas of social expression and micro sites of cultural life, such as women’s history, children’s history, indigenous history, the history of manners, the history of death, the history of unofficial sub-cultures, and so on. There is often an explicit rejection of national historiographies. The sort of evidence relied upon is often oral and artefactual rather than official. Central elements of this school’s approach include use of personal testimony, interpretation of non-documentary evidence, concepts of socio-linguistic power, rejection of quantification and generalization, emphasis upon individual uniqueness, and Micro-History. Theories and ideas from anthropology, linguistics, and cultural theory have played important roles.

Towards a Theory of the History of Historiography

Few philosophers of historiography have concerned themselves explicitly with the meta-question of why historiography has had the structure and history that have been described in such detail in the works of Collingwood, Thompson, Butterfield, Kelley, Breisach, Iggers, Bentley, and others. Indeed, some, such as Kelley (1991), have argued that fundamental aspects of historiography have remained essentially unchanged down the millennia while other aspects have changed markedly. The question of how and why historiography has changed over time is bound up, of course, with the general issue of the interrelatedness of the histories of ideas, knowledge, and societies. The theoretical and empirical study of the history and sociology of disciplines, sciences, discourses, and knowledge is now a large and flourishing area of

inquiry. As with all areas of the socio-humanistic fields of inquiry there is no scientific agreement about general concepts and explanatory theories that constitute an agreed scientific framework although there are certain themes that could be seen as the beginnings of such a consensus.

It is no longer sufficient for historians of historiography to fall back on externalist explanations that invoke nationalism, progress, and culture to explain their subject matter. Schools of ideas evolve; the questions are how and why? We can identify several candidate causes in this multi-causal picture. First is the nature of *homo intellectualis*, who, being a sub-species of humanity, seems to be powerfully motivated by a concern about moral careers and influenced by forces of social respect, deference, subservience, and domination, to form social relations that bind individuals into hierarchical social groups across generations. Second, is the influence of wider socio-economic-cultural milieux that set the contexts for modes of thought and conceptualization. Third, is the path dependency of the evolution of ideas – one can think only what the available conceptual/ideational materials permit. A discourse about the complex relationship between general theories, hermeneutics, cultural concepts, and interpretative strategies in the methodology of historiographic inquiry was not possible until most of those conceptual tools were at hand from the mid-to-late twentieth century. Their formation has not been a linear process. The history of concept formation and the schools through which concepts have been developed and articulated has been a contingent historical, branching, diverging, and re-combining path. And it seems clear that in the general sense of critical self-awareness of methodology and concept formation, historiography, especially from the nineteenth century, has followed a developmental path. From this it could be concluded that historiography is a progressive field of empirical inquiry on both the conceptual and empirical levels and new developments linking various approaches together, especially via the “Social Science History” and “World History” movements, are promising to break down the old divisions.

Will schools continue to exist? Schools are a feature of all fields of intellectual inquiry except for the advanced sciences. Those who are sceptical about the possibility of scientific historiography reject its possibility on the ground of the illimitable nature of human individual and social experiences (cf. Ankersmit 2005). From this perspective, the subject matter of historiography is past *experience*. The subjective relationship between the historian and the experience of past actors, mediated by various forms of evidence, cannot be generalized into a set of theoretical concepts. In this case, then, “schools” in the sense of particular approaches to describing, analyzing and, moreover, *interpreting* evidence, will always exist. On the other hand, the *project* of developing general and even scientific knowledge of the past drives inquiry in the direction of trying to develop general concepts and causal explanations of human motivation, behavior, consciousness, and relationships. The concept of scientific historiography that new, richer understandings of the possibility of socio-bio-behavioral science makes possible, is one in which the general and the particular inform each other in a non-deterministic manner. Boyd and Richerson’s (2005) evolutionary socio-biology and Foucault’s (1978) concepts and theory of “biopower” and “biopolitics” are further bridges between the science of the socialized biological realm and the socio-political experiential lifeworlds of humanity.

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Part IV

Classical Schools and Philosophers of Historiography and History

Leopold Ranke

THOMAS GIL

Leopold Ranke is the historian universally recognized as the founder of modern scientific historiography. Ranke was, indeed, the professional historian who applied the scientific method to historiography, giving thus a decisive new direction to his own discipline and, what is more, plotting a new role for historiography in modern culture.

Ranke propounds a scientific approach to historiography, based upon the critical study of sources. He developed a method for such a critical study that allows the use of evidence provided by the sources to discredit distortions and to isolate their origins and infer true descriptions of the past. He exemplifies himself his methodical scientific historiography in a series of works dedicated to the history of the chief European nations between the sixteenth and eighteenth centuries and culminating in a climactic and long anticipated attempt at a universal history. Ranke's oeuvre comprises fifty-four volumes of a yet incomplete edition of his collected works. Ranke devised the educational institution appropriate for the transmission of his new science: the "historical seminar," in which students could practice the new critical historiography under the supervision of their teacher.

According to Ranke, the scientific study of history could only thrive if philosophical speculations about history came to an end. Speculative philosophers of history like Hegel had dedicated themselves to eliciting the rationality of what had occurred. They firmly believed that reason is sovereign of the historical world, and that history, therefore, presents us with a rational process only to be grasped philosophically or speculatively. Against this speculative intellectual background, Ranke wanted to find out "what really had happened," what actually had been the case. Nevertheless, his own historiographic work was carried out on the basis of certain substantive assumptions that came close to the main assertions of the philosophy of history he criticized. For the mature Ranke, historiography was ultimately a harmonizing medium that allowed the reconciliation of man's inner life and the external world, the particular and the universal, necessity and freedom, subjectivity and objectivity, spirit and nature.

Scientific Historiography

Leopold Ranke was born in 1795 in the small rural Thuringian valley town of Wiehe. His family was deeply religious and Protestant, descended from a long line of Lutheran

pastors. His father turned from the ministry for which he was destined to the profession of law and civil service. After having attended the secondary school at Pforta, where he became acquainted with the ancient classical authors and where he acquired a passion for the literary arts, he studied classical philology and theology at the universities of Leipzig and Halle from 1814 to 1818. He then became a teacher in Frankfurt/Oder and accepted a professorship in 1825 at the University of Berlin where he settled. He was appointed official Prussian state historian in 1841.

As a professional historian, Ranke combined three pre-existing methods: a critical attitude toward historical sources; the insistence upon original documents; and the application of the philological method to the writing and teaching of historiography (Tucker 2004: 46–91). The critical attitude toward historical sources dates back to the first Greek historians. Ranke's doctoral dissertation at the University of Leipzig, now lost, was a study of Thucydides. The insistence upon original documents was in the tradition of humanist scholarship since the fifteenth century. Barthold Georg Niebuhr had spectacularly exemplified the application of the philological method to historiography in his studies of Roman history. Ranke explicitly acknowledged Niebuhr as his mentor. Ranke created a paradigm that could be handed down to an entire profession as its distinctive collective identity. Ranke's own achievement consisted of having written a large series of books showing how the paradigm worked in practice. He explained in short marginal notes, comments and reflections how he practiced the method, how he criticized the sources and inferred reliable historiography from the evidence.

The scientific historiography Ranke himself practiced in his works and reflected upon in his methodological observations is characterized by four main principles: the objectivity of historiographic truth; the priority of facts over concepts; the uniqueness of all historical events; and the centrality of politics. Each of these principles was immortalized by Ranke in a memorable formulation traditionally transmitted from generation to generation in the historiographic community.

The historian's objectivity means that the historian should not judge the past. Neither has he or she to instruct the present for the benefit of the future. This is a grand rejection of moralizing. Such high offices are not to be assumed by historians. History has simply to show "what actually happened" ("wie es eigentlich gewesen" – SW 33/34, VII), the famous formulation in the introductions to Ranke's main works, in his *History of the Latin and Teutonic Nations, 1494–1514*, in his *French History*, in his *English History*, and in the *History of Prussia during the 17th and 18th Centuries*. To achieve objectivity the historian must immerse in the historical object and grasp its inner necessity and the law it carries within itself. In his *English History* Ranke declared that he had tried to extinguish his own self to let the things speak and the mighty forces appear without any subjective deformation (SW 15, 103). The first Rankean principle implies the autonomy of historiography against possible pragmatic subordinations to moral, political or social interests: an ideal Ranke himself was not always able to live by.

The primacy of facts ("die strenge Darstellung der Tatsache") is the most effective prescription for the historian's work. It means a return to the sources, to the evidence provided by them, and the decision not to make abstract theory. The historian has to get to know and to present the facts as they are. He has to abstain from philosophical speculations. Strict presentation of the particular events and facts, even if that is unattractive and dull, should unquestionably be the supreme law in historiography,

which cannot imitate philosophical procedures of abstraction and generalization. Historiographic knowledge is documentary, not speculative. Historiography's medium is the document, not the conceptual construction. Historiography is concerned with the particular, individual, not the general and universal.

The uniqueness of all historical events as a principle and normative prescription for the historical profession is the logical consequence of Ranke's methodological insights. All historical units and forms like events, processes, revolutions, and evolutions, are unique and individual. They have their value in themselves, and not, as philosophers of history tend to assume, from the totality of the historical process. Each epoch must be seen as something valid for its own sake, and not from what may result from it (presentism). As an individual, autonomous epoch, each epoch is worth of consideration. "Jede Epoche ist unmittelbar zu Gott" ("Every epoch is immediate to God") became the maxim under which historians united against philosophers of history who, believing in the idea of progress, tended to subordinate some epochs under other pretendedly more prominent epochs (Ranke 1971: 54ff, 59ff, 64ff). Ranke protested against such a philosophical subordination making the case for a specific profession of historians who intellectually participate in the particular, individual, and enjoy it in and for itself. The mature Ranke betrayed this tenet in his commitment to the ideal of universal historiography.

Ranke's scientific historiography was focused on past politics and states considered to be "ideas of God." Such a historiography was, fundamentally, "political" historiography. Ranke did not neglect social and economic factors, but, in his works, they were seen and located in the framework of a political history. This focus on the political dimension of history and the corresponding conception of states as the primary units of history came about as a consequence of his own philosophical view of the historical process and on the basis of the documents Ranke as a historian mainly consulted: political and diplomatic documents centered on state's actions found in state archives.

These four Rankean principles make up the theoretical structure of his specific version of "Historism." Ranke found appropriate, easily transmissible formulations for each one of them in a series of what Leonard Krieger (1977) has called "theoretical pronouncements" mostly dispersed in the introductions to his major books. In his own historiographic practice, Ranke combined them flexibly. But he did not always remain faithful to them. As theoretical principles they are useful as definition of the normative poles around which Ranke himself and many other nineteenth-century German historians revolved. The principles implied the critical method and the devotion to factual accuracy which had been developed by earlier generations of historians, philologists, classicists, and Bible scholars. In their combination with a series of basic convictions in regard to the nature of historical individualities and the state's central role in history, they became the disciplinary matrix or paradigm of German nineteenth-century scientific historiography.

Substantive Assumptions

Karl R. Popper described critically in his book "The Poverty of Historicism" a philosophical view of history according to which historical prediction is possible due to the fact that

there are discoverable “rhythms,” “patterns,” “trends,” and “laws” underlying the evolution of history. Hegel was for Popper one of the main representatives of such a wrong approach to human practical reality. Hegel, like all other classical philosophers of history, affirmed the rationality of the historical process, which he conceived as teleologically directed, having a main goal, namely progress in the consciousness of freedom. He conceived of different historical phases and periods as well as the whole of history as the manifestation of what he called the development of the self-positing and self-realizing Spirit. In his lectures on world history entitled “Die Grundzüge des gegenwärtigen Zeitalters” (“The Main Traits of the Present Age”), Fichte, like Hegel, had also presented the ultimate goal and the main epochs of universal history. The ultimate goal of world history was determined by Fichte as the collective establishment of a rational culture (“Vernunftkultur”), a culture in which Reason (and not Understanding “Verstand”) will reign supreme.

Ranke conceived the historians’ task and profession in direct opposition to such speculative, philosophical constructions of world history, that affirmed the existence of historical laws and the teleological orientation of the whole historical process. Yet, Fichte’s romantic ideal of the “blessed life” and the special “Nature of the Scholar” inspired the young Ranke. Like Fichte, Ranke thought the academic calling was a sanctified task. The scholar has a mission to accomplish, comprehending and representing the divine idea and its vitalizing function in the world of appearances. But Ranke, unlike Fichte, would never affirm that fulfilling this mission is the philosophical explanation of the “concept” of history from which Fichte could deduce in his lectures history’s ultimate goal and its concrete, evolutionary realization.

The difference between the philosophical and the historiographic approach to history had also institutional consequences, specifically at the University of Berlin, which was divided into two hostile camps. One camp centered around Hegel. The other camp included a broad group of jurists, historians, philologists, and theologians. The jurists Friedrich Carl von Savigny and Karl Friedrich Eichhorn, the historian Barthold Georg Niebuhr, the philologists August Böckh, Franz Bopp, Karl Lachmann, and the theologian Friedrich Daniel Ernst Schleiermacher belonged to this second camp. The main cause of their division was their different concepts of truth and reality. For the philosophical camp, historical diversity was merely a manifestation of an underlying rational principle. Truth could, consequently, be attained by reducing this diversity to rational concepts, or by interpreting it as an expression of Reason’s development and self-realization. For the camp of historians, the philosophical reduction to conceptual schemes was a violation of the fullness and individuality of historical life. Both camps shared nevertheless the firm conviction that behind the phenomena and events of history, there was another reality, and that the aim of all academic study was the apprehension of that reality. Niebuhr, Savigny, and Ranke could agree with Hegel that in the long run philosophy and historiography coincided. However, they differed from Hegel in their deep conviction that such a transcendent reality could only be approached through historiographic research, which is much more suitable than philosophy to its complex, vitalistic, spontaneous, unique, and elusive character. Historiography was for them the only true way to knowledge of humankind’s spiritual condition, because it alone could recognize the value and autonomy of each epoch and each historical phenomenon without improperly subsuming them under a general linear process of fulfilment.

A bitter controversy between Leopold Ranke and Heinrich Leo, a young disciple of Hegel, on the interpretation of Machiavelli's work illustrates the difference between the philosophical and the historiographic standpoints (Iggers 1968: 66ff). Leo had reviewed Ranke's "Geschichten der romanischen und germanischen Völker" ("Histories of the Latin and Teutonic Nations") and its methodological appendix "Zur Kritik neuerer Geschichtsschreiber" ("On the Critique of Modern Historians") accusing Ranke of a poor style, and of having introduced sentimentality into his narration. Ranke replied in the "Hallische Literaturzeitung" (SW 53/4, 659–66) criticizing Leo's treatment of Machiavelli's work, whom Leo had judged by moral standards and as a "world-historical" person. Ranke thought that it is not the historian's task to judge the past. Rather, the historians should focus on the more humble challenge of showing "what really happened." Ranke recognized that there was something quite shocking in Machiavelli's teachings. But he interpreted them as means used for a specific situation, and urged that they should be understood as such. The conditions of corrupted Italy seemed so desperate to Machiavelli that he was bold enough to prescribe poison to save it. Ranke disagreed with Leo's philosophical application of ethical standards to the assessment of historical characters, and to studying historical personalities in terms of their role in world history. Ranke wanted to study historical personalities for their own sake, not passing moral judgments upon them, and trying to understand them in their uniqueness and individual particularity. The "Histories of the Latin and Teutonic Nations" (the title significantly in the plural!) appeared to the philosopher Leo to resemble a heap of unassorted details, his author having done little to seek the general within the particular, and to grasp the "world historical significance" of his own subject matter.

The dispute between the historiographic and the philosophical camp at the University of Berlin would be misunderstood if one concluded that Ranke was non-theoretical, non-philosophical, politically neutral, soulless positivist historian, who conceived scientific historiography as a technique that applied critical methods to the evaluation of sources. Ranke was not exclusively concerned with historical facts, rejecting all theoretical or philosophical foundation of historiographic practice. Ranke rather approached the theoretical problems underlying his historiographic practice mainly during the four years of editorship of the "Historisch-Politische Zeitschrift," between 1832 und 1836, and in a series of random remarks and observations scattered through his historiographies and correspondence. In the brief introduction to the private lectures "About the Epochs of Modern History" ("Über die Epochen der neueren Geschichte"), which he read to King Maximilian of Bavaria in 1854, and in his inaugural lecture as a professor in Berlin in 1836 "On the Affinities and Differences between Historiography and Politics" ("Über die Verwandtschaft und den Unterschied der Historie und der Politik"), Ranke developed a series of general, philosophical ideas on historiography very similar to those defended by the philosophical side.

Ranke's substantive "philosophy of history" is, therefore, not systematic and consistent. Some of his propositions are occasional statements, tailored to the requirements of the particular situation or to the individual character of his interlocutor, for instance, on the great powers ("Die grossen Mächte," 1832), on politics ("Politisches Gespräch," 1836), or when modifying his own exposition under the questioning of the Bavarian king.

Like the philosophers of history, Ranke believed in a divine purpose which he associated with world history. He was committed to world historiography (especially in his old age), and postulated the idea of a developmental totality, axiologically superior to the individual entities, with “eternal ideas” and “laws,” unknown to us, but nevertheless governing the appearances of the infinite variety of developments inherent in human affairs and, in general, humankind’s destiny. The late Ranke could even see a universal and developing pattern in the actual history of man, a continuous general process. Continuity between past and present was consequently for him fundamental. Such continuity allowed the application of certain insights extracted from the verifiable past to the present, and demanded politically a sense of moderation.

The Meaning of History

Philosophically speaking, Ranke’s position was much closer to Hegel than he would have admitted. Like Hegel, he saw a deeper reality behind historical phenomena. He interpreted these historical phenomena as concrete expression of a general spirit and objective order hidden in the individual events. The historian’s task was for him to become an outlet of that general spirit, and to present the concrete phenomena in such a way that the general order could be intuitively perceived. What distinguished Ranke was his insistence that knowledge of the objective order can be attained only through careful study of individual facts, which must never be approached in abstract concepts, and his firm conviction that the plan of the universe is beyond man’s grasp, so that man can only divine its outlines. The intuitive perception or divination of history’s spiritual meaning required for Ranke, more than philosophical or conceptual work, it necessitated artistic means.

The philosopher attempts to subsume all life under a unifying concept to reach a deeper spiritual reality, but misses that spiritual reality at the very moment of his intervention. The historian can elevate himself to this spiritual plane by proceeding from the condition of existence and fully respecting the individual. The task of historiographic understanding begins for Ranke always with thorough immersion in the individual subject matter, with exact research, step-by-step apprehension, and the humble study of the documents, approaching like this the spiritual essence through acts of intuition, and never through conceptual devices. Historiography then resembles art. It elaborates perceptive, vivid, and imaginative portraits and descriptions of individual characters and particular historical constellations. It does not deliver the concept of the totality, of divine providence, but stimulates a feeling of the whole, an intuitive knowledge of it (“Mitgefühl, Mitwissenschaft des Alls” – SW 53/4, 569). Historiography therefore cannot concentrate exclusively on the merely empirical and actual, getting lost in the details, as was the case in Niebuhr’s historiography. As a hermeneutic effort and discipline, it has, for Ranke, to try to understand the whole, in an act of “geistige Apperception” (“spiritual apperception”). Only then, the material process of history can appear as a “Hieroglyphe Gottes” (“God’s secret writing”), and the historian can describe the dominating trends and governing ideas (“herrschende Tendenzen” and “leitende Ideen”) that make up its real texture and continuity.

Historical continuity is, for Ranke, not only the basis and subject matter of the historian's grasping effort, but a precious political good, never to be neglected. The "Historisch-Politische Zeitschrift" had been founded to defend the policies of the enlightened Prussian bureaucracy against its numerous liberal critics on the left and to distinguish the position of the Prussian government from that of the reactionary right, which had the *Berliner Politisches Wochenblatt* (*Berlin Political Weekly*) to propagate Karl Ludwig von Haller's feudal doctrines. Ranke took on the editorship of the *Historisch-Politische Zeitschrift* between 1832 and 1836. He conceived his task as one of keeping equal distance between the extremes of the conservative *Berliner Politisches Wochenblatt* and of liberalism. But his criticisms were directed almost entirely at the liberals who, in his opinion, willing to push forward several political reforms did not appreciate enough historical continuity. Ranke adhered to the conservative status quo, which rests on past experiences and traditions. He wanted, at the same time, to open up a future in which one would be able to do justice to new ideas, and the new social trends, but without breaking with the past. The liberal reformers were in his eyes fascinated by abstract principles, which they wanted to blindly apply to politics. Their approach to social and political institutions was, according to Ranke, based on abstract theory, and not on concrete realities and continuities. Against such abstractness, Ranke intended to use historiographic knowledge, and scientific historiography, which actually could show that all institutions and ideas have valuable historical roots. Historiography alone could help to understand existing and dominant trends, preparing appropriate political decisions. Historiography alone could present specific diversities and differences, demonstrating that what was effective and good in a specific context must not necessarily be good in different and new contexts. The elimination of differences and diversity would kill the living reality of concrete human existence.

For Ranke the study of history was the best way to understand human nature. Historiography grew for Ranke to become a perspective on all of reality. Further, historiography became the only perspective from which the opposing principles of life could be perceived in their constructive interactions. Through historiography, the professional historian obtained access to the contradictory nature of man and practical (social and political) reality. Constructive interactions of different principles that sometimes contradict, at other times complement, each other, are in Ranke's opinion the subject matter of historiography. Historiography is for Ranke about interactive connectedness, the connection between past and present, the individual or particular and the universal, necessity and freedom, man's inner life and the external world, national and world politics. Historiography helps to render comprehensible manifestations of humanity that would otherwise remain incomprehensible. It helps to understand how diverse and heterogeneous motives, ideas and actions can coexist with each other. Historiography's logic is the "logic of the actual . . . when such heterogeneous relationships cannot be understood by the logics of either propositional thinking or the analytical sciences, they may still be manageable by a kind of thinking that makes sense of arranging things, however incongruous in themselves, along the time line" (Krieger 1977: 356). For Leopold Ranke, historiography became a harmonizing medium, the field where polar opposites get reconciled. God and the world, spirit and nature, religion and culture, the ideal and the real, feeling and understanding, the general and the individual, the universal and

the local, the present and the past, all came together in historiography, the master science.

Still under the influence of Fichte's idea of the intellectual vocation, the young Ranke had tried to show in his Luther fragment how the divine idea manifested itself on earth, i.e., in history, as an invisible life force graspable through the tangible opposites it vitalized. Ranke's original concern with Luther had been triggered by his interest in language and literature. In the course of writing the reformer's biography, Luther became the character who could illustrate how inner spiritual life could express itself in the external world. Luther was the spiritual individual who could vitalize the external world, activate the eternal in the world. Luther's conflicts and contradictions were conceived by Ranke as natural manifestations of the world's structure that placed restrictions on the desirable relations between the spirit and the empirical reality.

In the *Histories of the Latin and Teutonic Nations from 1494 to 1514*, the multiplicity of the contents (royal descriptions, diplomatic negotiations, military activities) and the copiousness of what really happened contrasted sharply with the intended unity of the project, which Ranke never had ceased to proclaim. Ranke himself conceded defeat when he confessed in the preface his failure to combine the two dimensions of his approach, the unity of what happened and the multiplicity of facts and events. The "Histories" illustrate tensions and contradictions in the historical field, in which unity and diversity interact with each other in forms not easily apprehended.

Ranke had always advocated the reconciliation of the national and the universal in historiography. In the *History of the Popes, German History of the Age of the Reformation, French History, Especially in the Sixteenth and Seventeenth Century, English History, Especially in the Seventeenth Century*, and in the series of private lectures he delivered in 1854 "On the World-historical Epochs of Modern Times," Ranke was able to combine the particular with the universal perspective, intelligently showing how both perspectives need and presuppose each other. The national appears in all these works intelligible only in connection to the "world-historical," and this "world-historical" is embedded in the local intricacies of national history. In the preface and introduction to the first volume of the *English History* Ranke left no doubt that he was writing from the perspective of universal history, and that this "world-historical" perspective was what made the relation between the present and the past historiographically significant. World historiography was to be Ranke's last project and perspective, not theoretically demonstrated or deductively inferred by conceptual means, but narratively presented and historiographically elucidated. Thematic universal realities were present in actual history. This was Ranke's firm conviction. The historian's task, accordingly, could only be to apprehend them in the particular constellations of actual history, and to show their indispensability for the comprehension of what really happened.

In his private lectures "On the World-historical Epochs of Modern Times," Ranke summarized his views on world history and the historian's task. Historians cannot "subsume" the complex historical process under "concepts" as philosophers usually do. They have to respect the diversity of history, and to accept that the laws behind everything that happens are unknown to them (Ranke 1971: 66ff). They can describe the so-called "leitende Ideen" ("leading ideas"), which Ranke prefers to call "dominant trends" ("herrschende Tendenzen"). However, they cannot conceive history as a

“logical process,” as philosophers tend to do, affirming an independent life of the “Idea” or “Spirit,” in which individuals have no other role than the role of mere instruments for the realization of the Spirit’s plan. In Ranke’s view, that would lead to “pantheism.” Historical progress, which Ranke would not deny, is always to be specified. There is, indeed, material, scientific, and technical progress. However, in relation to cultural, moral, and spiritual history Ranke avoids applying the concept of progress, and prefers to affirm the individuality and uniqueness of each epoch, its intrinsic value, and its “immediate relation to God.”

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THOMAS GIL

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Scientific Historiography

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The idea that historiography is or should be a science, founded on the critical research of documents, is connected to the processes of institutionalization and of professionalization of historiography as an academic discipline in the late eighteenth and nineteenth centuries. The origin of this idea is usually ascribed to the founders of the so-called “Historical School” in Germany, like Barthold Georg Niebuhr (1776–1831), Wilhelm von Humboldt (1767–1835), and Leopold von Ranke (1795–1886), but the idea has also been formulated outside Germany. In France, for instance, Fustel de Coulanges (1830–1889) stated in his inaugural lecture in Strasbourg in 1862 that “history is and should be a science.” Forty years later in England John B. Bury (1861–1927) made the very same point in his inaugural lecture, entitled “The Science of History” stating that though historiography “may supply material for literary art or philosophical speculation, she is herself simply a science, no less and no more.” Karl Marx (1818–1883) too considered it a “science,” although he had a very different idea of what “the science of history” looked like.

Claims to the “scientific” status of historiography have been debated and criticized for as long as they have been made. This situation had not changed when in the twentieth century many claimed that historiography is – or should become – a “social science.” I focus on three of the constitutive ideas of practically all advocates of “scientific” historiography: “truth,” “objectivity” and the “critical method,” concluding with a discussion of the “comparative method” whose fortunes fluctuated with the popularity of at least one version of scientific historiography. I do this on basis of what *historians* have been debating and arguing for rather than on the basis of what the *philosophers* have stated about the topic.

We may approximate the meanings of “scientific historiography” by examining first what its advocates contrasted it with. The most widespread type of scientific historiography, of the Rankean–Humboldtian variety, excludes literature, speculative philosophy of history (especially in its Hegelian form), and Enlightenment philosophical or conjectural historiography, though both Ranke and Humboldt acknowledged that historiography contains an irreducible poetic element. Scientific historiography was conceived of as professional historiography, not as the business of amateurs, or later of journalists, nor of other people susceptible to partisanship.

The “founding myth” of Rankean scientific historiography was its claim to describe the past “as it really was” (“*wie es eigentlich gewesen*”) and to be *beyond* any form of partisanship; that is, to be objective. This combination of a reality claim – implying a truth claim of historiography, in contrast to all fictional genres – and an objectivity claim – implying a claim to intersubjective validity in contrast to all non-scientific genres – has been characteristic of “scientific” historiography ever since.

According to the same “founding myth” historiographic objectivity was institutionally safeguarded by the impartial state that pays its historians and thus releases them from economic dependence on partisan interests, as had been the case in ecclesial and court historiographies. The identification of scientific historiography with objectivity was implicitly connected to a political theory of the impartial state that included the assumption that state archives were the primary storehouses of impartial information for scientific historians.

It is thus not accidental that scientific historiography and the institution of centralized state archives have developed hand in hand in the post-Napoleonic period: the archive was soon regarded as the historian’s only true workshop. Accordingly, those historians that would later reject the theory of the impartial state, ranging from the “Prussian School” in the later nineteenth century and all other proponents of explicit nationalism in historiography to the proponents of Marxism and “critical theory” in the twentieth century, have all rejected the idea of historiographic objectivity.

The same logic explains why historians critical of the idea of the impartial state, especially those working in postcolonial and subaltern studies have recently deconstructed the idea of impartial state archives, containing impartial state documents as the raw material of scientific historiography. Criticizing the very idea of impartiality as an unrealistic and ideological concealment of power relations, these historians subscribe to Foucault’s theory that the production of “knowledge” always takes place in specific power relations and corresponds with “truth regimes” – in contexts of supra- and subordination. The colonial setting was a pure specimen of such a power relation and Foucault’s theory thus is being used to undermine the supposed impartial archival basis of scientific historiography.

The same Rankean “founding myth” requires *distance in time* for the scientific historian to write objectively because it takes time for partisan interests to decay and for the principal political actors to die out. Most historians regarded 50 years distance to be the absolute minimum for “hot” history to “cool down” and to transform into “cold” history, but 100 years to be safer. So just like Hegel – whom they usually portrayed as their antipode – scientific historians subscribed to the idea that truth develops – or “unfolds” – in time. This conception of historical time has only recently become the object of reflection.

Contemporary historiography had therefore been considered for a long time a *contradictio in adiecto*. It was explicitly excluded from “scientific” historiography well into the twentieth century. Only in the aftermath of the Second World War and the Holocaust did contemporary historiography slowly gain recognition as a legitimate specialization of scientific historiography, manifested by professorships, peer reviewed journals, and institutes of its own. After 1945 scientific historiography could no longer require a substantial distance in time, because twentieth-century history was experienced by many as so catastrophic that it required immediate handling.

Significantly, however, historians had not considered important forms of partisan bias as impediments to writing scientific historiography until well into the twentieth century. In the case of Ranke and Humboldt, for instance, subscribing explicitly to the Christian religion and a Christian worldview was not regarded as being partisan and as threatening objectivity, nor was subscribing explicitly to the cause of the (German) nation/state regarded as such by most of the neo-Rankeans later on, nor their common pure Eurocentric worldview. The very same held for scientific historians outside Germany, so furthering the cause of the nation/state, being patriotic and Eurocentric has been regarded as being compatible with striving after objectivity – well into the twentieth century. This kind of partisanship came even more natural to those historians who identified their nation with *one* specific religion, like Protestantism in the Scandinavian states and Catholicism in Poland, Ireland, or Spain. The same held for class partisanship in Marxist historiography. In a similar vein, subscribing explicitly to a male worldview had not been regarded as a form of bias and partisanship until gender historians called attention to this fact since the 1970s. Scientific historiography thus has been plagued by serious blind spots in its identification of biases and interests.

These submerged or repressed political dimensions of scientific historiography have provoked three types of criticisms. The first type is the “apolitical” rejection of historiography’s claim to “scientificity” based on the argument that history is a form of literature, as we find in Jules Michelet, Thomas Carlyle, and Golo Mann. The second type is the political “presentist” position, which rejects the “scientificity” of historiography by the argument that “all historiography is politics,” as we find in Friedrich Nietzsche, Charles Beard, Michel Foucault, Hayden White, and in “postmodernists” like Keith Jenkins. The third type is the political neo-Enlightenment position which acknowledges the political character of historiography but calls for its “rationalization,” as we find in Jürgen Habermas, Jörn Rüsen, and Emil Angehrn. Thus fundamental criticism of the claims to “objectivity” and to “impartiality” have been accompanying “scientific” historiography from the very start.

Theory and Method in Historiography: Some Preliminary Distinctions

To clarify further the notion of scientific historiography, it is necessary to introduce some basic distinctions. There are theories that deal with the characteristics of history as a process such as, for instance, Ranke’s idealistic theory of history as an emanation of God’s ideas (Ranke 1867–: vols. 33/4, 665; vols. 49/50, 8), or Marx’s materialistic theory of history as class struggle, or social Darwinist theories of history as “the survival of the fittest.” Then there are theories that deal with the characteristics of the knowledge of history, such as Ranke’s theory that scientific knowledge of history has the form of concrete particulars and Marx or Comtes conception of law-like scientific knowledge of history. We call theories of the first type substantial or material theories of history, while theories of the second type are cognitive theories of historiography, i.e., theories of historiographic knowledge. Cognitive theories of historiography can be subdivided into epistemological theories and methodological theories.

Substantial and cognitive theories have often been interrelated, because presuppositions about *what* can be known about the past are linked to presuppositions about *how* the past can be known. Substantial theories that hold that the historical process is characterized by underlying causal mechanisms and regularities, analogous to theories of classical mechanics, have been interrelated with cognitive theories that posit that scientific knowledge of history implies knowledge of general laws (this position is usually labeled as positivism). The substantial theory that the historical process is *not* characterized by causal mechanisms, but by a fundamental contingency of all events has been interrelated with cognitive theories that posit that real knowledge of history is knowledge of particular facts instead of general laws (this position is usually labeled as *Historismus* or historicism).

Substantive theories not only have consequences for methodological positions – generalizing versus individualizing ideas of method – but also for epistemological positions, that is, positions concerning the validity of knowledge claims. Substantive theories that posit some kind of affinity or even identity of the knowing subject and the historical object, such as Hegelian idealism and its offshoots in Droysen’s and Dilthey’s versions of historicism, have usually been interrelated with methodological theories that posit a distinct kind of method for the human sciences – such as *understanding* or *Verstehen* or *interpreting*, in contrast to the *explanatory* methods of the natural sciences. These methodological theories have, in their turn, usually been interrelated with distinct epistemic theories, linking methods of understanding to intentional and hermeneutic theories of knowledge, and methods of explanation to empirical theories of knowledge.

A Short History of the Historiographic Method

In history, Jean Bodin first introduced the term historiographic method in a systematic way in the sixteenth century in his treatise on source criticism, *Methodus ad facilem historiarium cognitionem* (1566). The historiographic method was the method of *source criticism*, and this is the etymological origin of the “critical” historiography. Characteristically, Bodin’s treatise intended to establish the ways by which reliable *knowledge of the past* could be established intersubjectively by checking written sources against each other and by assessing the reliability of the information conveyed by them, relating them to the time and place of production and to the interests involved. The origins of the method of source criticism are in philology. Its hallmark is the intersubjectively controllable *footnote*, because as Anthony Grafton has argued “only the use of footnotes enables historians to make their texts not monologues but conversations, in which modern scholars, their predecessors, and their subjects take all part” (Grafton 1997: 234).

The critical method was intended to defeat skeptical doubts with regard to the possibility of reliable knowledge of the past. Descartes in his *Discourse on Method* (1637) had formulated this doubt in an exemplary way, where he compares historiographic knowledge to a “multitude of different opinions,” concluding that historiography was no match for certain mathematical knowledge based on deduction. Pierre Bayle in his *Historical and Critical Dictionary* (1696) was the first to develop a systematic critique of Cartesian skepticism in relation to historiographic knowledge by formulating rules that

verified or falsified historiographic propositions, just like Descartes had done for mathematical propositions. Bayle had insisted that “concrete” certainty was possible in historiography, though this certainty is different from the mathematical variety – anticipating Collingwood’s later arguments in his *Idea of History* (1946).

Skepticism concerning historiographic knowledge had been fed by the large quantity of forgeries parading as original historical documents, primarily produced by competing churches and nobles to back up claims to rights and property. The case of the *Donatio Constantini*, proven a forgery by Lorenzo Valla in 1440, is a famous example. Therefore, the traditional claims to impartiality, truth, and trustworthiness of historians, dating back to classical Antiquity, were rarely believed at face value.

According to the classical model going back to Thucydides (460–400 BC) it was the duty of the historian to stick to the facts and to tell the *truth*, the *whole* truth, and *nothing but* the truth. Contrary to poetry, historiography dealt with *res factae* and not with *res fictae*. This Aristotelian model had received its canonical formulations in Cicero (106–43 BC) and in Lucian (ca. AD125–200), who also had linked historiographic truth directly to *impartiality* by avoiding moral blame and envy (*sine ira et studio*). Ranke reproduced these classical ideals in the nineteenth century, but added the impersonal critical method by which historians could expose forgeries. The basic idea of the critical method, already originating in the Renaissance and in the humanist tradition, is to detect or to reconstruct the original version of a text, because all text variants (in whole or in fragments) are derivative and secondary to the original one.

This philological idea of the original text is known in historiography as the *primary source* or *document*. Johann Christoph Gatterer (1727–1799) and August Ludwig von Schlözer (1735–1809) at Göttingen University were the first to apply it systematically to history in the eighteenth century. Nevertheless, the later Ranke became associated with the introduction of source criticism to historiography in popular memory. Since Ranke, scientific historiography is conceived of as historiography based primarily on primary sources, at least in theory, because even Ranke did not stick to this own rules, as Heinrich Leo and Anthony Grafton have shown (Grafton 1997: 62–72). The link between the primary source and the statements based on them was established by the footnote.

The importance attributed to the primary source by Ranke stems from its identification with first-hand knowledge of an *eyewitness*: “I can see the time approach when we will no longer have to base modern history on reports, even those of contemporary historians – except to the extent that they had first-hand knowledge – to say nothing of derivative reworking of the sources. Rather we will construct it from the accounts of eyewitnesses and the most genuine and direct sources” (Ranke, cited in Grafton 1997: 51). Although Ranke of course knew that individual sources or documents could sharply contradict each other, he thought that every document widens the point of view of the historian and enabled him (not yet: her) to be more impartial and thus objective.

As he subscribed to the theory that the state archive was the primary and impartial repository of documents for historiography, Ranke implicitly promoted specific classes of documents as “transparent windows on past states and events rather than colorful reconstructions of them, whose authors wrote within rigid conventions, had not heard or seen everything they reported, and often wished to convince their own

audience of a personal theory rather than simply to tell what happened" (Grafton 1997: 59–60). Unlike contemporary philosophers of language and science, Ranke did not acknowledge that *all* observation statements are *theory-laden*, nor did he conclude from this fact that the historian must always decide *which* of the "observation theories" fits the evidence best.

Ranke did not reflect on the troublesome fact that the elimination of forgeries does not by itself produce "truth" – let alone the whole truth – nor on the relationship between what has been recorded in documents in state archives and what had *not* been recorded. The "silent voices" – and the "silenced voices" – in history had to wait till the 1960s before "symptomatic silences" and systematic repression were recognized as a problem for scientific historiography. The Holocaust – including the "problem" of eyewitnesses who can no longer testify to what they have experienced – has been the trigger for this new awareness in scientific historiography.

The direct *relationship between historiography's claim to truth and its critical method* would characterize all later treatises on scientific historiography, although what was meant by method changed over time. In the eighteenth century, Enlightenment historians like Schlözer regarded not only the source critical techniques as *the* historiographic method (*Forschung* or research), but also the way the results of historiographic research were synthesized into a textual whole (*Darstellung* or composition) and presented to an audience. This broader conception of method had its roots in the classical rhetorical tradition. The philosophy of historiography would rediscover it only in the 1970s. The German historian and theorist Johann-Gustav Droysen has become one of the pivotal historians behind this rediscovery of the textual and rhetorical dimensions of historiography, because his treatment of historiographic methods in the earlier and the later versions of his *Historik* embody the transition from the broader to the narrower conception of historiographic method.

Most of the nineteenth and twentieth centuries, however, were dominated by Ranke's narrower research-oriented conception of the historiographic method, limiting the methodical character of historiography and its truth claims to source criticism. Canonical formulations of this conception are: E. Bernheim's *Lehrbuch der historischen Methode* (1889) and Ch. Langlois and Ch. Seignobos', *Introduction aux études historiques* (1897). The latter echoed what Ranke had stated earlier on: "L'histoire se fait avec des documents" ("historiography is made with documents") and "pas de documents, pas d'histoire" ("no documents, no historiography").

The historiographic narrative, the *Darstellung*, was no longer regarded as methodical and truthful (*wahrheitsfähig*), but as the literary, artistic, or aesthetic component of historiography. Therefore, the question whether historiography belongs – partially or whole – to the "sciences" or to the "arts" has been accompanying "scientific" historiography from its very beginning and the question whether narratives can be called "true" or not has become part of the continuing *Methodenstreit*.

Critical Method and Its Discontents

In the nineteenth century, the historiographic method usually meant the techniques of conducting historiographic *research* of texts, the *sources*. These philological techniques

were traditionally subdivided into three parts, corresponding with three subsequent steps in research: *Heuristics*, the techniques for locating the relevant sources; *source criticism*, the techniques by which the temporal and spatial origins of the sources are established as well as their authenticity; and *interpretation*, by which the cleansed information derived from the sources is put together by means of interpretation in order to infer “what really happened.” The distinction between *primary* and *secondary* sources was crucial for historiography’s claim to be a science. Primary sources consist of the written material produced more or less contemporaneously with the object of research, ideally recorded by eyewitnesses; official records of parliamentary debates are a prime example. The time-span between the recording of primary sources and what they record should be as short as possible, because distance in time was regarded as a major source of corruption of the information conveyed. Secondary sources consist of records written a considerable time *after the fact* and not by eyewitnesses; hearsay testimony is a prime example. As the information in secondary sources is based on *other* sources, it is, at best, second hand; therefore, it is regarded as less reliable than primary source information.

Another traditional distinction in historiographic method is between *internal* and *external* source criticism. *Internal* source criticism is actually *textual* criticism, i.e., the philological procedures by which a text is inferred or reconstructed when it is preserved in an incomplete form or in several variants, and the textual procedures by which authorship and textual authenticity are checked and its meaning established. *External* source criticism consists of those procedures critics use to check a source against information derived from *other* sources – written and material. The so-called *auxiliary sciences*, such as paleography (the study of writing), chronology, toponomy (the study of etymologies of the names of places), numismatics (the study of coins), and diplomatics (the study of official documents), usually play an important role in this phase. When modern historians are dealing with non-scriptural, oral cultures, or with cultures that left few or no documentary traces, archeology and anthropology often function as auxiliary science.

According to the nineteenth century concept of scientific historiography as documentary, preferably archive based, non-scriptural or “low” cultures, were primitive *by definition*, lacking both a state and its archives, and thus lacking a history comparable to that of “high” cultures, susceptible to scientific investigation. The exclusively documentary conception of historiographic evidence defined and limited the object of scientific historiography to the history of “high” cultures, especially to the political and religious history of the west. Only in the second half of the twentieth century did these Eurocentric biases in the very conception of the historiographic method come under criticism by postcolonial theory and “subaltern history.” Before that time “history from below” had attempted to study the histories of the “lower” classes and of women, and of other societal groups that usually left no archived records themselves.

Since the 1970s, the notion of the impartial state archive has been deconstructed as the place where specific information is *constructed* under specific power relations instead of just being impartially stored. Under the influence of Foucault and Derrida, Manoff has summarized the recent deconstruction of the archive as follows: “The methods for transmitting information shape the nature of the knowledge that can be produced. Library and archival technology determine what can be archived and therefore what can be studied. Thus Derrida claims ‘archivization produces as much as it records the event’ ”

. . . “Government documents may prove useful, but not necessarily because of their accuracy or objectivity” . . . “All conservational decisions are contingent, temporary, and culturally self-referential, even self-laudatory: we want to preserve the best of ourselves for those who follow” (Manoff 2004: 12, 15, 20).

Only in the course of the twentieth century did reference to *multiple methods* within *one* discipline become the rule, thereby reopening the problems of truth and objectivity and paving the way for a new wave of anti-realism and epistemological skepticism in historiography. For if “the” method could no longer be regarded as *the* procedural guarantee for the truth and objectivity of the discipline, what could? Its practitioners, therefore, have often regarded recognition of plural methods within one discipline as a threat to the discipline’s scientific credentials and as a symptom of an epistemological crisis. This experience of being “threatened” was based on the positivistic idea that “normal” science is characterized by one paradigm and by a methodological and by epistemological consensus.

From the late 1960s onwards, this process of methodological differentiation within all of the human sciences took an unprecedented pace, leading to a poly-paradigmatic fragmentation of most disciplinary fields and a questioning of most traditional disciplinary boundaries and self definitions. This also held for historiography, which many regarded in the 1960s and 1970s as one of the social sciences leading to the founding of an academic movement of “Social Science History.”

The Comparative Method as the “Royal Road” to Scientific Historiography?

In this poly-paradigmatic situation since the 1960s both the quantitative method and the comparative method have been repeatedly suggested at times as the best way to restore historiography’s scientific glory, sometimes as separate methods, but usually together. These pleas are usually continuations of arguments developed earlier in the twentieth century especially by Marc Bloch (1886–1944) in France and Henri Pirenne (1862–1935) in Belgium, and by Max Weber (1864–1920) and Otto Hintze (1851–1940) in Germany. For both Bloch and Pirenne comparison was also a method for breaking out of the national framework usually taken for granted in historiography, thus anticipating many recent arguments in favor of transnational approaches such as “global history.” At least since Max Weber, the comparative method has been proposed as the royal middle way between the individualizing method of historicism and the generalizing, nomothetic methods, or at least aspirations, of positivism. Although it had turned out to be impossible to discover empirically substantiated “universal” or statistical laws that hold for the whole of history, comparative historians and social scientists have demonstrated the possibility of producing general statements about temporally and spatially restricted ranges of historical events in the form of empirical generalizations, middle-range theories, ideal-types, models, mechanisms etc. They can be formulated in either quantitative or qualitative forms. Moreover, only the comparative method allows for confirmed statements about the specificity and generality of historical phenomena and for an empirical justification of the attribution of causes. From an epistemological point of view, comparison therefore

is one of the main methodological “reality checks” available to historians – and the broader this “check” is done, the stronger is the empirical support for the knowledge claims involved.

From a political point of view, the comparative method has often been presented as a most effective way to “neutralize” political and cultural biases of historians. Some of the more sensitive minds in the historical profession, like Henri Pirenne and Marc Bloch, recognized political bias as one of the chief problems of scientific historiography. After the First World War had shown how easily scientific historians could transform themselves into overtly nationalist historians, Pirenne and Bloch sought the solution to this problem in *comparative* historiography, which they saw as the only mean to “correct” the national biases and the nationalist myopia of “scientific” historians. They propagated the comparative method as the cure to both the cognitive problems of “single case” historiography *and* to the political biases of national historiographies. In the words of Henri Pirenne: “The comparative method alone can diminish racial, political, and national prejudices among historians” and “The comparative method permits history in its true perspective” (Henri Pirenne in Meyerhoff 1959: 98–9). This creed was also formulated by Marc Bloch who – together with Lucien Febvre (1878–1956) gave birth to the so-called *Annales*-approach, which after the Second World War also spread widely outside France.

From the 1960s onwards, a new generation of usually social science-inspired historians forcefully advertised comparative historiography following the arguments put forward already by Pirenne and Bloch in the 1920s. Although “Comparative History” became a growth industry for a while – including specialist journals like *Comparative Studies in Society and History* – as in the interwar period the comparative method failed to “convert” the majority of scientific historians who usually remained working within one single national framework.

After the 1980s, “New Cultural History” and “New Narrative History” took their inspiration from the “linguistic turn” and emphasized the textual aspects of historiography. They forced the comparative method on the defensive again. Simultaneously, the New Cultural History rejected the generalizing social sciences as models for historiography. It focused on the deep study of single historical cases, as in “micro-history” and the “history of everyday life,” although it did borrow some of its methods and models from anthropology.

In an intellectual climate strongly influenced by postmodernist relativism, “collective memory” and “sites of memory” have turned into the new central categories of historiography in the 1990s. Since in this new climate the fragmentation of historiography and memory is taken for granted, the possibility of truth and objectivity in historiography are seen as outdated, and “ideological.” Therefore, the idea of scientific historiography has met strong cultural countercurrents in recent times. Nevertheless, scientific historiography is still being defended against its two traditional competitors – the “literary” and the “political” conceptions of what “doing History” is about. As before, epistemology is pitched against skepticism and comparative methodology against “anything goes,” albeit with more sophisticated arguments than before the challenge of postmodernism.

Philosophical defenders of scientific historiography, under the influence of postmodern positions varying from social constructivism to discourse analysis, have

become conscious of the radically linguistic and constructed character of past and present reality. Naïve realism has yielded its place to more sophisticated variants of realism making it possible to reinterpret all the fundamental notions of scientific historiography as constructed – facts, narratives, sources, and archives – without yielding the field of historiography to idealism – in its Collingwoodian, Gadamerian, Berkhoffian, and Ankersmitian variants – or to epistemological skepticism – in Hayden White’s or other postmodern variants. Against idealism and skepticism, scientific historians argue that only they can explain why historiographic narratives unlike all fictional narratives contain a truth claim and an objectivity claim, which are debated on the basis of evidence; why scientific historiography is necessarily and not accidentally connected with the critical method, based on the twin notions of evidence and the footnote; and why scientific historiography claims intersubjective validity for historiographic knowledge.

In response to the challenge of postmodern positions, practitioners of scientific historiography had to rethink their epistemological presuppositions and commitments, including their notions of truth and objectivity. Truth cannot be conceptualized as a simple correspondence of statements to reality as in old pre-war theories of truth, and objectivity cannot be conceived of in terms free of every prejudgment, bias, perspective or prejudice or in the Rankean terms of *wie es eigentlich gewesen*. Fundamental for all recent attempts to reformulate scientific/critical historiography is the presupposition of *multiple* reconstructions of every past. Therefore, all judgments of the representational adequacy of reconstructions of the past are *relative* to *other* reconstructions. Fundamental for scientific historiography is that these judgments are *evidence based* and thus are made on *epistemological* grounds instead of on aesthetic, literary, or political grounds, as is the case according to “literary” or “political” philosophies of historiography.

The philosophy of scientific historiography – in contrast to philosophies of “political” and “literary” historiography – keeps viewing historiography primarily as a cognitive enterprise, based on epistemology and on comparative methodology. In contrast to philosophies of “political” and “literary” historiography, philosophy of scientific historiography thus emphasizes the inseparability of historiographic narration and research. As Anthony Grafton put it, “Historical texts are not simply narratives like any other; they result from the forms of research and critical argument that footnotes document” . . . “The history of historical research cannot usefully be separated from that of historical rhetoric: even the best-informed efforts to achieve that separation distorts the developments they seek to clarify” (Grafton 1997: 232). It is the ongoing task of the philosophy of scientific historiography to elucidate why this is the case and to develop the criteria of its rationality.

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Darwin

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Charles Darwin never saw himself as anything but a naturalist, but his thinking about history was profound and wide ranging. Prior to the publication of *The Origin of Species* in 1859, there were various notions of history in biology and culture, one of the most pervasive of which in biology was the *scala naturae* or “chain of being.” The Great Chain, as the historian E. O. Lovejoy later famously called it, derived from Aristotle, and ranked living things as well as emotional states, societies, and various other aspects of the world, on a linear scale from lower to higher. A vestige of this remains in biological language when “higher organisms” are contrasted with “lower organisms.” Initial evolutionary hypotheses such as Lamarck’s, Geoffroy’s, and Darwin’s own grandfather’s effectively turned the chain of being on its side, adding a temporal dimension but retaining the notion of ascent towards perfection.

A later version of the chain of being found its way into typologies of social change in speculative philosophies of history such as August Comte’s 1822 tripartite division of the developmental stages of a society, from the *theological*, through to the *metaphysical*, leading to the *positive* states. This typology was at the same time both formal and ahistorical, allowing classification of societies at a synchronic point, and historical, indicating how the society in question had developed and would evolve in future. Like Lamarck’s notion of an evolutionary “lawn” of development from monads to elaborate forms through the influence of a *feu éthéré*, a physical impetus, Comte’s notion was that societies developed through the impetus of rational reflection. Harriet Martineau, an intimate of Darwin’s circle, translated Comte’s works into English in 1853 under the title *The Positive Philosophy of Auguste Comte*.

Darwin himself was influenced by Comte’s grand vision of history. Even before he had “got a theory to work by” from reading Malthus, he read a review of the French edition of Comte’s *Positive Philosophy* in one of his favorite literary magazines, the *Edinburgh Review* in August 1838. He was especially taken by the “grand *idea*” that science was in a theological phase to be overtaken by the unreligious positive stage. Later, however, his conception changed. His mature view of history, biological history at any rate, was not one of predetermined stages of development.

Interesting aspects of Darwin’s historiography (he would have conceived it as just “natural history,” that is, biology, as we would now call it and as some were calling it then) include several “theses” which are implicit or explicit in his work: The

employment of various metaphors for historical processes, in particular, the evolutionary tree; deep time, gradualness, and variable rates of change; a selectionist explanation of complex traits and interrelating systems; a rejection of presentism, a demotion of progressivism like Comte's, and the abandonment of typology of historical change; the importance of geographical distribution of variety as a way to infer the past; and a theory of innovation and diversity, the rarity of the first and the ubiquity of the second.

Obviously, Darwin employed each of these in the context of plants and animals. And so far as I know he never applied these principles to a human historical case, although he did suggest that the cultural evolution of moral sentiments was a special case of biological evolution in the *Descent of Man* (1871), just as in the *Origin* he had treated artificial selection as a special case of natural selection. These ideas find their way into later debates over sociology (for example the disputes between Boas and the cultural evolutionists), and history. I do not mean to suggest that Darwin was an extremely important philosopher of historiography or history. He borrowed many ideas he employed from economics and other sources. Nevertheless, the system of historiographic interpretation in the *Origin* was a major change of mindset, which is still underway. As the geneticist J. B. S. Haldane noted in another context, it will take a long time, perhaps centuries, to assimilate the implications of Darwin's way of thinking.

Progress and the Tree of History

At first, Darwin toyed in his Notebooks with the metaphor of a coral reef to model and explain the history of life. Darwin had originally explained coral atolls as the result of the shallow-living coral organisms building their calcified homes at a constant depth, while the land underneath them sank slowly and constantly. This meant that only the top level was alive, and underneath were the remnants of older past generations. But this metaphor was not terribly useful to anyone who did not know his theory or corals, and so he developed a different metaphor from various sources – the historical tree.

Darwin is remembered widely for his hypothesis of natural selection, but his hypothesis of common descent is as important as selection theory, if not more so. Darwin, like all naturalists of the day, was faced with the problem of arranging and explaining taxonomy. Under the Linnean system, which Darwin helped formalize in the *Strickland Code* of 1844, species were included in genera, genera in families, families in orders, orders in classes and classes in Kingdoms (phyla and, in botany, divisions, came later). This was not only convenient, but it seemed to reflect natural history. The question then was how and why the subsumption of “groups within groups” was “true”? Darwin solved it in the same manner as linguists had recently solved the development of language families, as the evolution of many forms out of a common ancestral form. He settled on the famous “tree” of evolution, with “layers” of time that resembled geological strata, a model the educated of the day would have been very familiar with. The only illustration in the *Origin* was a tree diagram (figure 36.1).

Tree diagrams for classification were very old – in the late middle ages the logic of division that had developed out of Aristotle's philosophy was represented by a diagram known as the Tree of Ramus, or Tree of Porphyry (*Arbor Porphyriana*, figure 36.2), in which one began at the top, as it were, of the Ten Categories Aristotle had said

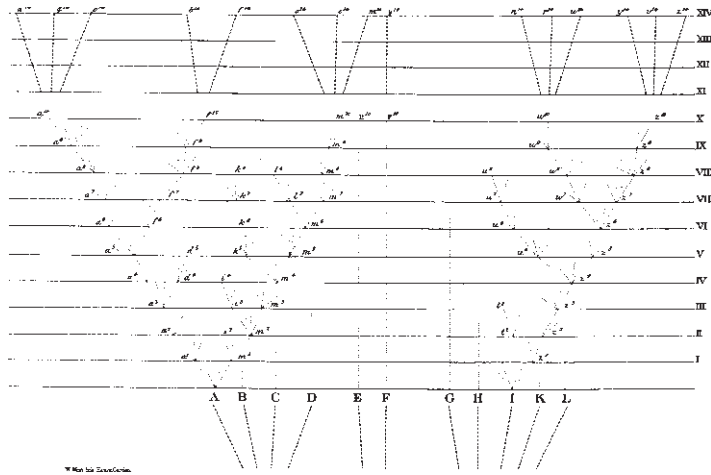


Figure 36.1 The only illustration in *On the Origin of Species*, showing a schematic tree of life
 Source: Reproduced with permission from *The Complete Work of Charles Darwin*, online,
<http://darwin-online.org.uk>

covered all metaphysics (what-it-is or substance, quantity, quality, relation, location, time, position, habit or possession, action, passion or undergoing), and divided into smaller categories called *species*, which could be themselves divided (making them lower ranked *genera*) into further species. At the bottom was the lowest species (*infimae species*). These diagrams had been revived in British logical writings, most notably by the Benthams, Jeremy and his nephew George, and were widely discussed.

But this tree structure, although employed to an extent in botanical and zoological classifications, was still static; it did not reflect change in time. In its traditional form, it was dichotomous – each genus was divided into two species, one that did have, and one that did not have the next differentia. This clearly did not work in biology, which was why dichotomous classifications were not used except as diagnostic aids (and logicians understood this necessity in biology). Tree diagrams in biology had then many species at each level. One such tree diagram was used in a journal Darwin read – the *Edinburgh New Philosophical Journal*, in which he had also read Comte reviewed. The diagram occurred in an article by Martin Barry explaining Karl Ernst von Baer's idea of development in groups of animals (figure 36.3). Von Baer argued larger related groups of animals such as families shared early developmental pathways, whereas in smaller groups later development expressed more particular traits. Thus, all vertebrates had a certain stage of development that looked very similar, but the specializations of birds and mammals arose later in development, and the species-specific characters developed last. Barry used the diagram to show this schematically, and Darwin seems to have been affected by this, as shortly after his own first tree diagram resembles it greatly. Darwin's tree diagram was drawn the same year, in 1837.

The idea of history modeled as a tree was rather remarkable, although it was implied by the idea that the Indo-European languages had a common ancestor (at the time thought to be very like Sanskrit). Von Baer, however, didn't initially see

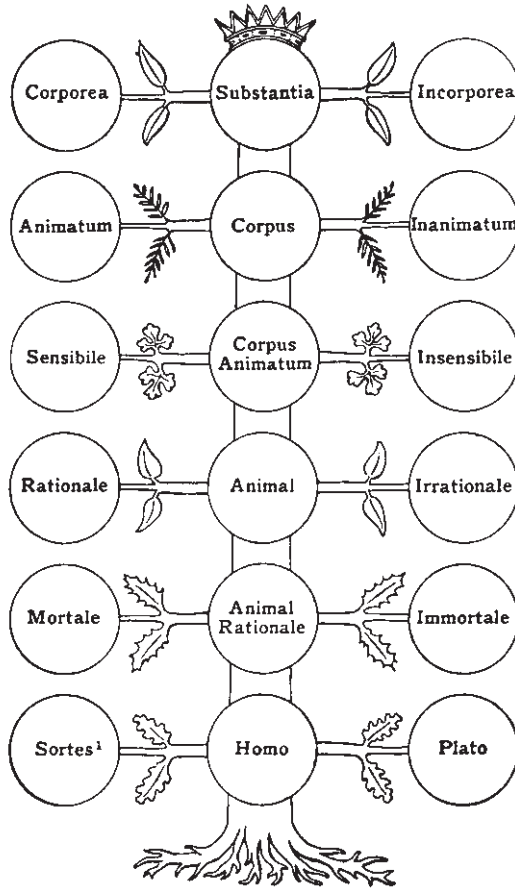


Figure 36.2 The Tree of Porphyry (*Arbor Porphyriana*)

Source: James Mark Baldwin (1905), *Dictionary of Philosophy and Psychology*, Oxford University Press

developmental variation as an evolutionary process, and indeed he rejected Darwinian evolution to the end of his life, well after the *Origin*, in 1876. Darwin offered something very new – not only was history not repetitive or static, changes occurred that caused unique branches over time. With this metaphor, which at a single stroke explained groups within groups, and allowed both transformation in gradual terms, and the discontinuity of the limbs at any given moment, Darwin effectively destroyed the Comtean view of history, and all linear or eschatological notions of history being akin to a developmental process. Lamarck saw a species' lifetime as a sequence of change like the maturation of an organism, a view that was repeatedly asserted in evolutionary biology for a century after the *Origin*. Darwin distinguished transformism from developmentalism (although the Latin word for that, *evolutio*, meaning the unrolling of an already written scroll, continued to be applied to species transformism), and rejected the notion of necessary universal progress in history.

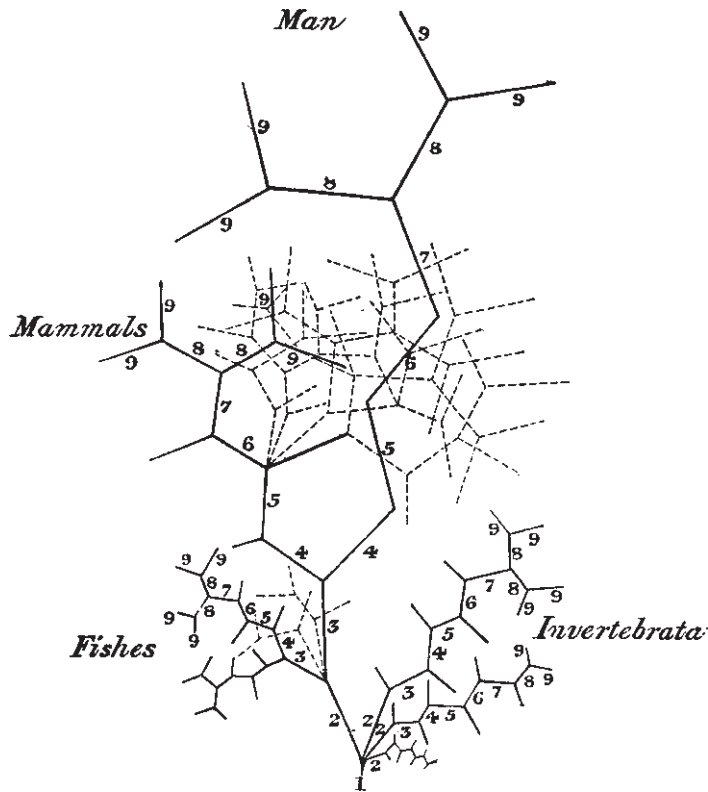


Figure 36.3 Martin Barry's diagram of Karl Ernst von Baer's idea of developmental pathways diverging from shared stages, which influenced Darwin
 Source: Reproduced with permission of Robert J. Richards, from his 1992 scan of Martin Barry, "On the Unity of Structure in the Animal Kingdom," *Edinburgh New Philosophical Journal*, 22 (1836–37), pp. 116–41

In contrast, Hegelians and Marxians saw history as a process of staged changes that were necessitated by the historical processes themselves, and this view continued apart from Marxist historiography in the views of historians like Toynbee well into the twentieth century (and in Kuhn's philosophy of the history of science). Historical developmentalism is a continuing temptation to popular and technical historiography and philosophy of history alike. But nobody was in any doubt about Darwin's view of history – there was no guarantee of progress beyond the merely local adaptation to the environment in which the species found itself, although Darwin hinted that there may be some general progress (*Origin*, 336ff). To many this was scandalous and even today it remains a matter of great debate.

Darwin himself was often not clear on this, conflating "race" with culture, and in particular the "British race," ascribing a progression to their cultural evolution. It is clear from his comments in the *Descent of Man* (1871) that Darwin thought that European *civilization* was superior to that of the "Turkish," "Negro," and "Australian" "races." This is ethnocentrism, not racism, showing that Darwin did not clearly

distinguish the two domains of biology and culture, perhaps due to his “Lamarckian” view of heredity and innate behavior. Darwin was certainly Eurocentric, but at the time anyone who had respect for the achievements of western science (and that included, for instance, the Japanese and much of the rest of Asia) also was. But he was of the opinion, expressed several times in his works, that Negroes and others were as capable of being “civilized” as any European, and Europeans being as degraded as any savage (in the *Journal of Researches*, 1839). Indeed, with his experience of Jemmy Buttons, the Fuegian that the *Beagle* took back to England, he could hardly think otherwise. Darwin was rabidly antislavery, and respectful of blacks in particular (he had learned taxidermy from an African ex-slave in Edinburgh). His co-“discoverer” of natural selection, Alfred Russel Wallace, went even further, asserting that there was absolutely no difference in ability between any two “races” of humans. Wallace put all differences down to ecological and social conditions. The evolutionary view of Darwin and Wallace was, in many respects, progress neutral, even if in connection with human society, they tended to be progressivists, and treated human variation as culturally based.

Discovering the Past

One of the problems Darwin faced was the incompleteness of the fossil record, analogous in historiography to having no written or material records for a historical period. Darwin wrote when a philosophy known (misleadingly) as *uniformitarianism* was a new view of geological processes, and in which the time horizon had been extended to many orders of magnitude greater than the traditional view of a few thousands of years. In effect, this latter change in geological thought, known as the discovery of deep time, meant that the timeframe for geology, and hence for evolution, moved from being more or less the same as for cultural change, to being immensely greater. This changed the way both cultural change was viewed and also, of course, biological evolution. The discovery of increasingly older human and other fossils, which had begun in earnest when Cuvier had surveyed fossil fishes around Paris, meant that human history was now seen as the ephemera of the recent past. Mark Twain remarked that seeing humanity as the goal of evolution was like seeing the layer of paint at the top of the Eiffel Tower as the goal of its construction.

How to make inferences about the past? Darwin’s solution was derived from reading the works of the leading uniformitarian Charles Lyell, who despite the prominence he now has in the historiography of evolution, was not widely well-regarded by geologists until much later. For example, Darwin’s mentor John Henslow told him to read Lyell’s *Principles of Geology*, but on no account to believe it. Lyell held that the forces operating in the present are the same forces operating in the past in geology, and so we can infer how some present state arose from knowledge of these forces. In conjunction with the idea of an articulating tree, Darwin was able to conclude that we can work out to an extent the relationships (i.e., genealogies) of modern taxa from their present characteristics. Of course, this only followed if recombination of the evolving lineages does not obscure evidence for past lineages, common causes and changes. Relying on von Baer’s model of individual development, or ontogeny, Darwin assumed that this

would be true of living things. One of the often-cited disanalogies between biological and cultural evolution is that this is not true of human history. Recent studies into lateral genetic transfer and hybridization suggest it is not always true of biology either, and the persistence of languages and traditions in culture indicate that it may hold in some aspects of history.

Darwin and Wallace also made much of the biogeographic distributions of species. Prior to the announcement of evolution by natural selection as a scientific hypothesis, Wallace had published a paper in which he noted that closely related species were usually adjacent geographically to each other. But migration muddled that signal too. Similarly in culture, ideas and practices are either independently arrived at (which in evolution is called *convergence*) or are spread by diffusion (an analogue to migration and speciation or local adaptation). Convergence is known as *homoplasy* and diffusion as *homology* in biology. The explanation of many traits being similar in independent lineages (homologous) is that they have a common origin, like the anatomical and physiological similarities of all primates. If only a few traits are similar then they may have evolved convergently (as homoplasies), like the similar body forms of tuna, dolphins and sharks despite their massively different developmental and anatomical forms. In culture, this explains why most alphabetical writing forms are similar in shapes, letter or sound value, and sequence (they share a common “ancestor” in Proto-Sinaitic writing), while pyramid forms are convergently arrived at in Egypt, South East Asia, and Mesoamerica. The differences between these pyramidal constructions can be easily explained from adaptation to the properties of stone based materials, but writing forms could have been (and often are) not alphabetical.

Teleological Thinking

As noted, most evolutionary thinking before Darwin, and a considerable amount since, has been teleological. That is, in matters of race, or adaptation, it is assumed that the human species, or the European race, or British, French, or German society, is the eventual goal of evolution. Teilhard de Chardin even went so far as to say that now that humans had evolved, evolution would conjoin all species in an “Omega Point.” But Darwin’s view of evolution was remarkably unpresentist or nonteleological. If he were a historian, we should say that he is free of Whiggism. Humans are one species with unique and remarkable traits, of course, but they are derived from older traits of our ancestors, and when you get right down to it, *all* species are the unique outcome of derivation from the forms of their ancestors. As he said in the *Notebooks*, “It is absurd to talk of one animal being higher than another. We consider those, when the intellectual faculties (cerebral structure) most developed, as highest. A bee doubtless would when the instincts were.” His most popular book was on effects of the lowly earthworms on soil, and he must have considered the worm equally as interesting to a dispassionate naturalist as any mere primate. Darwin has been accused or credited with a teleological account of evolution, beginning with his friend Asa Gray, but whatever he thought about matters beyond the biological realm, he clearly thought that there was no overarching goal to evolution, nor did he think there was a final cause for any particular aspect of evolutionary change. At the most his notion of finality was

that of local natural selection, and although he sometimes indicates that this is a struggle between species, most of the time this is simply within-species competition.

Competition has been perhaps the most misunderstood aspect of Darwin's view of history. From an early review in the *Manchester Guardian* through the reaction of Engels and later socialists to the supposed Malthusian basis for Darwin's theory of natural selection, and the opinion of Nietzsche to the present, Darwin has been supposed to have taken English industrial capitalism and made it a universal fact of life, thus legitimating it. But, despite seeing ecological differentiation as a kind of division of labor, in fact most of the cases of competition in his work involved struggle for the necessities of life in a condition of relative scarcity, and almost never direct violent battle of a Hobbesian kind, the main purpose of which was not survival, as Wallace tended to think, but reproduction.

In a historical analogy, this is more like the success of ideas and practices than the survival of classes or individuals. Some critics such as Mary Midgley have read Darwin as an apologist for violence, but it is more accurate to read what he said in the context of the work of people like Alphonse de Candolle, whom he cites (Darwin was strongly influenced by the French botanists and zoologists), on matters pertaining to the struggle for existence. The "war" is a war of economy, not of martial might. The trader (variety) who makes a higher profit and can thus fund future investments is more fit than the trader who barely manages to make ends meet, evolutionarily. This relies on Adam Smith and Malthus, but both those thinkers treated these phenomena as economic facts that had to be dealt with morally and civically rather than ends in themselves. So too did Darwin. His theory of evolution was a matter of explanation, not justification. Given the often-moral tone of historiographic *and* biological narratives of the time, this was also an achievement. Huxley, when he addressed the issues of drawing moral lessons from evolution the way Herbert Spencer did, pointed out that one cannot have a "pigeon fancier's polity" for human social prescriptions out of facts about biology. He asserted that we needed to know our nature in order to overcome it, a view advocated earlier by John Stuart Mill in his essay "On nature."

Darwin did not distinguish between types and species, and often casually discussed adaptation as the change of a species, but he did believe that what evolved were types within a population; as he put it in chapter 2 of the *Origin*: "Certainly no clear line of demarcation has as yet been drawn between species and sub-species. . . . These differences blend into each other in an insensible series; and a series impresses the mind with the idea of an actual passage." This is now known as "population thinking," but it is, in fact, much earlier than Darwin, and Darwin's influence on later statisticians helped deliver the new techniques of statistics (but they were also due as much or more to eugenics). The "insensible series" was both a taxonomic gradation, and a temporal one, and avoided the problem of radical discontinuities arising for which there was no explanation, expressed in the Latin phrase lifted from the Great Chain of Being literature (Lovejoy 1936), *Natura non facit saltum* ("nature makes no leaps" from which we get the term *saltation* for radical or discontinuous change). How true this was became the subject of much later debate, but for Darwin, even if the rate of change varied (and he explicitly allowed that it did in a later edition of the *Origin*), the changes themselves were always small at each stage. There was an organic unity in each life form that Darwin believed would be detrimentally disrupted by a major leap,

and we might consider if this is a principle that applies to other homeostatic systems, like societal institutions.

One of Darwin's greatest contributions was with respect to novelty. Every new structure or trait of organisms was a modified form of prior structures or traits, not something that arrived *de novo*. Darwin quoted the French zoologist Henri Milne-Edwards when he said in chapters 6 and 14 of the *Origin*, "We can plainly see why nature is prodigal in variety, though niggard in innovation." Given that traits are inherited, a novelty need arise only once and be subsequently modified. Novelty is rare. This is, of course, something that is also true in history, as we see from the example of alphabetical writing, but it goes also to uphold claims of the role of the Great Man in cultural history. A practice or idea can be hit upon only once and then passed on through time, being successively improved or modified to local circumstance, such as technologies or materials or level of education, and so on. Darwin's discussions of novelty are implicit, except in his discussions of language and sexual selection in birds in the *Descent*, and here he supposes that the love of novelty in the organisms drives the evolution of words and plumage. But the evolution of new features is throughout based almost entirely on the derivation of older features. If anything, modern genetics is far more saltative than anything Darwin expected. His discussion of the evolution of language in chapter 3 of the *Descent* draws an explicit parallel between biological and cultural evolution, to bolster the idea that language can evolve *from* biological traits. His descriptions of homologies in culture, of sounds, cognate terms, and grammars, indicate how closely he saw that analogy. He says there that like "organic beings" extinct languages do not re-evolve, and that extinction can be caused by invasive spread of other languages, like species extinction. Languages, like living kinds, can be classed "in groups under groups; they can be classed either naturally according to descent, or artificially by other characters," and they show variability like biological kinds. Words, like living beings, struggle for life. The "survival or preservation of certain favored words in the struggle for existence is natural selection." Note that it is not *analogous* to natural selection; it *is* natural selection.

Darwin's strong selectionism in biology and culture is perhaps the most questionable aspect of his philosophy of history. Modern discussions of selection in both biology and culture show how complex this matter truly is, and one problem of selective models is the "local optimum" problem, where the selected trait can find its way to a local suboptimum and be trapped there. Suggestions that science is a selection-driven process have been met with concerns that this might mean that science was not able to find the optimal solution. A historiographic research program into science may indeed find cases of a promising research program becoming trapped in a local optimum, unable to make further progress because of the assumptions or techniques on which they relied. Likewise, changes in other aspects of the evolution of society and culture may be driven to a local peak to remain trapped there until extinction or until the "fitness landscape" (the set of selection pressures imposed on a lineage by its environment) changes. Nevertheless, selectionist accounts of historical change continue to be written from a somewhat naive perspective.

Many of the objections to the application of Darwinism, a slippery term indeed, to human cultural evolution, arise not from Darwin himself but as a reaction to the ideas of those who were called Darwinians, such as Herbert Spencer, who had been a strong

cosmic progressionist before Darwin, and the original social Darwinian, William Graham Sumner. We must also consider the ideas of Ernst Haeckel, whose social philosophy of Monism challenged the autonomy of culture from biology, and who held that racial development, contrary to Darwin and Wallace, underwent a Comtean sequence of maturation, and that “primitive” races were in their “infancy.” Haeckel’s notion of *biogenesis* assumed that the evolutionary history a species was preserved in the developmental stages of its foetus. He was therefore a good deal more enthusiastic than Darwin about reconstructing those evolutionary histories, and coined the term *phylogeny* (evolutionary development), which he said famously is recapitulated by *ontogeny* (individual development). Phylogenetics has been elaborated and refined into a system of analysis and classification known as *cladism*, in part based on Haeckel’s ideas, but without the objectionable aspects of Haeckel’s view of history.

So what sort of historiography might we draw from Darwin, if we translated his biological view of history to the human case? Surely he would think this a legitimate move, given that he always knew that he was talking about humans as well (a note in his notebooks from 1838 says “Origin of man now solved. – Metaphysics must flourish – He who understands baboon would do more for metaphysics than Locke”). Can we treat change in human history as being the same sort of process as change in biology?

Darwin would have been inclined to treat history as a process of local adaptation by selection. Darwin would not have expected history to be staged or developmental, nor would he expect that it would be globally progressive or goal-oriented as in the *scala naturae*, or the eschatological, or providentialist, or Marxist philosophies of history. There is no typology for historical change – everything has its own history and not something else’s. But there may be post hoc empirical generalizations that we can draw. Darwin was not beyond drawing some himself.

Darwin would have seen classification of cultural types as being based on descent – that is, on transmission from generation to generation, with a proper classification relying on divergence and distinctness. The analogue for species would be, I believe following a suggestion of Stephen Toulmin’s (1972), “tradition.” Darwin would have rejected the Great Man theory of evolution, because the origin of variants is less interesting from an evolutionary perspective than the success they later have due to adaptive fit and their subsequent spread through populations. Moreover, the phylogenetic perspective would be more interested in what the novelties were derivations of, than assuming that they were entirely novel. He would expect that similar traits would be spread geographically and that their distribution would reflect this. Analogues (convergences) would be evident from the lack of a deep sharing of similarities.

Perhaps the most fundamental aspect of Darwin’s view of history would be that information about the past is preserved imperfectly. In history, information decays, and so we are left to make inferences as best we can. The discovery of a stele, or a document, or shards, in a period unrecorded by written contemporary sources is evidence that something existed at a date, but not evidence that it did not exist earlier. We have lost the data to determine most of history, either biological or cultural.

Finally, an evolutionary perspective (*sensu* Darwin) is a mindset. Darwin once noted that not having a hypothesis to test when making observations is like being a

geologist going to a gravel pit to count stones. Looking for the dynamics of the evolution of culture, and testing the general ideas and analogies of biological evolution, will help sharpen our investigations, and teach us what is unique to culture without allowing our folk historiographic assumptions to intervene.

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Logical Empiricism and Logical Positivism

KRZYSZTOF BRZECHCZYN

Logical Positivism: Basic Information

The beginnings of logical positivism go back to the 1920s, when Moritz Schlick (1882–1936) started a seminar in Vienna. In 1929, during a philosophical congress in Prague, Rudolf Carnap (1891–1970), Hans Hahn (1879–1934) and Otto Neurath (1882–1945) announced the philosophical manifesto *Wissenschaftliche Weltauffassung. Der Wiener Kreis*, which gave rise to the Vienna Circle. In the 1930s among the members of the Vienna Circle there were R. Carnap, Herbert Feigl (1902–1988), Philipp Frank (1884–1966), Kurt Gödel (1906–1978), H. Hahn, O. Neurath, M. Schlick, and Friedrich Weismann (1896–1959). In Vienna Karl Raimund Popper (1902–1994) was under the influence of the Circle, and in Berlin among the supporters of it were Carl Gustav Hempel (1905–1997) and Hans Reichenbach (1891–1953). The Circle was active until the end of the interwar period; its main exponents were murdered (Schlick), died a natural death (Hahn), or emigrated because of the growth of fascism in Austria, mainly to Great Britain and the USA, to continue scholarly activity there.

The ideal of science

One of the basic theses of the philosophical program of the Vienna Circle was the idea of the unity of science. This idea was a key influence on the attitude of the representatives of logical empiricism to the social sciences and humanities, including historiography. The very idea of the unity of science was variously understood within the Vienna Circle, as the unity of scientific language, or the unity of scientific laws. The unification of scientific laws was to be based on the reduction of social laws to physical ones, or on the idea of connecting all scientific laws. Carnap (1936–7) advocated the unity of the language of science and proclaimed that all scientific notions ought to be reduced to observational terms, which would confer upon them empirical sensibleness. Neurath ([1939/40] 1983) supported the idea of connecting scientific laws seen as part of one system of knowledge – unified science.

Neurath was also one of the few representatives of the Vienna Circle who paid attention to the social sciences. He presented ([1931] 1973) a project of “empirical”

sociology based on a radical reformulation of the language of sociology. It was to be excised of all statements that would not conform to scientific laws, and those that could not be coordinated with sentences relating to data arrived at by experience. Such statements were conceived of as “metaphysical.” On this basis, sociology was to be expurgated of such terms with no empirical reference as “soul,” “personality,” or “free will.” Scientific laws formulated on the ground of Neurathian sociology were to make prediction of the future possible and were to be the foundation for social practice. As a starting point for the postulated empirical sociology – a rare phenomenon on the ground of logical positivism – Neurath chose Marxism as a social theory that was the closest to the proposed physicalist reconstruction of the social sciences.

The absence of historiography in the methodological and philosophical reflection of the representatives of the Vienna Circle resulted, first, from the limits of their university education and, second, from the ideal of science that they adopted. Among the members of the Vienna Circle there were predominantly representatives of the exact sciences: mathematicians, logicians or physicists. They had little direct experience of the scientific practices of historians. Their ideal science was physics. From their perspective, historiography appeared peripheral and backward, much wanting with respect to the postulated ideal.

The Hempelian Model of Explanation in Historiography

This situation changed following the publication of Hempel’s “The Function of General Laws in History” ([1942] 1965), which initiated a discussion about the status and peculiarities of explanation in historiography. Hempel applied to historiography a model of explanation which was proposed by Popper ([1935] 1959) as a general model of explanation used in science.

The deductive-nomological (or covering-law) model of explanation, proposed by Hempel, consists of the *explanandum*; that is, the sentence that describes the phenomenon to be explained (the sentence, not the phenomenon as such), and of the *explanans*, which contains explanatory sentences. The *explanans* consists of a description of the antecedent conditions and of sentences containing general laws.

The process of explanation can be subsumed in the following schema (Hempel, Oppenheim [1948] 1965: 249):

C_1, C_2, \dots, C_n – sentences stating antecedent conditions,
L_1, L_2, \dots, L_n – sentences containing general laws
E – sentence describing the empirical phenomenon that is to be explained (<i>explanandum</i>)

For the presented model of explanation to be adequate, it must meet certain logical and empirical conditions. The *explanandum* must be logically deduced from the *explanans*. The *explanans* must contain a general law of a strictly universal form and sentences characterizing the antecedent conditions. A scientific law should not contain proper names or temporal-spatial determinants. The term “law” may suggest that the statement is well confirmed by empirical data, though philosophers like Davidson dropped any reference to the world in analyzing the covering-law model, claiming that

Hempel's argument was purely analytical, about the logical form of explanation. Both, the sentences containing the general law, as well as those describing antecedent conditions should be equipped with empirical content so that they can be tested by experiment or observation. Accordingly, all the constituents of the deductive-nomological model of explanation should be empirically testable: the sentences describing antecedent conditions (1), the scientific law upon which the explanation is based (2); a test should also be applied to see if the *explanandum* logically derivable from the general law and antecedent conditions (Hempel [1942] 1965: 234).

Explanations of this kind, Hempel observes, are implicit in historiographic narratives because many historians assume general psychological or group psychological laws. Since such laws are familiar from everyday experience, they are tacitly taken for granted by historians. For example, people who have jobs do not want to lose them; people who have acquired certain skills, do not welcome changes; people who exercise a certain level of power and control do not want to give it up but work toward still greater power and prestige.

Often the regularities called upon by historians in their explanations have the character of general tendencies. This makes it possible to distinguish a probabilistic model of explanation: When certain conditions are satisfied, an event of a specified kind will lead, with a certain degree of likelihood, to a specified outcome.

Hempel presented models of explanation. He claimed that scientists in their research practice merely approximate the models more or less. Accordingly, historians usually use *explanation sketches*. They consist of an indication of the scientific law and antecedent conditions that are considered relevant to a given phenomenon. An explanation sketch requires further "filling-out" to be transformed into a "full-bloodied" explanation. This filling-out necessitates undertaking further empirical research for which the explanation sketch only suggests the direction (Hempel [1942] 1965: 238).

The model of rational action

In historiography – in contrast to the natural sciences – we deal with the explanation of actions of conscious agents. In explaining the actions of an individual we reconstruct the aim and the motives of the individual, his or her situation, as well as the knowledge of the individual about the surrounding world which connects the undertaken action with its expected result. The knowledge of the individual is not in any way constrained – from the point of view of the interpreter it can be false, superstitious, or probable. Explanation proceeds according to the following schema (Hempel 1963: 155):

A was in a situation of type C,
 A was a rational agent,
Every rational agent will, when in a situation of type C, (resp. with high probability) do x,
 A did x.

It is assumed here that agent *A* in situation *C* chose (from among a certain range of actions open to him or her) such action *x* which will lead to the outcome that realizes the pursued objective in a highest degree. One of the criteria of rationality is maximizing

the expected utility of action (Hempel 1965: 463). The assumption of rationality functions as a general scientific law. For this reason, explanation through reference to the rationality of the subject can be subsumed under the general deductive-nomological model of explanation (Hempel 1962: 27).

The problem with this account is that it is unclear what is the empirical content of rationality? To whom does it refer? If we try to explain the actions of historical (or present) agents, it turns out that few of them acted fully rationally. Hempel compares the assumption of rationality with idealized explanatory models, for instance with the Boyle–Charles law of ideal gas. The law states that when the size of molecules and intermolecular forces equals zero, then for a given mass of gas in constant temperature the product of volume V and pressure p is constant. These conditions (omitting the influence of the size of molecules and of intermolecular forces) are of a counterfactual character, because no real gas meets them, and the Boyle–Charles law is applicable to them only approximately (for more on the use of idealization in science see: Nowak 1980; Nowak and Nowakowa 2000; Topolski 2007). The rational subject is an ideal. As the sizes of molecules and intermolecular pressure “prevent” gasses from behaving fully in agreement with the Boyle–Charles law, factors such as fatigue, pain, or worry prevent individuals from undertaking fully rational actions. According to Hempel, all intelligent individuals are able to make rational choices if the decision structure of the problem is clear and it requires relatively simple solutions, as long as disturbing circumstances do not interfere with making the optimal choice (Hempel 1965: 482; on the idealizing status of the assumption of rationality see Kmita and Nowak 1971). However, real people in everyday life rarely meet situations devoid of all disturbing circumstances. Therefore, they behave rationally to an approximate degree.

Genetic explanation

Historians often explain by describing the stages preceding, leading up to, the examined phenomenon. Hempel suggested that in such genetic explanations in the form of historiographic narration, expressions such as “hence,” “therefore,” or “because” are used to refer implicitly to certain general laws. These expressions connect the antecedent conditions with the events to be explained.

Genetic explanation usually present a sequence of events: event A led to event B , and this to event C etc. A genetic explanation of an event in a historiographic narrative does not include descriptions of all the events preceding the occurrence of the explained event, but only of such events that are “nomologically connected” with the *explanandum*-event. Historiographic narration is divided into stages. Each stage consists of sentences presenting nomological relationships and of simple descriptions. Historiographic narration, then, does not resemble annals in which events are only chronologically ordered, but “each stage must be shown to ‘lead to’ the next, and thus to be linked to its successor by virtue of some general principle which makes the occurrence of the latter at least reasonably probable, given the former” (Hempel 1962: 23). Genetic explanation in the initial stage of narration begins with a simple description of event A . The second stage consists of two parts: part one being a description of those features of the explained event B which are a consequence of the features of the preceding event A , described in the first stage of the narration. Part two contains a

description of those features of event *B*, which are to explain at least some aspects of event *C* in the third stage of narration, and so on.

Reception

Hempel's proposal gave rise to a lively discussion, lasting more than half a century. Here, we consider the reception of Hempel's views only on the terrain of the methodology and philosophy of history (for the discussion within philosophy of science, see Salmon 1990). Following Atkinson's classification (1978: 23–34), three general positions can be distinguished: assimilationism, autonomism, and methodological revisionism (cf. Danto 1968: 201–32; Dray 1964: 15–20; Mandelbaum 1961: 229–39; Topolski 1976: 181–6, 558–68).

Assimilationism held that the deductive-nomological model, obligatory in the natural sciences, can be applied to historiographic explanation, even if small corrections and modifications should be introduced. Autonomism claimed that this model did not reflect at all historiographic research procedures. Historiography is about understanding and interpretation of historical facts rather than their explanation in the sense adopted by the natural sciences. A middle position was taken by the methodological revisionists, who proclaimed the necessity of retaining Hempel's model but at the price of introducing a significant modification, so that the model would reflect the specificity of historiographic research. I present here the most representative voices in the discussion (cf. Gardiner 1974).

Among the proponents of methodological assimilationism one can include Patrick Gardiner, as well as Carey B. Joynt and Nicholas Rescher. Gardiner claims that one of the differences between historiography and the natural sciences lies in the very use of language (Gardiner 1952: 51–65). Historiographic concepts do not come with empirical criteria of applicability. They are then imprecise and unmeasurable. Historiographic laws, therefore, are not as precise and exactness as in the natural sciences.

According to Joynt and Rescher, the differences between the historiographic and natural sciences is methodological (Joynt, Rescher 1961: 153–4). The task of the natural sciences is to formulate scientific laws. Empirical data serves only to confirm or falsify scientific laws. The historian, by contrast, is interested in a particular historical fact as something exceptional and unique. If the aim of the historian is to explain a historical fact, then he or she uses both, laws formulated in other sciences, as well as commonsensical generalizations. The historian, in this view, is not a producer of scientific laws, but their consumer. If historians formulate general statements, then these historiographic generalizations are not strictly universal sentences, but include spatial and temporal determinants.

William Dray and William Walsh upheld methodological autonomism. According to Dray (1957: 66–72), explanation in historiography depends both, on the context of the situation in which it is carried out and on the fact which a given phenomenon explains. Explanation of a historical phenomenon consists of dividing it into a series of subphenomena until we reach such level, which is understandable and does not require further explanation. For instance, if we want to explain the outbreak of the Second World War, then this complex phenomenon must be divided into a series of subphenomena: the political strategy and the motivating factors of Hitler and Stalin,

the calculations and the attitude of the government of the Polish state and of the western allies, and so on.

Walsh (1976: 59–63) following Whewell, interpreted explanation as colligation. In his view, if a historian wants to explain a particular phenomenon, he or she classifies it as part of the general tendency present in a given historical epoch. The outbreak of the Second World War can be understood only when we look at it as part of the German policy of expansiveness after the First World War, and after we link it with other facts, such as the withdrawal of Germany from the League of Nations, the *Anschluss* of Austria, or the annexation of the Sudetes. The process of explanation in historiography consists, then, in the quest, by the historian, for “certain dominant concepts or leading ideas by which to illuminate his facts, to trace connections between those ideas themselves, and then show how the detailed facts become intelligible in the light of them by constructing a ‘significant’ narrative of the events of the period in question” (Walsh 1976: 61).

Finally, Arthur Danto’s ideas can be presented as an example of the revisionist position. According to Danto (1968: 233–7), all explanation takes the form of narration. What is explained, however, is not a given state of the *explanandum*, but its changes. The model of explanation assumes, then, the following form:

- (1) x is F at time t_1 ,
- (2) at time t_2 , x undergoes event H ,
- (3) x is G at time t_3 .

In this structure, the *explanandum* are the sentences (1) and (3), which describe the change of the explained phenomenon from F into G , from time t_1 to time t_3 . Sentence (2) is the *explanans*, which in a specific case can be subsumed under the deductive-nomological model. The structure of explanation itself has the structure of narration, where (1) is the beginning, (2) the middle, and (3) the end of it.

Popperian Critique of Historicism

Popper’s philosophy of science and his ensuing views about historiography, followed, to an extent, the ideas of the Vienna Circle, but in other respects also clearly rejected them. Popper supported the unity of the natural and social sciences, even if, contrary to other representatives of the Vienna Circle, he located the basis for the unity of science in the appropriate method of inquiry. In science we begin, on the basis of the already existing theoretical or empirical knowledge, from the formulation of the problem—it can be a problem of a practical, theoretical, or historical nature. Then we produce a tentative solution to the problem by formulating a tentative theory. This theory is then subjected to critical discussion, during which it is tested empirically, corrected, and developed. As a result of this procedure, new problems arise that are generated by the adopted theory.

Historiography attempts to explain historical events, past human action. Popper acknowledged that human action can be explained differently than natural events, as the philosophy of action has elaborated. Action takes place in a social situation, which

consists of four elements: the physical and social components of the situation, the aim of the agent, and the knowledge that the agent has about the conditions of action – the physical and social constraints. An analysis of social situation should reconstruct all these elements. On the basis of the knowledge about the material and social conditions of the situation in which the agent finds himself or herself, he or she will choose – from the range of all the actions open – those actions that will lead to the realization of the pursued aim.

Popper illustrates this with the following example: let us imagine a pedestrian who wants to catch his train and crosses in a hurry a street full of rushing cars. Let us assume that we want to explain the somewhat nervous and hurried movements of the pedestrian as he heads towards the other side of the street. In order to do so we have to reconstruct the components of the situation in which the pedestrian has found himself. It will be composed of material components: the width of the street and the speed of the moving cars, as well of social components: social institutions such as the highway code, police regulations, traffic lights, or zebra crossings. The material and social components of the situation impose certain constraints on the behavior of the pedestrian.

To explain the actions of the pedestrian one has to ascribe to him certain aims (for instance, to cross the street) and knowledge of the components of the social situation. The pedestrian, from among the range of actions open to him in the light of his knowledge, will choose such action (crossing the street in a hurry when there is no police officer in sight, or waiting for the green light when there is a police officer nearby), which will allow him to realize the aim he pursues – to cross the street and to buy a railway ticket.

Critique of historicism

Popper did not reject the possibility of discovering general laws in the historiography. He only claimed that laws of this kind are so banal and trivial that they cannot generate serious research problems (Passmore 1962; Scriven 1974); they are, therefore, useless. To use his illustration, we explain the first partition of Poland in 1772 by the impossibility of resistance by the Polish forces against the united armies of Russia, Prussia, and Austria, we adopt a certain banal general law (a law of the sociology of military forces): “when from two armies of more or less equal level of weaponry and command, one has colossal advantage in the number of troops, the other can never gain a victory” (Popper 1950: 448ff). This law is however too banal to generate any inspiring research problems for sociology or historiography.

Popper rejected the view that it is possible to formulate laws of historical development which will allow to predict the future. He called such a view historicism, in which he distinguished a naturalist and antinaturalist versions. The antinaturalist version of historicism emphasizes the separateness of the humanities from the natural sciences because of the uniqueness and complexity of human beings and society. This version of historicism postulated, however, research into such supra-individual entities as classes, nations, or civilizations, and determining the historical tendencies that characterized them, as well as the forces that stood behind them. This kind of historicism cannot be falsified, and therefore, according to Popper’s philosophy of science, is non-scientific.

Naturalist historicism models itself on the natural sciences, assumes the possibility of formulating laws of historical development and on their basis, of making predictions (Popper [1948] 1968). The natural sciences, for instance, astronomy, can with a high degree of precision and for a long time ahead, predict certain natural phenomena such as solar eclipses. Likewise, the social sciences should be able to forecast certain social phenomena, for instance, revolutions. The social sciences have, therefore, fundamentally the same tasks as the natural sciences – to make scientific predictions, forecast the social and political future of humanity. Politics should then follow these scientific conclusions, in Marx's terminology, to lessen the "birthpangs" that precede the predicted, unavoidable political events.

Popper argued, however, that there is an important difference between *scientific prediction* and *unconditional historical prophecies*. The historicists ignore this difference. Scientific predictions usually have a conditional character. If certain changes take place, they will be accompanied by other phenomena (if the temperature of water in the kettle increases, the water will boil). The physicist will say that under certain conditions the boiler will explode, the economist will say that under certain conditions a black market will develop, and so forth. The formulating of unconditional prophecies without theoretical justification are, however, only illusions. The historicist does not derive his or her prophecies from conditional scientific predictions. Predictions can, in Popper's view, be made about isolated and recurrent systems. Among such systems are the solar system, the life cycles of biological organisms, or weather cycles. The method of long-term prediction cannot be, however, applied to human history, because in the development of human societies there appear non-repetitive and novel phenomena. And, in Popper's view, prediction is conditional on repetitiveness. Popper moderates his criticism in one respect and says that in as much as certain social phenomena are repetitive, predictions can be made in the social sciences. Repetitiveness is to be found in how new religions arise, dictatorships come into being, and so forth.

The following argument by Popper contends, however, that historical development is mainly non-repetitive, and, therefore, unpredictable:

1. The course of human history is strongly influenced by the growth of human knowledge . . .
2. We cannot predict, by rational or scientific method, the future growth of our scientific knowledge . . .
3. We cannot, therefore, predict the future course of human history.
4. This means that we must reject the possibility of a *theoretical history*; that is to say, of a historical social science that would correspond to *theoretical physics*. There can be no scientific theory of historical development serving as a basis for historical prediction. (Popper [1944/5] 1964: vi–vii)

What is left, then, to the social sciences? According to Popper, the main task of the theoretical social sciences is "*to trace unintended social repercussions of intentional human actions*" (Popper [1948] 1968: 342). Popper illustrates this, writing about the wish to buy a house in a certain neighborhood. The intention of the buyer is certainly not to increase the prices of the houses in the chosen area. However, the very fact of there appearing a potential buyer on the market will tend to lead to a rise in the prices of

houses. And this was certainly not the aim of the buyer. The aforementioned task, according to Popper, brings the theoretical social sciences close to the experimental natural sciences. Both types of sciences formulate technological rules stating what cannot be achieved:

The second law of thermodynamics can be expressed as the technological warning, "You cannot build a machine which is 100 percent efficient." A similar rule of the social sciences would be, "You cannot, without increasing productivity, raise the real income of the working population." These examples may show the way in which the social sciences are practically important. They do not allow us to make historical prophecies, but they may give us an idea of what can, and what cannot, be done in the political field. (Popper [1948] 1968: 343)

The Popperian critique of historicism has been received critically. The object of discussion was the very notion of historicism, the classification of Marxism as belonging to it, and Popper's argument demonstrating the impossibility of formulating forecasts in the social sciences (Topolski 1976; Hall 1985; Nowak 1991; Rozov 1997).

Topolski (1976: 179–80) focused on the fact that the Popperian notion of "historicism" is quite an arbitrary cluster of positions grouping together various authors (Marx, Toynbee, Spencer), whose common feature is to be a certain holistic conception of society and a fatalistic understanding of historiographic laws and tendencies. However, Topolski argues, Marxism cannot be subsumed under historicism so conceived, and it was Marxism that was to be the main addressee of Popper's criticism.

Leszek Nowak (1991: 217–21), in turn, analyzes the structure of Popper's argumentation on the possibility of formulating prediction and observes that premise (1) above is a typical statement about the historical process. As Nowak further argues, if somebody wants to demonstrate the incorrectness of a certain domain of thought, he or she cannot use in the argument propositions belonging to the domain in question. Popper, in Nowak's view, makes the same mistake that the representatives of the Vienna Circle were charged with, as they demonstrated the logical impossibility of metaphysics on the basis of a specific proposition of a kind of metaphysics – materialist metaphysics of physicalism. In this case, Popper, as Nowak argues, demonstrated the impossibility of theory of history on basis of an idealist theory of history which may, but does not have to, be accepted (cf. Hall 1985: 6–7).

In Nowak's view, premise (2) of Popper's argument is true – to predict a future discovery means to know it in advance, but its not this premise but its enthymeme that is employed in Popper's argument: "(2') To predict the future course of history it is indispensable to know the content of future scientific knowledge" (Nowak 1991: 218). This premise, however, in Nowak's opinion, is false because the shape of the influence of future scientific theories upon many important social phenomena can be determined quite independently of their content. Economists tend to measure the rate of technological progress in terms of its influence on the effectiveness of labor. We do not know anything about future technological processes and discoveries, but we do not have to know this to predict increasing economic productivity. It will suffice that, extrapolating from the type of relationship present to date, we will predict further increase of the effectiveness of labor.

Let us, shortly, comment on the Popperian positive program for the social sciences, based on the formulating of certain social rules. However, the technical rule Popper used: "You cannot build a machine which is 100 percent efficient" presupposes the second law of thermodynamics. This raises a question about the status of the aforementioned rules ("You cannot, without increasing productivity, raise the real income of the working population") in the social sciences, if Popper rejects the possibility of building "a *theoretical history* . . . that would correspond to *theoretical physics*." What remains is then formulating common sense generalizations of the type "power corrupts." This leads to Popper's noticeable methodological inconsistency: in a hypothetistic theory of science he approves of bold and risky theories formulated on the ground of the natural sciences, while in the methodology of the humanities he prefers careful commonsense theories and the making of predictions of a similar kind (Kmita 1977: 14).

Conclusion

Logical positivism influenced the philosophy of historiography along with other areas of philosophy. It added debates on laws and explanation to the core problems of the philosophy of historiography and reformulated older debates about the unity of science vs. the autonomy of historiographic method. It then touched on what would become philosophy of action. However, the logical positivists did not develop a united approach to these problems. There is no logical positivist answer to whether historiography is a science or not, whether there are laws of history and whether historians use them to explain, or whether they manage to explain without such laws. The end of logical positivism in the philosophy of historiography has been marked by a move from philosophic interest in the analysis of the language of historiography, to examination of its relations with the evidence and the reality.

Note

Translated by Janusz Kazimierczak.

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Jewish and Christian Philosophy of History

SAMUEL MOYN

“Some people see in all earthly things only a dreary cyclical movement,” Heinrich Heine wrote around 1833. “In contrast to the fatal and indeed fatalistic view,” however, “there is a brighter view, more closely related to the idea of providence. According to this view all earthly things are maturing towards a beautiful state of perfection . . . a higher, godlike condition of the human race, whose moral and political struggles will at last lead to the holiest peace, the purest brotherhood, and the most everlasting happiness” (Heine 2003: 195–6). It is the latter opinion about the shape and meaning of history that, in its original sacred and eventually in its secularized form, the Jewish and Christian philosophy of history contributed to the world.

The peculiarity of the Jewish and Christian philosophy of history is that, instead of beginning from first principles, it arose from reflection on a prior article of faith, recorded first in the Bible, that history is a meaningful process, the “medium” in which God’s will is at work, if sometimes in mysterious ways. If the philosophy of history is about the analysis of the structure, meaning, and end of history, and how such claims could possibly be validated or refuted, if at all, it may seem difficult to say what exactly was *philosophical* about the Jewish and Christian “philosophy of history.”

It is undoubtedly true that the idea of the providential structure of history – and the related issue of how to interpret it – may have originated before the historical appearance of philosophy. And they always remained ultimately subordinate to a theological scheme. To insist on the persisting pre-philosophical framework for the development of western historical thinking, however, would be to slight the extraordinary pluralism and increasing sophistication that this framework permitted over more than two millennia. Scripturally based chronology collapsed as a viable intellectual project in the early modern period, but it remains living not simply in unreconstructed popular understanding, but also in fragmented form in the tools of contemporary enlightened thinking.

There were a number of analytically distinct ways in which the Jewish and Christian traditions of thinking about history were philosophical. The near eastern salvation religions, far more so than many other historical belief systems, placed temporal thinking at the core of their cultural apparatus (Eliade 1959). From the outset, they featured different kinds of “historical” claims, which presumably committed them to some at least implicit philosophy of history or even interpretive philosophy of

historiography. Over time, these substantive visions of the shape and meaning of time as a whole and the interpretive commitments they required about how to decode events became more explicit than they were in the beginning.

Substantively, religious writers developed numerous accounts of the historical process from the creation of the world to the end of time; the “theistic and especially Christian philosophy” of history that for so long defined the subject simply meant reflection on the “script” of time – how many acts it had, how long it would run, where the present was in the drama, what the end would look like. Further, over the ages, Jewish and Christian speculation about history underwent a partial secularizing process. The result was a historical teleology, the doctrine that the meaningfulness and direction of time could be intuited or inferred through or from a careful study of history by itself, not simply from scripture or dogma. On the interpretive side, thinkers in both traditions, in different ways, developed a hermeneutic for how to understand the events of recorded history in light of biblical prediction, and eventually according to an interpretation of history that did not require direct revelation but attention to the intrinsic logic of historical development. These philosophical endeavors were a crucial source for much modern thinking about historical and social change, even once the pre-philosophical framework of religious speculation had long since been given up.

Biblical Foundations

As a generally weak and subordinated cult in the ancient world, the Near Eastern salvation religion that eventually became Judaism faced the problem not only of its frailty but also of its comparative recent origins. Their shallow roots in the past – unlike those of their potent and ancient political rivals – led the Israelites to invent an unprecedented form of futurism that would present history as the forum in which God would eventually compensate for the community’s humbling string of defeats and overall political marginality. God’s promises to Abraham will take time to realize, and tests – whether bondage in Egypt or military defeat after the exodus from there – are part of an overall design.

Later known as “theodicy,” this theological apology for present tribulations – one that worked by placing them in a causal relationship with future triumph – is the site of origin of the western sense of history as an unfolding “rational” plan rather than the chaotic anarchy it may have seemed. “If Herodotus was the father of history,” one recent authority puts it, “the fathers of meaning in history were the Jews” (Yerushalmi 1982: 8; cf. Löwith 1949; Taubes 1947). The notion that God has a plan that will redeem the Israelites from the reversals of the times – which are his punishment for their sad derelictions – is recorded early, for example in Moses’ song to his people before he departs (Deut. 32).

The Pentateuch has a basically victorious conclusion, leaving the Israelites on the brink of the fulfillment of God’s original promises. It took the prophetic and apocalyptic books – frequently charged with accounting theologically for later political disasters – for theodicy to truly come to the fore. For the prophet Isaiah, the victory of grand empires over the Israelites must be understood as an early sign of God’s enduring preference for the losers. The conquering Assyrians are “the rod of [God’s] wrath,” only

apparently serving their own interests but actually advancing the Israelite cause by recalling them to their faith (Is. 10: 5). The growing apocalyptic quality of futuristic speculation in later biblical books not only sharpened the sense of eventual reversal of present defeat, but laid the foundation for later “philosophical” attempts at rational inquiry into history’s periodization. Compared to the mundane content of God’s future blessings and curses in the Pentateuch – geographical expanse, population size, and agricultural yield are the main concerns of the original covenant – the early prophets had already begun to imagine more cosmic threats and dreamed of a more extraordinary redemption. But the apocalyptic literature proper beginning not long before the birth of Christ amplified what is at stake in history yet further.

In the characteristic apocalyptic vision, God’s plan will come to pass through his own direct intervention, and in a revolutionary spasm; nothing man can do will forestall or hasten God’s design, which will come to pass explosively. “Our world hurries towards its end,” says the apocryphal Ezra apocalypse (cited in Funkenstein 1993, 70). The end will not come “by human hands” (Dan. 9: 25). Barred from actually being prophets as of old, apocalyptic writers were forced to follow one of two models. They could attribute their speculation to old prophets whose texts were supposedly hidden and now discovered; here the book of Daniel, with its extraordinary premonition of four kingdoms, began a very long tradition. Or they could claim to grasp the meaning so far hidden in the opaque prophecies long possessed. The latter strategy made apocalypticists not just self-styled revivalists of religious mythology but “decoders” with intellectual claims about how to relate predictions and events. The results went far beyond the Pentateuch and the early prophetic books. “Nowhere before,” as Amos Funkenstein argued, “was historical time, the course of history as a whole, so strongly perceived as a unity structured by precise periods” (Funkenstein 1993: 70–8 at 76).

The advent of Christianity occurred on a terrain sown by these apocalyptic visions that portrayed the realization of God’s long-stirring intentions as enigmatic, cataclysmic, and possibly imminent. The Jewish Jesus, who preached that the “kingdom of God is at hand,” understood himself to be on the brink of or actually in the predicted end-times (e.g., Mark 1: 15). With the Jewish apocalypticists, who were also often associated with radical fringe sects insisting on a return to fundamentals in a sinful world, early Christian texts explain that the end of history will take most people unaware, “like a thief in the night” (1 Thess. 5: 2).

Christianity developed the apocalyptic scripts of Judaism in several ways. The premise of Jesus’s incarnation of divinity radically altered older conceptions by making a single decisive event in the (at first recent) past the pivot around which all of time is organized; all of history before Jesus prepared his coming, just as history since is the script by which his return will be staged (Cullmann 1964). Pauline supersessionism gave rise to a tripartite division of history into phases before the law, under the law, and under grace. On the interpretive side, Christianity also pioneered to an unprecedented extent new symbolic and typological readings of history. Past figures and events – and not simply prophetic texts – came to be read as anticipatory prefigurations of later ones. Prophecy could occur “through things” and not only in sacred texts (cited in Funkenstein 1986: 250).

Arguably, this new development in monotheistic “historicism” forced a new kind of rationalism on interpretations of the past. Historiographic interpretations no longer merely

corresponded with initially puzzling scriptural passages but investigated the immanent direction of history and even its progressive drift. The *logos* posited by a Christian gospel as “the beginning” (John 1: 1) referred to God’s word; but it also gave evidence of the Hellenization of monotheism of the era, which could eventually make room for the demands of reason (as the Greek word *logos* also meant). Not simply an external guide imposed onto confusing events, providence was supposed to be legible in and through history. But this sort of rationalization truly flourished only later.

Post-biblical Variations

With the rise of normative, rabbinic Judaism in the first centuries of the common era, earlier apocalypticism was repressed, albeit never expunged, from the tradition. In Christianity, it had to be reinterpreted. Having been defined much more essentially as an apocalyptic cult, complete with new canonical texts with obscure outlines of the end (most famously, the Book of Revelation), the communities that would become the burgeoning Christian Church had to face far more squarely the hard fact that the most literal interpretation of its inherited and newly scripted apocalyptic predictions had been mistaken. Not the promises themselves, but only the overly simple readings they first attracted, had to be given up to explain and to interpret history’s continuation.

Church Fathers of the first Christian centuries like Irenaeus, Eusebius, and – above all – Augustine, were the pioneers of a “deapocalypticized philosophy of history” (Funkenstein 1993: 83). Incorporating Greek reflections on the conventional origins of human culture, these Christian authors had to reckon not simply with confusing political tides of the Roman Empire, but also with the originally foreign insights into the development of social complexity over time. But mostly it was the sheer failure of the apocalypse to materialize that forced a more philosophically developed “eschatology” to develop from the apocalyptic mentality, distinguished by its concern for the complexity of God’s designs, their penetration into the details of collective development in time, and eventually their evolutionary rather than revolutionary character. In their interpretive nuance and theoretical sophistication, these accounts were a far cry from bare scriptural precedent, especially from the simplicity of the apocalyptic mentality. Moreover, this new reflectivity involved both a substantive and a hermeneutic component: thinkers were forced not simply to add new detail to prophecy or to find inventive ways to correlate history with it; they also required a theory of interpretation both of scripture and events to make sacred history legible.

A disconcerting political episode – Rome’s fall – also proved a catalyst. In fact, it can even be argued that what made the western philosophy of history philosophical was its constant need to distance itself from the apocalyptic scheme in order to explain why God’s intentions were so extended and intricate in their slow and multifaceted realization. Jesus had criticized his opponents for their failure to understand history (“You know how to interpret the appearance of the sky, but you cannot interpret the signs of the times” [Matt. 16: 13; but cf. Acts 1: 7]). For this reason, the Christian philosophy of history forced upon itself the task of unraveling the mystery of God’s doings so as to justify them to men. Initially, some enthusiastic but naïve Christian progressivists had been willing to see Rome as God’s instrument, identifying the secular history of

imperial gradeur and expansion with the Christian script of salvation: Irenaeus proclaimed Rome the “fourth monarchy” of Daniel’s confusing prophecy (Rowley 1935). By Augustine’s time, the previously unthinkable had occurred: no sooner had the emperor Constantine proclaimed Christianity the official faith of the Roman empire, than it fell.

This apparent failure of progress motivated Augustine’s enterprise in *The City of God*. The true city, Augustine says, has to be utterly distinguished from worldly politics and events, so that the collapse of the dream of Christian empire by no means implies the falsehood of Christianity’s promises. Anyway, history had been full of horrors even before Rome came, and the constancy of human suffering confirmed the thesis of God’s rule rather than refuted it. Augustine’s student Orosius, at his teacher’s behest, filled in many of the details. But instead of being simply elsewhere, the city of God must also be seen as “on pilgrimage” here below. The idea of history as the forum of God’s intention, far from being utterly falsified by Rome’s fate, means that God’s designs were simply more complicated than once thought. Of the seven ages of the world (recapitulating the seven days of creation), Augustine argued, five have passed, during which the two cities were tightly linked; now, the sixth age is occurring in a new, more spiritual dimension essentially disconnected from events and therefore not so legible (see Mommsen 1951; Markus 1989; Günther 1993).

For Augustine, the hour was late, even if when exactly the plan of salvation would come to pass could not be definitively stated. God’s tarrying between the first and second coming is what made the long era of Christian history that followed in the “middle” ages. After the patristic discussions, however, philosophies of history considered the present as just one more phase in the senescence of the world. Yet in the high middle ages (especially in the twelfth century), alongside the continuing practice of composing barebones chronicles of events, speculation about the meaning and division of history flourished briefly in an extraordinary renaissance of symbolic or “typological” inquiry.

Though always admitting of confirmation by scriptural authority, this approach drew on and extended the earlier Christian breakthrough to making the philosophy of history analytically independent of scripture and read off from events in relative autonomy from any detailed biblical prediction. Otto of Freising, followed by numerous others, saw in the “revived” Holy Roman Empire Daniel’s fourth monarchy again (Chenu 1957: chs. 3, 5; Goetz 1958; Funkenstein 1966). But after Augustine, the crucial figure in whole canon of Christian historiosophic speculation was the Cistercian abbot Joachim of Fiore (ca. 1135–1202), whose new three-stage theory of history resonated down through the centuries (cf. Reeves 1976, 1969). According to this scheme – the basic categories came from scripture but it was extended to cover the totality of known history down to Joachim’s own day – there were three ages of the world, those of the father, the son, and the Holy Spirit. Supposing that all events, down to and beyond his own time, were part of an intelligible history of salvation, Joachim’s neo-apocalyptic promise of an imminent “third age” proved immensely influential. Such new prophecies regularly failed to come true. But others who were more moderate made the “theology of history” a permanent Christian philosophical enterprise (Ratzinger 1959).

Besides the rare imitator of the progressivist habits and typological hermeneutic of Christian philosophy of history – like Nachmanides – there was no obvious Jewish philosophy of history in this period. Judaism had no opportunity to link its fortunes to

Rome, and therefore needed no Augustine to recover from the flirtation. Stereotypically, and to some extent really, Christianity, even after the Roman disaster, came to understand itself as proving itself in and through history, while the Jewish experience became one not of progression and expansion but one of survival (Funkenstein 1993: ch. 4). Jews had no medieval historiographical or historiosophical renaissance of their own; there was no Jewish Joachim either.

All the same, this does not mean that medieval Judaism subsisted on ritualized and repetitious “memory” alone. There were longstanding assumptions about the nature of historical change built into the very Jewish practice of scriptural and legal interpretation. It was apparent to some Jews in the middle ages that their founding forebears to whom God spoke lived in very different historical circumstances, and their interpretive practices often relied on what has been called “the principle of accommodation” that allowed them to see their texts and their rules in correlation with the society of past times, thus requiring historical thinking.

The Jewish philosopher Maimonides argued, to take the best studied example, that the ancient but superseded institution of sacrifice had been once thought integral to the Jewish religion because the Jews of the earlier period of the religion could not be expected to break fully from the polytheistic ways of their neighbors and the less rational circumstances of the time. Maimonides suggested that monotheism, much as in the more full-blown Christian scripts, developed in and through a complicated history, with time being, as for Christians, the forum for its realization and perhaps perfection (Socher 1999). The implication was that, besides simply longing for the world to come or the messianic days predicted in the Bible and theorized in basic terms in early rabbinic culture, later Jewish traditions developed a version of the much more complex historicist philosophy of history in evidence in Christian circles.

Contrary to a once dominant thesis that the modern concept of progress depended on the suppression of religion, many studies have stressed the Christian invention and nourishment of the eventually secular idea that civilization constantly improves humanity’s lot, leading ever upward (see Bury 1920; Teggart 1949; Mommsen 1951; Tuveson 1964; Blumenberg 1974). In the early modern period, Christian philosophy of history, challenged but not overthrown by discoveries of new peoples, unknown languages, and deep time, led to accounts of “Universal History.” This set the stage for to the rise of the modern conviction that human history took the form of a compulsive and unidirectional sequence of discrete stages, this time without God’s visible hand as the external cause of events.

Modern Legacies

Far from immediately or simply taking the modern form of secular theories of progress, the Christian philosophy of history survived in overt form, in a wide variety of contexts. Joachimite scripts deeply informed, for example, the discovery and later conquest of the new world. Christopher Columbus understood his mission in part in terms of the “cosmic drama” of the medieval philosophy of history; and later Spanish clerical authorities drew on this canon to explain the event of discovery, often seen as beginning the third age, and to justify the process of conversion that would bring the

reign of spirit to fruition (Watts 1985; Phelan 1970). For their part, in North America, Protestants claimed a close coincidence between their Reformation and the discovery of the new world. With its roots in the specific version of covenant theology they brought with them, Puritans placed a providentialist stamp on the American enterprise that would prove difficult to wash away (Miller 1939, 1943; Hatch 1977; Winship 1996; Walsham 1999).

Christian philosophy of history, in its sundry forms, has remained a primary template for the assimilation of novel events down to the present (see esp. Reeves and Warwick 2001). The grounds of a modern reformist mentality which saw human history optimistically as a forum of Christian opportunity and of human self-preparation for salvation had been laid in the patristic period with the fusion of Greek and biblical thought (Ladner 1959; Jaeger 1961; Benz 1966). In spite of Augustine's worries about too exact a correspondence between God's will and human history, this optimism was revived in the early modern period, especially in some Protestant circles; and, in combination with other ingredients like the ancient Greek Stoic version of providence, it was secularized. On this theory, man had to establish the kingdom of god on earth in order to allow it to exist in heaven. But this was not just the case for the conscious striving for perfection. It was secularized as the claim that the forum of history was the scene of humanity's constant self-betterment, often behind its members' backs and in spite of their intentions (Klempt 1960). This (sometimes barely) secular theodicy has been seen at the root of a wide range of substantive accounts from the early eighteenth century of the historical process – from Bernard Mandeville and Giambattista Vico's theory that private vices cause public benefits through Adam Smith's vision of the common good guaranteed by an invisible hand, to Immanuel Kant's account of a "universal history" that, though driven by unsocial tendencies, results in cosmopolitan sociability, to G. W. F. Hegel's grandiose vision of the workings of spirit in history (Passmore 1970; Viner 1972; Dickey 1986). With its substantive logical and hermeneutic finesse, Hegel's philosophy of history, above all, perfected the transition from the hypotheses of scriptural prediction or God's intervention to the claim of a purely immanent dynamic explaining time's direction and meaning: "To him who looks upon the world rationally, the world looks rational in return."

As late as the mid-nineteenth century, a number of young Hegelians were calling for a third age in which the reign of spirit would mean a redeemed humanity (Breckman 1998: ch. 5). Heinrich Heine referred to them and others who envisioned future brotherhood, harmony, and peace as the outcomes of the slow workings of events. But sometimes it was thought that this result, as in the biblical apocalypse, might require a last battle or convulsive transformation. Originally a young Hegelian himself, Karl Marx violently rejected the politics of spirit in the name of "historical materialism," but he preserved the progressive arc, inexorable course, unwitting advancement, and redemptive end of the historiosophic script he inherited. Much of the mid-twentieth century work on philosophy of history by historians and social scientists was inspired by the Cold War attempt to understand the Soviet Union, apparently godless, as in fact driven by a "political religion" or even "political messianism." Often, this meant tracing its original form in the chiliastic scripts of Christianity in order to find its finale in an allegedly millennialist "leap into the kingdom of freedom" (Wilson 1940; Löwith 1949; Manuel 1965; Talmon 1960; Cohn 1961; Walicki 1995).

At the same time, versions of these historicist commitments sank deep into the liberal tradition too. Victorian social theorists were famous for their belief in necessary progress – often, as in Charles Darwin’s rhetoric, achieved through the motor of competitive self-interest (Butterfield 1931; Burrow 1966). For their part twentieth-century liberal elaborators of “modernization theory” shared similar commitments, and recently liberal capitalism, no less than communist socialism, could come to be seen as “the end of history” towards which the script of the past had always been heading (Gilman 2001; Fukuyama 1989). Even unyoked from the assumption of divine intention, the idea of history as a compulsive sequence of upward moving stages remains tenacious.

An equally interesting secularized outcome of the religious philosophy of history, however, was its contribution to the formation of the interpretive and methodological assumptions of modern historiographic reasoning. Of course, the tributaries that flowed into modern historiographic thought were many, especially Renaissance and post-Renaissance humanist traditions of antiquarian fact-finding and erudite philology, culminating in the *artes historicae* of the early modern age that were to be fused with Enlightenment philosophy to create modern historiography (see Momigliano 1966; Kelley 1970; Pomata and Siraisi 2005; Grafton 2007). But these, though essential, were hardly the sole source.

Christian philosophy of history and its Jewish analogue fostered the modern habit – shared both by professional historians and presumably by ordinary people, quite independently of their theological origins – of thinking historically in the sense of understanding things in the context of their times. On this account, the epoch of obsession with the script of history, and its division into different eras, inevitably fostered a sense of stark distinctions from age to age not just in political systems or in religious beliefs but also in overall social meanings. Only in the incubator of the Jewish and Christian philosophy of history did westerners learn to think that things depend for their meaningfulness on their place, and their moment, within the framework of history (Moyn 2003). Much has changed in the philosophy of history, to the point of making its original – and by far most enduring – western forms unintelligible as philosophy proper. But from both a substantive and an interpretive angle, they have left their legacy all the same.

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Muslim Philosophy of History

ZAID AHMAD

Introduction

Islam is a religion that has a strong sense of history. The Qur'an recognizes two major sources of information, nature and history. The Qur'an narrated stories about the past for the purpose of teaching lessons to the people. Historiography has always been tantamount to and considered one of the major components in the Muslim intellectual and literary tradition.

This general overview of philosophy of history and historiography within the context of Muslim traditions will discuss questions such as how did Muslim historians view and understand history? How and why did they compose, compile, and write historiography and for what purpose? What value did they give to history? How did they relate history to social change? How did they link historiographic composition to other literary genres?

Muslim or Islamic Philosophy of History?

What would be the proper terminology? Is it Muslim or Islamic philosophy of history? Of course, this question is an intricate one because there are ambiguities or gray areas between the two terms. Both terms refer to different facets of the subject.

The term Muslim refers to the person or people who adhere to the religion of Islam. It carries the historical, social, and physical meaning of the community and people whose religion is Islam. One might argue, of course, based on what is commonly perceived: whatever the Muslims do, they do it in the name of Islam. However, we must bear in mind that not everything the Muslims do could be taken as representing or in accordance with the principles of Islam. There are many instances where the Muslim conducts him or herself not in conformity with the basic injunctions of Islam. In this case, that particular conduct, although carried out by a Muslim, should not be taken as Islamic because it does not comply with Islamic principles. The Muslim carried out this conduct on his own wish and free will, which in no way could be connected with Islam.

Conversely, when we refer to something as Islamic, we refer to Islamic principles derived from the teachings of the Qur'an, the *sunna* and the other authentic Islamic sources.

The Muslim, with appropriate qualifications, expresses views and interpretations on aspects of religious teachings and principles. It is quite inappropriate to categorize whatever comes from a Muslim as Islamic because it is humanly fallible, subject to misrepresentations, while the Qur'an is considered theologically divine, sacred, and infallible.

On this basis, it is safer and more logical to refer to a Muslim rather than an Islamic philosophy of history. Although in most cases, the views of Muslim scholars and intellectuals can be considered as representing Islam, it is more appropriate to associate the views and opinions with the person rather than with the religion. This is more reasonable since there would be variations, differences and dispute among scholars. However, in the course of this discussion, the term Islamic philosophy would also be used occasionally and interchangeably whenever appropriate, especially when the discussion deals particularly with the questions of theological and doctrinal matters.

Islamic Concept of History

Is there an Islamic concept of history? To trace the Islamic concept of history is actually to trace the root of the idea of history in Islam. As the primary theological and doctrinal sources of Islam, the texts gathered in the Qur'an contain many narratives about events of different societies over different ages. In fact, one of the *suras* (chapters) of the Qur'an, *sura 28* is called *Sura al-Qasas* (meaning: history or narration). Apart from this, other verses elsewhere in the Qur'an contain various forms of narratives about ancient generations and civilizations. The purpose is to give lessons to the people. The Qur'anic term for this is '*ibra* (example). In one of the verses, the Qur'an reads, "In their history, there is a lesson for those who possess intelligence" (Qur'an 12:111). The term *ibra* is also used elsewhere in slightly different context such as in the phenomena of cattle (Qur'an 16:66) and the succession of day and night (Qur'an 24:44). However, the ground is the same, i.e., it carries the principles of education and lesson. In this sense, in so far as the Qur'anic notion is concerned, the concept of '*ibra* can be regarded as the most important precept to the later development of the idea of history in Islam. Apart from '*ibra*, there are other concepts that imply similar implications. The concept of *ayat* (signs) for example in the verse "We have set it up as a lesson [*ayat*]. Do any of you wish to learn? (54:15) and "In Joseph and his brothers there are lessons [*ayat*] for the seekers" (12:7) and the concept of *dhikr* in "This should be a lesson [*dhikra*] for everyone who possesses a mind, or is able to hear and witness" (50:37). Ibn Khaldun was one of the earliest to properly use and utilize this concept. This can be seen from the very title of his magnum opus, the *Muqaddima li-Kitab al-'Ibar*.

Second to the Qur'an is the Prophetic tradition, *sunna* or *hadith*. This is about the sayings and the deeds of the Prophet. The Prophetic tradition gave a strong impetus to the later development of historiography in Islam. Theologically speaking, the deeds and sayings of the Prophet are regarded as the second source next to the Qur'an. It is vital for Muslims to precisely record and document these Prophetic traditions for later generations to study and derive from their religious teachings and injunctions.

Islam, as inspired by the Qur'anic notions, deals with history, with the past, from the perspective of prescribed laws and principles. It implies that all events are governed by a set of laws known as the law of God (*sunnat Allah*) and at the same time obey the basic natural principles of cause and effect. The Qur'an also emphasizes the principle of free choice (*ikhtiyar*) and free will, according to which human beings have the ability and freedom to choose. Human free will should be understood within the scope of a larger and broader spectrum of universal divine will. There are certain limitations that human free will cannot surpass.

Under the principle of free will, human beings have the ability to choose, decide on their conducts and actions. God will hold them responsible for it. God uses events such as successes and failures, victories and defeats, prosperity and decay, to distinguish the good from the bad. People are tested in this life so that they should take the opportunity to show their worth, to prepare for the next life, which is eternal. Certainly, the process of testing requires a person to possess free will and freedom of choice. This would enable him to determine and to choose between what is lawful and what is unlawful, what is good and what is bad and above all, what is permitted and what is prohibited. The concept of *khilafa* (vicegerent of God) and the principle of free will express these ideas.

Development of Muslim Philosophy of History and Historiography

The origins of historiography in Islam were doctrinal and theological. Historiography was theological because it was considered as another category of authentic religious source of laws, values, and religious rituals and practices. Biographic stories about the Prophet and his tradition namely the *sunna* and *hadith* fit well in this category. From an Islamic point of view, history is also seen as the practical manifestation of the divine plan. Therefore, historiography cannot be perceived as merely a subject acquired for the sake of knowledge alone; it is something that carries a religious thrust in it. In Islam, the spirit and foundation of historiography is to offer lessons for later generations. For this reason, Muslim historiography has had a very close connection with the general development of Islam, including its doctrines, law, and jurisprudence.

The Prophet Muhammad is the central figure in Islam both theologically and historically. His personality, sayings and deeds are regarded as the primary source of Islamic law and jurisprudence next to the Holy Book, the Qur'an. In Islam this doctrine is firmly coined in the concept of *sunna* (exemplary conduct of the Prophet). Because of this concept, there was a crucial need to acquire and record authentic reports about his life and activities. These reports were gathered, compiled, and preserved for reference by the later generations.

Due to the significance and the theological nature of the Prophet's biography, there was a pressing need to develop a particular method to ensure the accuracy of the reports. Since the narratives and reports were obtained and then collected through a chain of reporters or transmitters, an oral tradition, the trustworthiness and reliability of these transmitters needed to be thoroughly scrutinized. The role of transmitters was so important because if one of the transmitters was not fully reliable, according to a certain set of prescribed standards, the whole report was considered weak. For this

reason the traditionalists and historians had to formulate approaches and methods for collecting and authenticating the accuracy of reports and the information transmitted through them. This generated a new science called *'ulum al-hadith* (the science of Prophetic tradition).

Muslim historians and jurists set a standard that needed to be met before reports were to be accepted as authentic. This exercise required an extensive knowledge of the background, history, and personality of the transmitters. In *'ulum al-hadith* this procedure is called *al-jarh wa al-ta'dil* (disparaging and authenticating). This method is used to determine the level of reliability of the transmitters of the traditions (Ahmad 2003: 42–3). This development of the science of transmission is an important milestone for the later development of historiography and historical traditions in Islam.

Many modern historians are of the opinion that the biographical literature is the corner stone for the subsequent development of Muslim historiographic tradition. Franz Rosenthal (1968: 101) for example, clearly acknowledges that the biographical literature had its share in the development of Muslim historiography from the very outset, and eventually achieved a dominant position in it. Joseph Somogyi (1958: 34) agrees, regarding the biography of the Prophet (*sira Rasulallah* and *maghazi*) as the original form of Muslim historiography. He proposed Ibn Hisham (d.218/834) as the prototype and eponym of the *sira*. Others, suggest an earlier historian, Muhammad Ibn Ishaq (d.151/768) as the leading authority in *sira* literature (Harun 1979: 11). A prominent orientalist Giorgio Della Vida suggests an even earlier historian 'Urwa ibn al-Zubayr (d.94/711–12) as the initial biographer of the Prophet Muhammad (*First Encyclopaedia of Islam*, hereafter EI1, 1987, 441), whereas a renowned Iraqi historian Aziz al-Duri (1983: 27) gives credit to Ibn Shihab al-Zuhri (d.124/742) as the proponent of *sira* literature.

Taking into account all these diverse views, it should be possible to safely conclude that the development of biographical literature in Islam can be dated no later than the second century of the Muslim era, *Hijra*.

Two Muslim Philosophers of History: Ibn Miskawayh and Ibn Khaldun

This section deals with the ideas of the two most prominent figures in Muslim philosophy of history, Ibn Miskawayh and Ibn Khaldun. The writings of these two men have turned out to be the major source for Muslim philosophy of history conceptually and substantially. These two major Muslim thinkers turned historiography into a new, lively, and dynamic subject rather than just a received narrative with stories, full of unfounded tales of myths and legends. These men have initiated a new historiographic outlook as a scientific and rational subject. They have set an important principle that any historiographic description of an event must follow procedures of historiographic scrutiny before it can be recognized as historical fact.

Ahmad ibn Muhammad ibn Miskawayh (d.421/1030), also popularly known as Ibn Miskawayh was a poet, moralist, and philosopher. He was an eminent historian in Persia during the Buwayhid era, and held prominent office in the Buwayhid administration. This hands-on experience, as government official and a key player in social and

political activities, together with his official responsibility, put him in a better position to write historiography. This background influenced his stance and outlook on historical reports and interpretations of events. His historiographic masterpiece is the *Tajarib al-Umam* (*The Experiences of the Nations*), in six volumes.

As a moralist and philosopher, Miskawayh presents a distinct perspective in *Tajarib al-Umam*. Historiography is no longer merely a factual and chronological storyline but a critical account of the past (Siddiqi 1971: 21). This is clearly shown at the preface to his work:

When I look at the narratives of nations, and the chronicle of kings, and the accounts of cities, and read books on history I found in them such things which we can gain experience concerning events the like of which have always been happening and may also be expected to happen in the future e.g., the birth of states, the rise and growth of countries, followed by the decline in their fortunes and then the person who sets aright their conditions and brings them back to the state of perfection and the person who, through his negligence, hastens their decay, all these things and along with these the discussion of political matters concerning the population of the cities, union and the cooperation of the people, and correction of the intention of the army . . . That is why I compiled this book and gave it the name of "Experience of the Nations" from which mostly the people who are deeply involved in the affairs of the world, viz., the local emirs, military generals, statesmen, administrators and even people of all classes and ranks can get benefit. (Miskawayh 1909: 4)

Miskawayh voices his dissatisfaction from the way history was presented by earlier historians. Upon subjecting ancient historical sources to scrutiny, he finds that those reports are full of information that contains entertaining stories and idle talk that had no use except for making one fall asleep (Miskawayh 1909: 4). Since he wants to use the experiences of nations to serve as examples for later generations, he excluded all kinds of unsubstantiated information from his work.

Miskawayh's conception of history rejects predetermination and its conception as an expression of providential design or purpose. For this he writes,

I am beginning with reporting the historical information about the time after the deluge, because the information about events before it can be little trusted and also because that information is in no way useful for accomplishing the professed purpose of the work. For the very same reason, we did not undertake to report the miracles of the prophets and their political achievement, because the people of our time can gain experience for the task they will face in the future only from human behavior that is unconnected with anything miraculous. (Miskawayh 1909: 5ff)

As Siddiqi (1971) rightly points out, this assumption marks the inauguration of the philosophy of history and its emancipation from the theological interpretation of history in the world of Islam. With it, historiography becomes a mean for the realization of human interest and purposes. History for Miskawayh is a movement of human purpose and counter-purpose. We can gain experience from it for routing through similar situations in the future.

Muslim philosophy of history reached its zenith in the work of Abdul al-Rahman Ibn Khaldun of Tunis (d.808/1406). Ibn Khaldun, who has also been honored as a father

of sociology, developed historiography into a new science of society, namely *'ilm al-'umran*. In the hand of Ibn Khaldun, historiography became a critical and essential part of the study of the rise and fall of civilizations and societies.

Like his predecessor Ibn Miskawayh, Ibn Khaldun was unhappy with the development of Muslim historiography prior to his time. He found that most of the documentations and reports on historical events were not gathered properly and would satisfy methodical requirements for reliable historiography. This followed a lack of scientific procedures or appropriate methodological devices to verify the correctness of historiographic reports. As a result, these reports had factual flaws. He realized that the Muslim scientific community at that time was in dire need of a new paradigm, a new approach to the study of history. Hence, the birth of *'ilm al-'umran* should be viewed as Ibn Khaldun's attempt to bridge the gap or provide the "missing link" in historiographic procedures and to fill up the methodological vacuum.

Although for some scholars, *'ilm al-'umran* is just a system of sociology, for many others, there is much more to it. This "new science" is indeed a system of sociology aiming to explain the nature, the process, and structure of human social and political organization, but Ibn Khaldun's magnum opus, the *Muqaddima* or Prolegomena, shows that it encompasses more than a mere system of sociology. It stands as general framework of theoretical as well as applied science of human society. In this way, *'ilm al-'umran* may be seen as operative and practical approach to historiographic studies of human society.

A forerunner of sociology, Ibn Khaldun, proposes a unique approach in treating historiography as part of a science of society with its own reasoning procedures. He treated historiography as a science and not merely as a narrative. He wrote historiography in light of a new method of explanation and reasoning and developed it into a proper and systematic social philosophy.

Despite his active life in political and public affairs, he managed to produce a monumental historiography, dealing particularly with human social development in general. He perceived the historical process as the outcome of interactions between human society and the physical environment. History is a process in which human communities, societies, and institutions transform continuously. History deals with the dynamics of social affairs, which move in a constantly changing cycle. Ibn Khaldun believed that the historical process is dominated by two essential groups of people, whom he termed *badawi* (the nomads) and *hadari* (the townspeople). This becomes the foundation of his theory of rise and fall of human civilization.

The dual classification of *'umran* into *badawi* and *hadhari* parallels sociologist Ferdinand Tonnies' (d.1936) concepts of *gemeinschaft* and *gesellschaft*. From *'umran* Ibn Khaldun entered into a broader discourse about social and indeed civilizational factors, elements and processes. Human society develops from simple to complex, from *badawi* to *hadari*. Historiography should describe the progression line, composed of a myriad of important historical events, experiences, affairs, and incidents. Ibn Khaldun's historiography collects and classifies data, connects and explains it, and then comes up with universal judgments. In so doing,

A philosopher of history *par excellence*, Ibn Khaldun's works possess remarkable originality, criticizing and analyzing history. He rejects the perception of social historical events

as the outcome of a chance. Before the social sciences or European substantial philosophy of history, he argued that social history obeyed rules of its own that had to be discovered and applied in the study of society, civilization, and history.

In his famous Prolegomena, Ibn Khaldun deals with society and its origin, sovereignty, the birth of towns and villages, trades, ways of making a living, and sciences. This is the best and most important part of the book in which he sketches his philosophical outlook on history, analyzing politics, economics, society, and history with outstanding originality and brilliance. He associates the rise of civilization with the growth of villages and towns. He adopts the ancient Aristotelian concept of the "political" nature of human beings. The center of his theory is man's faculty of rationality (Ahmad 2003: 160). Ibn Khaldun is undoubtedly among the first to attempt to explain the evolution and progress of society. He explains the characteristics of race, climate, and the means of production, and how they affect the formation of man's mind and sentiment, as well as the formation of society.

Another key component of Ibn Khaldun's historiography is the emphasis on rationalism. He uses a logical apparatus and rational empirical assumptions as conceptual and theoretical foundations for his new science. He correlates rationalism with civilizational cycles. He asserts that rationalism may bring up civilization and it may also bring it down. For example, rationalism may lead to social corruption. He develops a rationalist approach in understanding socio-cultural phenomena, using classical logic to understand socio-economic realities underlying cultural experience and temporary events. He associates the good cultural life with the interrelationship that must be established between God, the world, and the Hereafter. Ibn Khaldun's influence in the fields of sociology and historiography was tremendous, chiefly because of his great emphasis on reason and rationalism.

Muslim Philosophy of History and Encounters with the West

The philosophies of history and historiography in the Muslim world after Ibn Khaldun in the fifteenth century were not so remarkable. However, there were still reasonable developments on a moderate scale within a limited scope.

This situation continued until the late nineteenth century when Muslim scholarship entered a new phase of its development. This was marked by an increasing interest among western scholars in the study of Islam, Muslim society, and traditions. A growing number of western scholars who engaged in this enterprise had subsequently created a new wave of scholarship, orientalism (for a fuller and critical account on orientalism, see Said 1994). Orientalism comes with a new literary style, offers epistemological and methodological assessments and critiques of Muslim literary history and historiography and frequently deconstructs the already established tradition. The orientalist even goes to the extent of questioning the authenticity of the transmitted reports, the tradition, using methods developed initially in Germany in the eighteenth century for the analysis of the Christian scriptures (see, for example, Shacht 1950).

Muslim scholars and academics reacted to orientalist scholarship. For example, Muhammad Mustafa Azami has published *Studies in Early Hadith Literature* together

with *On Shacht's Origins of Muhammadan Jurisprudence* as a direct reaction to Joseph Shacht's work on Islamic law (Azami 1987). There are three types of responses. First, some western educated Muslims fully adopted and applied oriental studies methods in their study of Islamic tradition, and presented their studies and analyses in a "westernized" fashion. Second, the so-called "fundamentalists" totally reject orientalism, and embark on sharp criticisms to launch intellectual battles, on religious and cultural grounds. For them orientalism is a stranger to Islamic scholarship and it is just another form of intellectual colonialism. In their opinion, it is more of a political than an intellectual or scholarly movement. Third, some rational scholars appreciate orientalism positively and attempt not only to bridge between tradition and modern scholarship, but also to take advantage of a dialogue for the benefit of the development of better Muslim scholarship. This group can be considered as modernist, its members are mostly trained and educated in the west but have a strong religious and cultural affiliation. They adopt a more accommodative stance but are selective and critical when dealing with orientalism. Overall, this group can be considered the mainstream in contemporary Muslim scholarship.

Conclusion

This chapter has portrayed a general overview of Muslim philosophy of history. It has touched on various questions including its origin, development, and debates. We can highlight some essential points here. First, it is important to really grasp the correct perspective in view of understanding and differentiating between the terms Islamic and Muslim. In this chapter, we have suggested that the term Muslim should be used as general term while the term Islamic might be used in the appropriate circumstances. Second, the primary Islamic sources namely the Qur'an and the *sunna* are the chief sources and become the precept to the development of historiographic ideas and tradition in Islam. Third, Muslim philosophy of history should be understood as the attempt to inquire and grasp the very meaning and concept of history in Islam based on the best interpretation of Islamic principles. Fourth, in Islam, history is important because it serves both religious and social functions. History serves as source of Islamic doctrines, law, and jurisprudence as well as ethical values. Fifth, although the Qur'an comes with peculiar principles regarding history, philosophers and the historians developed their own ideas and interpretation and were influenced by their own backgrounds and circumstances. Sixth, Muslim philosophy of history in one way or another has also been influenced by the previous ancient thoughts and traditions particularly that of the Greeks. Finally, yet importantly, orientalism as a new type of scholarship in the Muslim world has had a certain impact and influence upon the modern Muslim philosophy of history and historiography.

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Vico

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In recent decades the work of the Neapolitan thinker Giambattista Vico (1668–1744) has attracted a great deal of attention from scholars in the humanities and social sciences. Vico is now celebrated not merely as the author of one major *New Science* but as the instigator of many modern (and postmodern) new sciences: whereas older admirers appraised his achievements in the classic fields of philosophy, Latin philology, or Roman Law, contemporary scholars discern in his writings much more daring premonitions of “symbolic anthropology,” “historical genealogy,” “developmental psychology,” “narratology,” and “sematology.” The current revitalization of Vico’s legacy is also evident in modern philosophy of history and historiography, where Vico’s peculiar notions and formulations have inspired major studies on the historical and theoretical origins of such key terms like the “hermeneutical circle” (Gadamer 1975), “hypothetico-deductive method” (Pompa 1975), “reconstructive imagination” (Berlin 1976), or “contextual reasoning” (Funkenstein 1986). “The Vico Road goes round and round to meet where terms begin,” as James Joyce put it in *Ulysses*, indicating that, for Vico and his readers, the meaning is always in the beginning, or, in Vico’s own term, “coming-into-being” (*nascimento*).

Vico published several books in his lifetime: two tracts on pedagogical and philosophical matters which appeared under the titles *On the Study Methods of our Time* (1709), and *On the Most Ancient Wisdom of the Italians* (1711); the historical biography *The Deeds of Antonio Carafa* (1716); and the compendious study in Roman law, *Universal Right* (1722). In 1725 he published the first edition of his major work, *The New Science*, on which he continued to labor, constantly rewriting and revising the text, and eventually published two more editions in 1730 and in 1744. In 1728 he wrote his *Autobiography*, in which he recounts his intellectual development. Some of his inaugural orations and lectures on rhetoric at the University of Naples, as well as other occasional compositions, were published after his death. Among his admirers were some of the greatest authors and scholars in our times: James Joyce, Georges Sorel, Benedetto Croce, Erich Auerbach, Isaiah Berlin, Hayden White, and Carlos Fuentes. Yet, Vico’s *New Science* remains one of the most difficult texts in the canon of modern cultural history. The full title of the book – *Principles of a New Science by Giambattista Vico concerning the Common Nature of the Nations* – evokes both its subject-matter and

enigmatic character. The following observations will thus examine its key terms from theological, philosophical, philological, and historiographical perspectives.

Theological Convictions

According to Vico's own testimony in his *Autobiography*, he was throughout his life and in his writings most faithful to the fundamental ethical and historical doctrines of the Catholic Church. He firmly believed that religious observation, from above (providence) as well as from within (obedience), was indispensable to the preservation of human life and history. He reiterates this theological conviction in the very last words that he ever wrote, which comprise the final paragraph of the *New Science*: "To sum up, from all that we have set forth in this work, it is to be finally concluded that this Science carries inseparably with it the study of piety, and that he who is not pious cannot be truly wise" (*New Science*, hereafter NS, 1112. All references to the *New Science* in the text are to Vico (1968), indicating in parentheses NS and the standard paragraph number). Vico may have initiated or even invented, in all but name, the hermeneutic methodology of the modern human sciences, but, as far as he was concerned, the main aim of his work was to counter certain "modern" theories of the natural origins and development of man, society, and history, which proliferated in his time. His main adversaries were radical thinkers like the "Epicurean" Hobbes or the "Stoic" Spinoza, who ascribed all human actions and creations to sensual motivation, and thus came to perceive of man as thoroughly egotistic, and of society as utterly naturalistic. Vico duly perceived behind these mechanistic or pantheistic modes of explanation a secular and ultimately atheistic conception of reality. Other adversaries were contemporary geographers, who argued that peoples in the new world, let alone the world itself, were much older than the reckoning of biblical chronology, and fellow antiquarians such as John Marsham and John Spencer who sought to prove that ancient Egyptian religion was prior – and even superior – to that of the Hebrews. Above all, Vico fought skeptical thinkers like Pierre Bayle who used the new critical discoveries and theories to argue "that peoples can live in justice without the light of God" (NS 334).

In his *New Science* Vico sought to save faith not so much *from* the new scientific methodology, as *through* it. His direct engagement with the scientific discoveries and theories of his time enabled him to forge an alternative and more positive theory of natural sociability, and ultimately a whole new science of human creativity, which reaffirmed the old Christian principles of faith in terms and methods culled from the new human sciences. Already in the opening paragraph of the *New Science* Vico declares that "a chief business of this Science" would be to construe "a rational civil theology of divine providence" (NS 2), that is to say a scientific, and not beatific, demonstration of how divine agency has operated in human life and history through certain "civil institutions" that men themselves have created, albeit without any rational intention or comprehension. Whereas Hobbes and fellow natural law theorists assumed that men were able to overcome the nefarious state of nature and attain civil society by means of their rational capacities, Vico would argue that the biblical narration of the Fall offers a much better explanation to the primal condition of men as being ruled by natural

and immoral passions, and also reveals how mankind has actually overcome this predicament by eventually rediscovering God. He postulated that all men must have gone through the same mental stages or “ages” of development:

This New Science or metaphysics, studying the common nature of nations in the light of divine providence, discovers the origins of divine and human institutions among the gentile nations, and thereby establishes a system of the natural law of the gentile nations, which proceeds with the greatest equality and constancy through the three ages . . . The age of the gods . . . The age of the heroes . . . The age of men, in which all men recognized themselves as equal in human nature. (NS 31)

In his *New Science* Vico deals almost exclusively with the first two “ages,” because these were, in his view, the decisive phases in the history of mankind. Following on his axiomatic postulation that “theories must start from the point where the matter of which they treat first began” (NS 314), Vico sought to ground his theory of human history at the point where it actually began, namely when men began to think and behave humanely. He found a “common ground” between the old biblical stories and the new anthropological or hypothetical theories on the origins of humanity in the account of the descendants of Ham and Japheth, the errant sons of Noah, who, having lost during the Flood the idea of God (which the Hebrews, the descendants of Shem, still possessed), had to regain it by their human, all too human, mental capacities. The imaginative reconstruction of their adventures culminates in a passage which heralds the main theological as well as methodological lessons of Vico’s *New Science* of humanity:

In their monstrous savagery and unbridled bestial freedom there was no means to tame the former or bridle the latter but the frightful thought of some divinity, the fear of whom is the only powerful means of reducing to duty a liberty gone wild. To discover the way in which this first human thinking arose in the gentile world, we encountered exasperating difficulties which have cost us the research of good twenty years. [We had] to descend from these human and refined natures of ours to those quite wild and savage natures, which we cannot at all imagine and can comprehend only with great effort. (NS 338)

Modern scholars have commonly, and quite rightly, appraised the latter part of this oration as essential to Vico’s “hermeneutical revolution” in the human sciences, as if he thereby intimated some major modes of investigation in historical anthropology, social psychology, or comparative epistemology. Yet as the first part of the oration, and the whole book, make clear, these and other methodological innovations in the *New Science* derived from, and meant to reaffirm, his deep theological convictions.

Philosophical Assumptions

Vico’s assertion that it was possible, even if only “with great effort,” to “descend” into and “comprehend” the mentality of people in other times and places, rests on certain philosophical assumptions about the scientific validity of human knowledge. His principal assumption has become known as the *verum-factum* principle, namely that “the true” (*verum*) is identical in meaning with that which is “made” (*factum*), implying that

men could really or “truly” know only what they themselves have made or done. Vico developed these assumptions in his early tracts of 1709 and 1711, where he sought to rebut the dominant Cartesian philosophy which confined all valid knowledge to the critical analytical procedure whereby the *Cogito* refines ever more clear and distinct ideas of itself as *res cogitans* and, by further logical deductions, of the existence of other *res extensa* and of God. Vico denied that the *Cogito* could attain such pure knowledge, either of itself or of external objects, because the very nature of the human mind, bound as it is to sensual and experiential impressions and to such conventional and even irrational modes of cognition like words and images, renders the *Cogito* only conscious or certain (*certum*) of its existence. As Vico would have it, this is a mere psychological certainty that does not prove or make the existence of the self, let alone of other bodies and entities, absolutely true (*verum*) on strict scientific standards of truth. Moreover, spurred by his specific academic vocation, Vico countered the rationalistic ideology and methodology of Cartesian philosophy by exposing the sensualistic and linguistic sources of reason itself and, a fortiori, of the rhetorical, rather than logical, construction of human knowledge. In the *New Science* he redefines that “common-sensual” form of human knowledge as no more than a “consciousness” (*coscienza*), made up of “certain” plain truths or intuitions that men acquire by experience and practice of everyday life, through the languages and other “vulgar traditions” of their particular historical community. As such, this “consciousness” has been dismissed by philosophers since Aristotle as partial, circumstantial, at most prudential, but definitely not up to the standard of proper scientific “knowledge” (*scienza*) that consists in general theories of “what is true of things” (NS 137–8).

Vico’s major philosophical achievement in the *New Science* was to have turned this apparent epistemic fallibility of human knowledge into the main source and standard of all truths. Setting out from the general epistemological observation that “human choice, by its nature most uncertain, is made certain and determined by the common sense (*senso comune*) of men with respect to human necessities or utilities” (NS 141), namely by habitual reactions and considerations, he eventually came to see that “common sense” is not really such an uncritical manner of thought as Descartes and other rationalists had made it out to be, but is in fact sensible, reliable, usable, and even veritable in the pragmatic sense of the term: “Common sense is judgment without reflection, shared by an entire class, an entire people, an entire nation, or the entire human race” (NS 142). Hence, whereas in his earlier writings Vico had argued that “true knowledge” through conventions was possible only in mathematics wherein we deal with definitions, axioms, and postulates – all artificial objects that men themselves have made, in the *New Science* he transposed the notion of *verum* as *factum* from pure conventions (such as geometrical figures) to such cultural and social conventions like languages, myths, laws, states, and all other “civil institutions.” He was able to show that the “certainties” of *coscienza* and the “truths” of *scienza* were different yet equivalent productions of the same mental operation, whereby men come to believe or accept as “true” what they themselves have made or done.

The invention of a new synthetic category of *scienza nuova* signifies Vico’s subsequent and more radical realization that, inasmuch as “the institutions having to do with human affairs are more real than points, lines, surfaces, and figures are” (NS 349), the pragmatic certainties of *coscienza*, however atavistic and even mythic, are even more true

than the analytic verities of *scienza*, and could yield a whole *New Science* of humanity. In terms that were just emerging at the time, but have since become cardinal to the modern human sciences, the essential “truths” in human life and history are more “cultural” than “natural,” and more “communal” than “personal.” As Vico would argue against the rationalists and fellow modern social scientists, what motivates primitive and combative people to “live in justice and to keep themselves in society” (NS 2) is not rational calculation and contractual obligations, but deep emotions, creative imagination, and mythic traditions in which the people believe – and live.

Having come to realize that practically all our “civil institutions” are as artificial and conventional as are geometrical forms, and that consequently our knowledge of the human world results from what is equally an essentially constructive activity, Vico arrives at the conclusion that we know this world because, and only in as much as, we still make or share its constitutive ideal fictions, its myths. He celebrates this seminal illumination in some memorable words:

But in the night of thick darkness enveloping the earliest antiquity, so remote from ourselves, there shines the eternal and never failing light of a truth beyond all question: that the world of civil society has certainly been made by men, and that its principles are therefore to be found within the modifications of our own human mind. Whoever reflects on this cannot but marvel that the philosophers should have bent all their energies to the study of the world of nature, which, since God made it, He alone knows; and that they should have neglected the study of the world of nations, or civil world, which, since men had made it, men could come to know. (NS 331)

Vico claims to have discovered the “truth” about the “civil world” (*mondo civile*) – how men had made it and why, therefore, men could come to know it – on the assumption, which bears all the marks of “revelation,” that the “modifications,” or mental configurations, by which men in “earliest antiquity” had actually made *their* “world of civil society,” are the “principles,” or foundational “human institutions” (*cose umane*), of *our* world of civil society, and are recognizable as such “within” our own human modifications, namely in certain primal notions of humanity that have made up and sustain civil society. These “principles of humanity” are counter-analogous to Newton’s *Principia* of 1687, and are probably modeled on them. Just as Newton based his “natural philosophy” on the discovery of some basic physical properties and forces – like cohesion of bodies, inertia, or gravity – which form and govern the movements of all objects in natural reality, so too did Vico seek to discover “in the deplorable obscurity of the beginnings of the nations and in the innumerable variety of their customs” certain universal “human institutions” which have likewise regulated the affairs of men in social life and history. In order to accomplish this difficult task we must indeed “descend into” and “enter” the “minds” of the historical agents whom we study. Yet, not by any mystical feats of intuitive “empathy” or speculative identification with “those quite wild and savage” brutes, whom indeed “we cannot at all imagine,” but rather by methodical investigation of those mental expressions, or “modifications,” which we “can comprehend only with great effort.” Vico assigned this task to “philology,” the venerable art of interpretation, which he transformed from an ancient discipline of reading classic texts and Holy Scriptures into a new science of understanding human nature and whole cultures.

Philological Interpretations

Vico believed in the absolute veracity of the Scriptures. Yet, he realized that to counter the Epicurean philosophers on their own secular terms, the profane historiography of the gentile nations, and above all the myths in which they recited the genealogy of their deities, he must present a better story of how natural men, deprived of both reason and revelation, were able to transform themselves by their own poetic fictions:

From these first men, stupid, insensate, and horrible beasts, all the philosophers and philologists should have begun their investigations of the wisdom of the ancient gentiles . . . And they should have begun with metaphysics, which seeks its proofs not in the external world but within the modifications of the mind of him who meditates it . . . Hence poetic wisdom (*sapienza poetica*), the first wisdom of the gentile world, must have begun with a metaphysics not rational and abstract like that of the learned men now, but felt and imagined as that of these first men must have been, who, without power of ratiocination, were all robust sense and vigorous imagination. This metaphysics was their poetry, a faculty born with them . . . and for which they were called “poets,” which is Greek for “creators.” (NS 374–6)

On these premises Vico devotes the whole of Book II of the *New Science*, by far the largest in the entire work, to philological interpretations of that “poetry.” Though his ultimate reason was to establish the priority and superiority of the biblical stories over all the pagan historiographies, he produced in these “philological proofs,” however inadvertently, some of the most original discoveries and theories of the *New Science*. Among these was a new theory of the origins of language that defied not only the specific explanations but also the basic presumptions of the main philosophical schools – be they the “idealism” of the Adamic–Platonic theologians, the “conventionalism” of the Aristotelians, or the “naturalism” of the Epicureans. Against all these, Vico put up the notion of “expressivism.” He rejected the common assumption that the main function of language is cognitive or descriptive, that it served men merely to identify and specify things in the world. He argued instead that language is primarily expressive and creative, that its main functions are to articulate emotions and notions, and to signify things in the world by imbuing them with meaning. Accordingly, Vico supplies all kinds of etymological, rhetorical, philological, and historical examples to prove that the first language of the “first men,” children and primitive people, has been poetic, metaphoric, and mythic (NS 34, 400–455).

Another major new theory that emerged from Vico’s philological interpretations is “mythology.” Vico revokes the traditional conceptions of classical myth as “false” by redefining it as the “true narration” (*vera narratio*) of history in the age of poetry:

It follows that the first science to be learned should be mythology or the interpretation of fables; for, as we shall see, all the histories of the gentiles have their beginnings in fables, which were the first histories of the nations. By such a method the beginnings of the sciences as well as of the nations are to be discovered, for they . . . had their beginnings in the public necessities or utilities of the peoples and were later perfected as acute individuals applied their reflection to them. (NS 51)

The observation that the beginnings and ultimate meanings of all myths lie in certain “public necessities and utilities” of all men in all societies, confined the range of his philological interpretations to the historical origins and functions of classical mythology. On these grounds, he rejects the allegorical and other philosophical interpretations of classical mythology as utterly anachronistic, a typical fallacy which he dismissed as “conceit of the scholars, who will have it that what they know is as old as the world” (NS 127). Such fallacies of interpretation have been particularly harmful in Homeric studies, most notably in the so-called the “Quarrel of the Ancients and the Moderns” over the aesthetic qualities of Homer’s epics. Both parties were equally guilty of not paying due consideration to the concrete historical conditions in which Homer must have lived and could have produced his work. Vico uses his new conception of mythology to reassess the Homeric question, and he sums up his inquiries and new theories in book III of the *New Science*, suitably titled (not by Vico) “Discovery of the True Homer.” His main conclusion was that the Homeric poetry, replete with such awesome characters like Achilles, Ajax, or Odysseus, reflects the mentality of the “heroic” age in Greek history, which, in Vico’s terms, means also the most “barbaric” age (NS 792–805). Vico doubted whether the same person could have composed both the *Iliad* and the *Odyssey*, and, moreover, whether Homer ever existed as a person, arguing that inasmuch as the creation of mythology is a collective, objective, and largely anonymous process then it is likely that “the Greek peoples were themselves Homer” (NS 875).

“The Discovery of the True Homer” exemplifies the rich potentialities of Vico’s philological methodology. Erich Auerbach appraised it as the first and best elaboration of “aesthetic historicism,” namely of the anti-classicistic realization that there are no, nor could there ever be, absolute norms and forms of beauty because all works of art are cultural expressions of particular historical societies, and must be judged accordingly (Auerbach 1949). Other scholars discern in Vico’s critical reflections on Homer a deeper and more radical paradigmatic shift in the conception of human nature. Most Enlightenment thinkers believed, with Hume, that there is universal human nature, Vico espoused a genetic conception of human nature, regarding it as routinely evolving through the three general “ages” and their concomitant social formations and typical dispositions (*sette de’ tempi*), yet always uniquely particular due to different local conditions and traditions (Croce 1913; Berlin 1976; Haddock 1986; Kelley 1990).

Vico drew from his philological interpretations of classical mythology important lessons for his “new scientific” methodology:

Truth is sifted from falsehood in everything that has been preserved for us through long centuries by those vulgar traditions which, since they have been preserved for so long a time and by entire peoples, must have had a public ground of truth. The great fragments of antiquity, hitherto useless to science because they lay begrimed, broken, and scattered, shed great light when cleaned, pieced together, and restored. (NS 356–7)

Vico saw, long before modern scholars of mythology such as Malinowski, Eliade, and Levi-Strauss that men not only create myths but also consecrate them because these fictions preserve “the memories of the laws and institutions that bind them in their societies” (NS 203). As such, the ancient myths were not only the earliest but also the best philological sources for the discovery of the “principles of humanity.” The ancient

myths encode in their poetic images and tales the common-sensual “certainties” of *coscienza*, some of which – those that have proven absolutely essential to the preservation of human morality and society – will be duly recognized by Vico as the universal “principles” of the *scienza nuova*.

Now since this world of nations has been made by men, let us see in what institutions all men agree and always have agreed. For these institutions will be able to give us the universal and eternal principles (such as every science must have) on which all nations were founded and still preserve themselves. We observe that all nations, barbarous as well as civilized, though separately founded because remote from each other in time and space, keep these three human customs: all have some religion, all contract solemn marriages, all bury their dead. And in no nation, however savage and crude, are any human actions performed with more elaborate ceremonies and more sacred solemnity than the rites of religion, marriage, and burial . . . These must be the bounds of human reason. And let him who would transgress them beware lest he transgress all humanity. (NS 333, 360)

The validity of Vico’s concrete “principles” may be – and has been – contested on empirical grounds. Vico himself cites the counter-arguments of Arnauld, Bayle, and Spinoza, and modern readers could certainly produce even better empirical refutations. But such objections miss the essential point in Vico’s argument. The novel and important in Vico’s notion of the “principles of humanity” is the hermeneutical, not the empirical, assertion, namely the contention that there ought to be certain absolute norms of morality within which various forms of life can be exercised and be recognized as human.

In the brief “Conclusion of the Book,” Vico warns of what might happen to societies that have lost those “principles.” In his final reflections on the predicament of secular modernity, he describes how men in the cities and universities have become so thoroughly “ironical,” that they are like “beasts made more inhuman by the barbarism of reflection that the first men had been made by the barbarism of sense” (NS 1106). This second “Fall” regenerates the entire process of “coming into being” (*nascimento*) that all men and societies must undergo from bestiality to full humanity, if only, at the end, to fall and start all over again.

Historiographical Implications

With the establishment of the “principles of humanity,” Vico could claim that he had discovered the generative origins and evolution of all societies in history, and thereby fulfilled, as it were, the implicit ambition in the title of the book – to build up a “New Science of the Common Nature of the Nations.”

Our Science therefore comes to describe at the same time an ideal eternal history traversed in time by the history of every nation in its rise, development, maturity, decline, and fall. Indeed, we make bold to affirm that he who meditates this Science narrates to himself this ideal eternal history so far as he himself makes it for himself . . . For the first indubitable principle posited above is that this world of nations has certainly been made by men, and its guise must therefore be found within the modifications of our own human mind. And

history cannot be more certain than when he who creates the things also narrates them.
(NS 349)

Idealist philosophers of history like Croce or Collingwood use these enigmatic formulations as confirmation, or at least inspiration for their own basic assumption, that historiographic knowledge is primarily recognitions or reenactments of actions performed by historical agents, whose motivations or intentions could be regained by modern historians and whoever “meditates” and “narrates” the actions to himself, thus re-“making” history in his own mind. However, for more critical and sociological theorists of history such as Marx and other “Materialists,” Vico’s assertion of the primacy of “modifications” and the “world of nations,” or the deconstruction of persons like Homer and Solon to communal representations, makes him a discoverer of collective mentalities and identities in history. “Historicists” from Dilthey and Meinecke to Berlin, and fellow “hermeneuticists” like Gadamer, commonly find in Vico’s accentuation of the “creative” and “narrative” aspects in historiography a certain intimation of the modern sensibility to the different epistemic perceptions and poetic configurations by which each nation or civilization accounts to itself for its past. What all these, and many other, appropriations of Vico’s text and wider legacy imply is that even if Vico himself did not produce a coherent philosophy of history, let alone a competent historiography, his *New Science* remains an insightful source for reflections and innovations in modern historiography and social sciences with an interest in history.

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Further Reading

For general overviews of Vico's life and works see Adams (1935), Fisch (1963), and Burke (1985), as well as Verene (1991) on Vico's *Autobiography*. On Vico's theological preoccupations, see Caponigri (1953), Milbank (1992), and especially Lilla (1993). Vaughan (1972) and Bedani (1989) argue to the contrary that Vico was, tacitly, a radical heretic. The most substantial and influential discussions of Vico's philosophy are Croce (1913) and Berlin (1976). But see also Pompa (1975) and Verene (1981) for radically opposed interpretations of Vico's conception of "science," significance of "imagination," and much else. Vico's rhetorical vocation and philosophy of education are thoroughly elaborated by Mooney (1985), Grassi (1990), and Schaeffer (1990). For more technical studies of Vico's linguistic theory see Danesi (1993) and Trabant (2003). On Vico's new science of myth see Mali (1992). For a more general exposition of Vico's poetical philosophy of life and history see Mazzotta (1999). On Vico's political theory see Haddock (1986). The best discussion of Vico's historical scholarship is Rossi (1984). For astute observations on Vico's contribution to legal theory and history see Kelley (1990). Finally, the volumes edited by Tagliacozzo (1969, 1976, 1979, 1981, 1983), and the annual *New Vico Studies*, contain many excellent studies of Vico's thought and intellectual legacy.

Kant and Herder

SHARON ANDERSON-GOLD

Philosophy of history in the eighteenth century was chiefly concerned to discover principles by which history could be understood “as a whole.” By assuming that Nature forms a system, the great mathematicians and physicists of the seventeenth century had provided universal principles for physical nature. We can “know” the parts, they argued, because we can rightly assume that certain principles hold for nature “as a whole.” Human beings are also part of the natural world and social and moral philosophy became challenged to find a similar lawfulness or patterning in the human historical and cultural realm. The particular type of lawfulness thought appropriate to explanations of human behavior is the lawfulness that unfolds from the pursuit of a common or collective purpose attributed to human nature. Such lawfulness, it was assumed, would give to history a direction that not only orders the phenomena of the past, but also would make history intelligible through the projection of an overall purpose. Contemporary historians do not generally accept the claim that history has an overall purpose and this claim is part of what distinguishes a philosophy of history from historiography proper, or the empirical study of past events.

The year 1784 saw the publication of two comprehensive works on the philosophy of history, Immanuel Kant’s *Idea for a Universal History with a Cosmopolitan Purpose* and Johann von Herder’s *Reflections on the Philosophy of the History of Mankind*. Kant, the elder of the two, had been Herder’s teacher and in time, a lively rivalry would develop between them and the supporters of their competing perspectives. While both philosophers are concerned to understand history as a whole and believe that humanity is a species that has a collective purpose, their treatment of the structure and goal of history and the source of its value is radically different. For Kant, the final goal of history is the moral perfection of the species expressed in the ever increasing realization of freedom under law. Culture for Kant is a vehicle for a universal form of moral progress culminating in a federation of free republics in a condition of perpetual peace. Herder, on the other hand, viewed history as the unfolding of the unique qualities of humanity as conditioned by time and place. Each unfolding, each national “genius” or culture, has an intrinsic value of its own not derived from its temporal location. Universal history is the complex pattern created by the complete realization of human potentialities within which each culture makes its unique contribution.

I first provide an exposition of several of Kant's essays followed by a section summarizing contemporary interpretations and criticisms. I then provide a general exposition of Herder's *Reflections* that includes his objections to Kant's philosophy of history followed by a section on interpretations and criticisms of Herder's philosophy of history.

Kant's Philosophy of History

The development of human freedom is the theme of *Idea for a Universal History with a Cosmopolitan Purpose* (1784). Human activity, Kant tells us, occurs within the natural world and therefore must occur in conformity with natural laws. Yet, human actions are "free" and exhibit a type of spontaneity not characteristic of mechanically determined phenomena. For the causal observer the effect of free will on human action makes individual choices appear random and chaotic. Kant's solution to the problem of the lawfulness of human freedom is to adopt the standpoint of the human race as a whole. From this perspective, Kant claims, we will discover a regular movement in the aggregate. Here we are concerned not with the individual, but with the species and the development of its natural capacities. But herein we discern a difference between humanity and other natural species. Humankind is characterized by a far greater dependency of the individual on the species than is the case for animals generally. This is because the capacities associated with the exercise of reason require practice and cannot be fully developed within the life time of a given individual. The only means of rational development is the cultural transmission of skills from one generation to another. The unique impact of reason, which continuously expands the scope of the natural capacities of the species, implies that we must study humankind historically. Cultural development depends upon institutional mechanisms to discipline the competitive antagonisms that are built into the human condition as a consequence of this social interdependence. In particular, it is necessary to establish a form of civil law that will guarantee the greatest possible freedom consistent with the freedom of others so that this competition will proceed in a manner that fully develops these capacities. However, since all law requires a human administrator, and since humans naturally attempt to exempt themselves from its discipline, the task of creating and instituting a perfect civil order is an ideal that can only be approximated.

A further hurdle to be overcome in order to perfect civil society is the establishment of lawful external relations between civil societies or states. The same antagonisms that exist between individuals within civil society bring states into conflict. War drains civil society of the resources it requires to provide for the internal education and development of its citizens. Justice, then, must be pursued on two fronts simultaneously, both within civil societies and between states. Fortunately, Kant argues, the economic forces of development once underway will cause states to desire mediations to prevent war, which is economically disruptive. States will attempt various combinations of internal and external arrangements until they arrive at a condition that is self-sustaining. Kant refers to this self-sustaining condition as a cosmopolitan condition because it is only within a dynamic, interactive, lawful, and peaceful international order that all of

the natural capacities of the species can be fully developed. This, Kant concludes, is the goal of history and the moral destiny of the species.

Clearly, cultural development is an important part of Kant's philosophy of history. But what exactly is Kant's theory of culture and how is it related to humankind's moral destiny? In *Conjectures on the Beginning of Human History* (1786) Kant provides a general theory that clearly separates culture from nature by arguing that culture arises from the exercise of choice made possible by reason. Earlier in the same year Kant published a review of the first two installments of Herder's *Reflections*. In this review Kant criticized Herder for constructing a new form of naturalist metaphysics (knowledge claims that go beyond all possible experience). Herder's goal in these chapters of *Reflections* was to demonstrate the continuity between the history of the universe and the history of humankind. To ground this continuity, Herder postulates invisible forces operating within matter from which living things and reason as a natural power arise. Kant objects that in postulating such forces we are moving beyond the possibility of experience that for Kant is the boundary of all natural explanation. In particular, Kant takes issue with Herder's characterization of reason as emergent from and predetermined by mankind's physical organization. This naturalization of reason was antithetical to Kant's Critical System that depended upon a discontinuity and analytical separation of reason from nature. In *Conjectures* Kant distances himself from Herder by making clear that the history of freedom is distinct from the history of nature and arises from the free choices of human beings. Kant states that his account of "origins" will assume that humans have linguistic and conceptual skills that they have acquired by their own efforts. If such skills were already innate, Kant argues, they would be inherited whereas experience tells us that human beings in every generation must learn these skills anew. Reason for Kant introduces discontinuity with natural instinctive powers by creating artificial desires and modifying human responses accordingly. The successful development of the skills necessary to satisfy increasingly complex desires requires patience, practice, and instruction, making the individual radically dependent upon the group.

While *Conjectures* provides the same general overview of history as *Idea*, this essay lays particular stress on the origin of cultural conditions from human reason, freedom, and choices. This enables Kant to locate responsibility for cultural ills in individual human beings. Cultural choices entail differentiation along a number of dimensions such as labor, class, and economic base, leading to distinctly different cultural forms and ultimately to group conflict. Although for individuals cultural dependence creates burdens, for the species cultural development is from the worse to the better, since these ills can be overcome by progress toward more perfect forms of culture. The essay ends with a call to individuals to contribute toward this progress. The notion that individuals have a duty to contribute toward the progress of the species is implicit in many of Kant's writings but is spelled out with particular clarity in part three of *On The Common Saying*. In this essay Kant argues that we have an inborn duty, as a consequence of membership in a series of generations, to influence posterity in such a way that the cultural and moral progress of the species continues. In *Idea* Kant had maintained that the ideal of morality belongs to culture but must be refined and developed into clear practical principles. Culture then is a vehicle for moral development and contributions toward the moral refinement of culture are part of the general duty of every individual.

Conjectures provides us with a complex view of human development that places the internal perfection of culture and the adjudication of cultural differences that cause conflict at the constantly moving center of moral progress. Peace, Kant tells us, will require a perfect culture and a perfect culture will require the full development of mankind's capacities including the fullest possible differentiation of cultures. Here there is an overlap in the visions of Kant and Herder. Full cultural differentiation is seen by both as inherently valuable and as part of a cosmopolitan destiny. However, Kant believes that the adjudication of these differences and their ultimate flourishing in a cosmopolitan condition requires the development of institutional legal orders at both the civil and international levels. Herder, as I shall explain in a later section, was suspicious of legally coercive mechanisms as a vehicle for cultural advancement and viewed the state as a degenerate, rather than a progressive development. Kant's theory of culture in *Conjectures* thus leads back to the philosophy of history articulated in *Idea*, where the problem of a perfect civil society and the problem of a just international order are interconnected.

Kant's mature philosophy of history cannot be fully understood without reference to *Perpetual Peace* (1795), which lays out the complete legal institutional structure necessary to achieve the cosmopolitan condition that is the ultimate goal of history. Here Kant repeats some of the themes of *Idea*. Peace will require the institution of republican governments and a federation of republics dedicated to the preservation of the security and freedom of each state. What is new in *Perpetual Peace* is Kant's addition of a third level of law that he calls "cosmopolitan right." All states entering into the republican federation must accept the requirements of a cosmopolitan right that Kant terms hospitality. Hospitality forbids hostility toward anyone who merely attempts intercourse with their neighbors. Such intercourse can be refused, but it cannot be attended with violence nor can the traveler ever be unnecessarily harmed or endangered. Hospitality does not automatically lead to permanent residence or to privileged terms of commerce. These must all be negotiated. But Kant insists that the principle of hospitality is a form of right, a matter of law and not of etiquette. This third article guarantees that a republican federation will enjoy an open form of communication and not become an aggregate of "walled" communities. Cosmopolitan right is then the institutional precursor of the cosmopolitan condition and fills out an additional prerequisite of moral progress.

Criticisms of Kant's Philosophy of History

The social and cultural interdependence of human beings is a constant theme of Kant's philosophy of history. Human beings are culturally formed and socially dependent creatures who must build the institutional structures of republican governments and the international federation under cosmopolitan law. The essays on philosophy of history are consistent and mutually self-supporting in their identification of humanity's moral and historical goal. Nonetheless, Kant's philosophy of history has long been considered marginal to his critical system and has been criticized as incompatible with his ethical theory on two general grounds. The first is that moral development in history implies a moral inequality between generations violating the postulate of the

moral equality of all humanity. The second is that moral progress in history implies moral causality in the phenomenal (empirical) world violating the postulate that morality has a strictly noumenal, non-empirical, character.

The claim that moral progress in history is incompatible with Kant's ethical principles has a long lineage going back to Emil von Fackenheim's 1956 article "Kant's Concept of History." In that essay, Fackenheim argued that given Kant's definition of rationality, a rational (moral) man must be free and responsible without qualification. He concluded that historical development either qualifies moral freedom, or that morality to the extent that it is possible does not develop historically. From this line of reasoning, it follows that Kant's claims concerning the moral development of the species violate the moral equality of all humans because earlier generations would be either not free, and therefore not moral agents, or, lacking full moral development, morally inferior. Furthermore, history would produce unfairness because earlier generations would contribute toward a progress that they would not enjoy. While this implies a hedonistic goal that Kant would not endorse, it nonetheless appears to devalue these earlier generations by valuing their contributions only as a means to the final moral goal. This was Herder's view of the implications of Kant's *Idea* and is also implicit in the complaint of Hannah Arendt (1982) that Kant's conception of moral progress violates human dignity.

Other commentators, such as William Galston (1975), have focused on the apparent contradiction between Kant's definition of moral freedom as a non-empirical reality and his claim that morality is causal in shaping the historical order. According to Galston, Kant does not sufficiently identify the source of moral progress in the historical order. In addition, Galston maintains that Kant goes so far as to indicate that historical progress can be purely the result of natural causes or even the consequence of "evil" in the form of unsociable sociability. Naturalist explanations Galston concludes cannot, on Kant's own terms, carry the weight of the claim that there is moral progress in history. These objections arise from the apparently dogmatic (metaphysical) character of the claims made in *Idea* concerning nature's substantive (purposive) and causal role in the historical process. Allen Wood (1998) has since argued that Kant's later development of the subjective status of teleological judgment (judgments about nature's purposes) in the *Critique of Judgement* (1790) rectified the dogmatic character of Kant's naturalist assertions in *Idea*. From the perspective of the *Critique of Judgement*, statements about nature's purpose are a heuristic device that enables observers to interpret holistic patterns on analogy with human purposes. Nature's purposes whatever their explanatory value do not have the objective or causal status of physical laws and so do not violate human freedom. Along these lines, Paul Guyer (1995) has argued that Kant's references to the "cunning of nature" when describing the creation of social institutions must be interpreted as shorthand for the use by human agents of natural knowledge about the human condition in designing social institutions to support the achievement of moral goals. Under this interpretation it is humans in their exercise of freedom and not physical nature driving historical development. Yirmiahu Yovel (1980) has provided further support for the proposition that human agency is the principle factor driving historical development in Kant's philosophy of history. He argues that on Kant's own terms, with the post-enlightenment appearance of reason, history must be interpreted to be the work of human agents capable of and responsible for the

creation of institutional mechanisms designed to promote moral progress. Sharon Anderson-Gold (2001) has maintained that in *The Contest of the Faculties* (1798) Kant develops a theory of historical “signs” intended to represent agent produced moral causality in the historical order. Contrary to Galston, she claims that Kant does identify a specific source of moral progress in the disinterested public judgments that affirmed the republican principles of justice espoused by the French Revolution. By demonstrating moral sympathy for the (moral) principles of the revolution, the public according to Kant has exhibited its own “disposition.” Having exhibited a moral disposition toward the goals of human freedom and equality, Kant concludes that humanity can be the source of its own improvement and on that basis, moral progress can be predicted over the course of the species’ (indefinite) future history.

Early criticisms of Kant’s philosophy of history that focused on presumed incompatibilities with his ethics insufficiently recognized that Kant regards individuals as having genuine moral duties to contribute to the progress of humanity and that these contributions are both part of the history of freedom and of the individual’s own moral good. These duties, Anderson-Gold (2001) argues, tie the individual’s moral good to the social good of the species. Kant’s postulation of duties to contribute to historical progress are expressions of the duty to contribute to the highest good which he claimed to be the ultimate object or final end of the moral law. While the concept of the highest good remains controversial in Kant’s ethical system, Yovel (1995) has demonstrated that this concept necessarily and systematically brings Kant’s ethical ideals into the realm of human history. Kant’s moral philosophy then necessitates his philosophy of history through the conception of the highest good, rather than being incompatible with it.

Herder’s *Reflections* and His Objections to Kant’s Philosophy of History

Herder’s *Reflections on the Philosophy of the History of Mankind* (1784–1791) consists of 20 books published over a period of 7 years. The scope of this work that grounds universal history in cosmology is unusual even by eighteenth-century standards. Herder begins with an overview of man’s place within the solar system. He then narrates the natural history from which the shape of the earth, the atmosphere and the distribution of the lands and oceans of the earth has arisen. This “origin” story is intended to provide the background conditions from which variations in the histories of nations will later be drawn. The unique qualities of humanity, Herder tell us, follow from the unique physical organization of the species, in particular its upright posture. From this organization flow reason and ultimately even language and religion. Culture, for Herder, is continuous with nature, and nature has as her purpose the creation of the most variegated forms of life possible. Following these general cosmological and geophysical speculations, Herder provides detailed accounts of the manifold cultures that have occupied the earth leading up to modern European civilization. In this sweeping account of universal history, Herder gives new valuation and status to the culture of primitive peoples. Cultural historiography and not merely military or political historiography takes center stage.

Herder believed passionately in the individuality of each *Volk* (people). The representations of every people are uniquely shaped by their physical environment, history and traditions, which he referred to collectively as “climate.” The shepherd, Herder tells us, perceives nature with different eyes than those of the fisherman or the hunter. The unique circumstances of each people led Herder to maintain that measures of happiness and perfection were internal to each culture. Individuals must find their fulfillment within their cultural traditions. Reason is found then in a social consensus within a given tradition. Herder rejected the enlightenment notion of reason as a source of universal standards, for this implied the possibility of external criticism of culture from a higher or privileged position. In his theory of historical understanding Herder insisted on an interpretation and evaluation of cultures based upon internal standards of value. He even accepted that these standards were incommensurable but believed that they could be penetrated by a sympathetic identification (*Einführung*).

The historian then looks not to external standards but seeks to reenact events empathetically by “feeling” his or her way into the circumstances of others. This form of cultural relativity led Herder to reject the notion of history as a story of progress in which prior stages of cultural development were mere means to the development of later stages. In particular, Herder rejected Kant’s notion of the coercive political state as a necessary condition for historical progress. For Herder peoples constituted the natural basis of the state. Modern expansionist states had destroyed these natural arrangements and replaced them with artificial groupings. In Herder’s view, the modern state was responsible for the condition of oppression and war. For Herder the modern state represented a degenerate condition that tended to deform and destroy authentic forms of culture. Because he viewed authentic cultures as the proper basis of self-governance, Herder emphatically rejected Kant’s claim that man was an animal that needed a master, calling this an “evil” proposition.

Herder’s disagreement with Kant’s philosophy of history is largely driven by disagreement with his moral philosophy and its underlying dualisms. Despite broad agreement with Kant on the cultural and historical qualities of humanity, Herder makes clear that he cannot accept the overall evaluative framework that structures Kant’s philosophy of history. He strongly rejected universal moral values insisting on the intrinsic value of each cultural form. Yet, paradoxically, despite cultural variety, Herder also insisted on the unity of the species. Despite his rejection of an external linear progress, Herder did believe in a form of progressivism that flowed from his general conception of nature as an active vital force striving to produce durable forms of life. Despite their unique core qualities, Herder recognized that cultures influence one another both chronically and diachronically, thus interpenetrating and building upon one another.

Unlike Kant’s view of nature as a “step mother,” who cares little for humanity and uses antagonisms to force humans to produce their own good, Herder conceived nature as nurturing. He rejected the need for any universal institutional mechanism for eradicating conflict arguing for a more direct application of the natural principles of justice in the relations between states ending exploitation, domination, and oppression. According to Herder, evils oppose and destroy one another until only utility, or that which serves the general welfare of humanity survives. Reason Herder tells us may be disturbed, but it does not exist as an autonomous force external to particular traditions. Rather, embodied in traditions, reason is wholly within individuals and

peoples, transmitted and even “propagated” as peoples interact. Justice is the balance and equilibrium of active powers contending within determinate limits. Thus defined, reason and justice are natural propensities providing the overall order without which the “species would cease to be.” Humanity is then the particular combination of reason and equity achieved in the various conditions under which the species has existed and will continue to exist into the future.

For Herder, authentic cultures are not imperialistic, and he saw no contradiction despite his rejection of universal values in condemning Rome for destroying the ancient balance of nations, claiming that under Rome “a World bled to death.” Even with the example of an imperialist culture before his eyes Herder does not give way to pessimism but trusting to nature’s purpose he assumes that new nations will rise from the ashes and each will continue to seek their own maximum perfection. Thus, Herder claims, nature’s purpose is accomplished in each individual because each people are all that they can be in their time and place.

Herder rejects Kant’s postulation of a universal and autonomous moral realm. Still, he nonetheless accepts that humankind has a purposive inner direction displayed through cultural formations, which do in effect build upon one another and tend toward the development of what for Herder appears to be a moral goal. He calls this moral goal *Humanitat* which despite violent oscillations is best grasped as the production of ever more refined, mutually reinforcing and harmonious cultural forms. While rejecting any simple hierarchy of cultures, Herder’s universal historiography terminates in modern European civilization, which he regards as fortunately situated by virtue of its rivers and trade to develop its own peculiar genius of “industry” in the arts and sciences. What this new cultivation will mean for universal history is not yet clearly spelled out but Herder clearly invests great hope in the contribution that modern Europe is yet to make. In many respects *Reflections* is intended as a pedagogic tool for the education of modern Europeans.

Criticisms of Herder’s Philosophy of History

In addition to being influential in his own time, Herder is credited with numerous significant intellectual contributions to many modern disciplines. Because of his insistence on the significance of language in the life of a people, Herder is considered a contributor to the modern philosophy of language and comparative linguistics. Through his sympathetic appraisals of the mythology and poetry of primitive peoples, he strongly influenced the development of aesthetic theory as well as cultural anthropology.

Perhaps Herder’s greatest contributions are to the transformation of historiography. F. M. Barnard (2003) credits Herder (along with Vico) for being among the first to grasp that historiographic explanation is fundamentally different from natural causality. In rejecting mechanistic modes of causation as inappropriate for historiography, Herder is one of the earliest proponents of viewing historiographic explanation as concerned primarily with the “inner” qualities of events, grounded in the reasons, motivations and values of historical actors. Otherwise, Herder’s historiographic explanations are

multi-causal, referring to genetic, geographic, and cultural forces. Ultimately, Herder does not distinguish sharply between external causation and (internal) human agency in history, leaving the nature of historiographic explanation embedded in a naturalist (though not mechanistic) framework.

Herder's emphasis on the intrinsic value of all cultural forms and his insistence on a historiographic methodology that denies an external evaluative framework are among the sources of a profound change in philosophy of history that led to the rise of historicism. Friedrich Meinecke (1972) considered Herder one of the originators of this new historiographic perspective that would emphasize the individuality of historical events and jettison the idea of an unchanging human nature. Herder contributed to the rise of historicism by insisting on the individuality of cultural forms and their contribution to the deep structure of human existence. The philosopher and intellectual historian Isaiah Berlin (1976) finds Herder's most original contribution to be in his explorations of the meaning of "belonging" to a group. According to Berlin, in this respect Herder exceeded the understanding of his contemporaries and preceded modern social psychology. Herder is most radical in his insistence on the incommensurability of cultural values. Berlin detects an inconsistency between that fundamental commitment and Herder's attempt to see in human history a general advance (*Fortgang*) or overall purpose of development that he called *Humanität*. F. M. Barnard (2003) also noted this inconsistency stating that Herder leaves unexplained how distinct cultural forms in any sense "belong to" an historical process that should exhibit a universal pattern. According to Barnard, Herder cannot explain how the progressive changes that over time result in the human quality he deems *Humanität* are related to the morally distinct cultures that are the specific products of the historical process.

Criticisms of Herder's philosophy of history flow directly from the qualities that are most unique and admired in his work, such as his rejection of dichotomies, his use of analogies, and his poetic use of metaphors. What appear on the surface as contradictions may reflect this mixed mode of argumentation. Perhaps the best way to understand Herder's apparently unsystematic approach to the manifold materials that profoundly interested him is to see his style as driven by a strategic use of rhetoric, intended to shake up and shake loose what he perceived to be misguided a priori conceptualizations in Kant's philosophy and in much of the philosophy of the Enlightenment. It is not that Herder rejected the mathematical sciences or the possibility of enduring human values. He simply saw too much richness in history and the human condition to force either into analytical categories. If Herder's methodology was not precise, it may reflect his attempt to capture the emergent quality of the life sciences that he sought to position as the foundation of the human sciences.

While Herder's naturalism remains problematic, it has, nonetheless, also been a source of independent methodological orientation for the human sciences. Having set the human sciences apart from the mathematical and physical sciences, Herder has provided inspiration to subsequent generations of culturally oriented researchers, who have gone on to construct independent methodologies from which much original and insightful analysis of the human condition has arisen.

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Hegel

TOM ROCKMORE

This chapter describes some main features of Hegel's complex view of history. It is well said that in Hegel history and philosophy meet, since he is a historian of philosophy and a philosopher of history, who also changed history (Hartman 1953: ix). It is also well said that at Stalingrad Hegel's right-wing and left-wing students met on the field of battle (Holborn 1943: 62). As the popularity of Fukuyama's (1992) thesis about the end of history proves, Hegel's philosophy of history (through Kojève in this case) remains influential.

This account of Hegel's theories of history begins with Hegel's general interest in historical phenomena, especially the French Revolution. Next, it will turn to his view and conception of the philosophy of history. It will end in an account of Hegel's view of the relation of knowledge and history.

Hegel's Interest in History and the French Revolution

Hegel's interest in history is well known and undisputed. His interest in history is influenced by such factors as his classical training in German secondary school and later in the Protestant seminary, his concern with current events, the French Revolution, and the development of the problem of knowledge in German idealism.

The impact of the French Revolution can scarcely be overestimated. The French Revolution destroyed the *ancien régime*, leading eventually to the republican form of government that still persists in France. It gave increased prominence to the idea of the modern citizen as endowed with inalienable rights, the so-called rights of man. It further enfranchised sections of the population that had been disenfranchised up until that point, including Jews. It finally led to a separation between church and state in a predominantly Catholic country.

The French Revolution produced deep and lasting changes in philosophy. Hegel, who remained interested in politics throughout his career, developed a very subtle analysis of the French Revolution in the *Phenomenology*. He was in favor of its ideals but deeply opposed to revolutionary excesses (Harris 1997: vol. 2, 384–96; Ritter 1982: 3). In the introduction to his *Lectures on the Philosophy of History*, Hegel remarks that the Revolution is rooted in thought, more precisely in the desire without precedent to

create a society based on a rational concept of human being. (Hegel 1984a) This explains the fact that in the *Phenomenology*, he situates the passage concerning the Revolution in the midst of other passages on the Enlightenment, a historical moment when the faith in reason was manifest, and another, more detailed passage regarding the ultra-rationalist Kantian view of morality.

According to Hegel, the doctrine of the Enlightenment is opposed to faith. There is an opposition between faith, which naturally concerns the beyond, the infinite, the other world, and the universal, on the one hand, and the intellect, which busies itself with the here and now, the finite, this world, and self-certainty, on the other. Religion arises from a level that is pre-conceptual, and that has not yet attained the level of philosophy. Hegel rejects neither religion nor faith. He detects a permanent tension in the Enlightenment between faith and intellection, which is resolved in utility (Hegel 1977: 349–54). This concept constitutes the link between faith, which is lacking all efficacy, but which possesses truth, and pure intellection, which possesses self-consciousness, but is lacking in truth.

Utility forms the criterion through which Hegel analyzes the French Revolution. His discussion contains three parts, or moments, concerning absolute freedom, the terror, and the awakening of free subjectivity. Absolute freedom represents self-awareness or self-consciousness without any real opposition, or again a pure intellection without resistance. Pure intellection destroys and, hence, surpasses the bounds following from the structure of society to accomplish what Hegel calls its law, its aim. Yet just when there is no longer any opposition within its world to intellect, a new opposition arises in the distinction between individual consciousness and universal consciousness. For the individual, who takes himself for universal consciousness, imposes his law under the form of terror, for which the terror of the French Revolution is the best example.

Hegel, who is perhaps thinking of Napoleon, offers an analysis valid for dictators of every stripe. Universal freedom, without any limit, is only negative, producing, as he remarks, no more than “the *fury* of destruction” (1997: 359). The French Revolution expresses pure intellection that knows no limits, and that consists in self-expression in actions wholly insensible to anyone other than oneself, the revolutionary actor on the historical stage. It lacks the necessary connection, or true mediation, between the universal principle motivating the action, its maxim (the principle governing action in Kantian terminology) and the action following from it. Hegel contends that the result of intellection without any restrictions can only be death. Even on the political plane, reason cannot be realized in this way. For a government that acts in this manner represents only the faction that has won, not the general will, and, hence, not the will of the people in general (in Rousseau’s language).

Hegel is not hostile to the concept of revolution; and he is not hostile to the political event of the French Revolution that, according to him, transmits the idea of freedom. For Hegel, all modernity consists in coming closer to freedom on the concrete, practical, level. Yet he believes that when we evaluate the Revolution according to its own aims, it is clear that it was unable to realize them, for the revolutionary desires were transformed into their opposites.

The Hegelian analysis of “Absolute Freedom and Terror” is triply important. To begin with, it transmits the nuanced opinion of Hegel who accepts the fact of the French Revolution as an instance of relative progress but unequivocally condemns the

excesses to which it gave rise. Although he criticizes the immoderate dimension of the French Revolution, Hegel explicitly recognizes the new possibilities it creates in a world that is henceforth in transition toward another epoch, the post revolutionary period. He underlines these possibilities when, in a famous passage, he says that our time is a period of gestation and of transition to a new period (1977: 6).

This opinion clearly exemplifies the sustained interest that Hegel accords to concrete facts and to history. Hegel, though reputed to be very abstract, shut up in the proverbial academic ivory tower, constantly breaks with this image through the practical, concrete dimension of his thought, which follows from its historical character.

The Hegelian, unlike the Kantian, philosophy of history is specific, yet central to his philosophic system. Kant is concerned with the idea of history; but he fails to integrate it into his theory. Kant's view of theory finally remains on the margins of his theory of knowledge, eccentric to his epistemological theory that is resolutely a historical. Hegel is arguably the first major thinker to integrate the historical and systematic aspects within a single philosophical vision. With the exceptions of Karl Marx (1818–1883) and perhaps Max Weber (1864–1920), Hegel remains perhaps the first and the last to integrate history into his system.

Hegel and the Philosophy of History

Philosophers who write about history often do not know even the main historians or have more than a minimal acquaintance with historical writings. Hegel, who was an exception, had a working grasp of all the main historians up to his time and a deep knowledge of historical events.

In holding that history is rational, hence cognizable, Hegel disagrees with his predecessors, who believed that history is important or unimportant, but not cognizable. Aristotle thought that history was unimportant, whereas Augustine, who invented the familiar eschatological conception of history, thought it is important but cannot be known. For Aristotle, poetry, which is concerned with universals, is more important than history, in which things happen only once (Aristotle 1985: 1451b, 5–7). If that were the case, knowledge of history would be impossible. According to Augustine, we indeed know the final aim of history, which is to return to God, but, since we cannot know God's mind, we cannot know history itself (Augustine 1950). On the contrary, I take Hegel to be saying that under the proper circumstances, we can and indeed do know history.

Hegel's *Lectures on the Philosophy of World History* is very controversial. Here as elsewhere in his writings, different interpretations, which find support in his texts, are possible. A further problem is that we do not possess a final version of his view published by him, but only different versions of his lecture notes, hence different versions of his view of history to work with.

Hegel begins by distinguishing three main ways of writing history or historiography: original historiography, reflective historiography, and philosophical historiography. By original historiography, Hegel has in mind the writings of historians who were contemporaries of the events they described and which they undertook to describe in the form of representative thought. It follows, if the writer must be a contemporary of

what he describes, that the scope cannot be large and that he shares in and does not reflect upon the content.

Reflective historiography, which goes beyond the present in spirit, deals with the past. It divides naturally into four subtypes. Universal historiography puts the accent on synthesis in surveying the entire history of a people, a country, or the world. This kind of historiography reflects the author, and the spirit of the historical moment he or she belongs to, which may differ from that of the materials, while achieving a certain generality. In the pragmatic form of reflective historiography, the author unifies the materials through a general idea in making the event(s) present. In this context, Hegel makes the famous remark that history teaches us that, as he says, "nations and governments have never learned anything from history" (Hegel 1984a: 21), hence they cannot act upon such lessons. The deeper reason is not ignorance about the past, but the difference between each historical configuration, which has its own difficulties and solutions. Critical or scientific historiography, which was introduced and then widely employed during Hegel's lifetime by German historians, was a historiography of historiography, which evaluated the authenticity and credibility of historiographic narratives. Specialized historiography, the fourth kind, is fragmentary, particular, and abstract. It consists of selecting a single general perspective or point of view as the focus, such as the history of art, law, or religion. It differs only in its particular focus from the third kind of historiography, or philosophical historiography of the world, which also adopts a general perspective.

Philosophical historiography, Hegel writes, is "nothing more than the application of thought to history" (1977: 25). The aim of historiography is to comprehend the past. He specifies his view, adding, "The only thought which philosophy brings with it is the simple idea of reason – the idea that reason governs the world, and that world history is therefore a rational process" (1977: 27).

The interpretation of this claim is not easy. The main religious and secular alternatives, which are incompatible, have support in the texts. It is unclear what Hegel is claiming, whether it is better to regard him as favoring a religious, a secular, or a providential-faith based approach to history? Or can the alternative approaches be compatible? Evidence for the religious reading, which is often adopted, derives from the many things Hegel says in this passage about providence and knowing God. Evidence for the secular reading, which is more rarely adopted, follows from Hegel's approach to reason here. Hegel notes that Anaxagoras' idea that reason rules the world is illustrated in nature, and that, with the exception of Epicurus, after it was taken over by Socrates, it was accepted throughout philosophy (1977: 35).

The religious and the secular approaches to history are anticipated in earlier thinkers. Voltaire, who coined the term "philosophy of history," is often credited with being the first to work out a non-religious approach to history. He was writing against Bossuet, who provides an updated version of Augustine's eschatological conception of history. Instead of treating history as the familiar march of God through the world, Voltaire treats it as a collection of facts to be interpreted in a secular manner from the perspective of human reason. In place of the revolutionary return of man to God located outside (human) history, he believes in a moderate form of progress within the historical space, hence the perfectibility of human beings within as opposed to beyond history.

It is usual to read Hegel from a right-wing, or religious perspective. After Hegel's death in 1831, the Hegelian school quickly fragmented into right and left, or young Hegelian wings. The right-wing Hegelians insisted on a theological reading of Hegel, which the left wing accepted as correct but criticized. A religious reading of Hegel's position, including his view of history, is common. According to Löwith, Hegel is the last philosopher whose view of history depends on Christianity (Löwith 1949: 57).

At least since Kojève, who pioneered an anthropological reading of Hegel's *Phenomenology*, many observers deny Hegel's view of history is theological in stressing an anthropological approach (Bouton 2004). I believe this is correct, with the qualification that whatever Hegel is claiming is not theological in the ordinary sense of the term, though it may indeed be religious in an "unusual sense" since, as Hegel sometimes indicates, he defends religion against various theological systems.

Whether this is correct, it is clear that Hegel's view that history is rational can be read in a secular fashion. Hegel's central thesis, that is, that there is reason in history, leads to an interesting theory of knowledge of history. I take Hegel to be saying that under the proper circumstances, we can and indeed do know history. Hegel's approach to history depends on his reading of Aristotle. According to Aristotle, human action is teleological, or goal-directed, directed toward the human good, hence rational. In contending that human history is composed of the actions of men and women in the social context, Hegel extends an Aristotelian approach to action to history. If human action is rational, then history, as the record of human actions is also rational, hence can also be known.

Hegel adds nuances to this view of history as rational through related discussions of great men in history, or world-historical individuals, and the cunning of reason. His aim is to make the point that, though history is indeed rational, it is far from transparent. His explanation starts from two extremes, which are brought together in the course of human action: reason and passion. According to Hegel, the general ends of reason are manifested in and through the concrete actions of individuals. Such aims are realized through "passion," according to Hegel "not quite the right word" to refer to "any human activity which is governed by particular interests, special aims, or, if you will, by selfish intentions" (Hegel 1953: 72). Hegel distinguishes between the intentions of ordinary individuals, concerned with their own ends, and the so-called world-historical individuals, such as Alexander, Caesar, or Napoleon. The latter are concerned with realizing deeper ends, whose moment is at hand so to speak, and for which they sacrifice themselves. Such individuals follow their own passions, but the import of their actions is universal. According to Hegel, it is the cunning of reason that a particular person realizes a goal different from his intention (1977: 75, 89).

Hegel and the History of Philosophy

Hegel, who lectured on the history of philosophy nine times in his career – he was giving the tenth set of lectures in 1831 when he fell ill and suddenly died – is not the first to be interested in the history of philosophy. Aristotle, for instance, typically studies the views of his predecessors before formulating his own. Numerous modern historians of philosophy treat the philosophical past as a series of opinions of different

thinkers. Yet Hegel is the first modern thinker, perhaps even the first important thinker, to link philosophy to the history of philosophy. In that specific sense, he can be said to invent the academic sub-field of philosophy, the history of philosophy, as we now know it.

Hegel stresses the importance of the history of philosophy for philosophy. He typically does not distinguish between philosophy and its history. He approaches the history of philosophy as in effect a giant Socratic dialogue, in which different perspectives vie with each other in an ongoing search for the truth. There is no single royal road to the truth, that is, a preferred philosophical tendency, and different theories recommend themselves as relatively better with respect to alternatives. Later philosophers have before them the previous discussion. Philosophy builds on all that is positive in the preceding history of philosophy in attempting to make progress toward the “solution” of outstanding problems.

Philosophical theories, like individuals, belong to their own historical moment. Hegel's position grows out of his effort to come to grips with philosophy at the beginning of the nineteenth century. In his initial philosophical text, entitled the *Difference between Fichte's and Schelling's System of Philosophy*, he criticizes the main philosophical theories of his time, which are identified with the names of Kant, Fichte, and Schelling. He already saw Kant, Fichte and Schelling as embarked on a single project, demonstrating the speculative identity of subject and object, knower and known. In relation to this approach, he created the idea of the German idealist tradition, with four main members, culminating in his own position. He later deepens and develops this approach but never alters it substantially.

Hegel later extended his claim concerning a single central philosophical task to the entire history of philosophy. The entire philosophical tradition is concerned with the problem of knowledge, which hence links together the many disparate theories as so many attempts to arrive at a solution. Since beginning in ancient Greece, philosophy has always asserted but never demonstrated the idealist claim for the unity of thought and being. This claim already arises in Parmenides' claim for the identity of thought and being. It is later reasserted by Anselm in the ontological proof of the existence of God, and restated in different forms by a great many important thinkers, including Kant.

Hegel possessed a truly encyclopedic grasp of the history of philosophy. His detailed study, entitled *Lectures on the History of Philosophy*, is replete with interesting readings of the main philosophical positions, as well as many lesser thinkers, which also illuminate his own theories. Hegel's detailed criticisms cast light on the genuine accomplishments of prior thinkers as well as his own position. Plato, a world-historical individual, who had enormous influence on later thought, is already concerned, through study of the intellectual world lying beyond sensation, with the unity of reality and thought as depicted in the movement of science. Hegel is particularly interested in Plato's *Parmenides*, as an outstanding example of dialectical thought. Plato focuses on generality or universality. Aristotle is an unusual genius, whose like has never again occurred. Hegel's remarks on activity (*energeia*) are particularly interesting. He already focuses on the concept, which overcomes all dualisms. Hegel accords particular attention to modern philosophy, the period to which he belongs. Modern philosophy begins with Descartes. This period is marked by the principle of thought, also called the

Protestant principle, which arises in Christianity. Philosophy asserts that thought is the principle of the world.

Hegel closes with three comments. To begin with, the history of philosophy is not a mere collection of different thoughts. On the contrary, in all times there is only a single philosophy, whose differences express different, but necessary aspects of a single principle. Further, the development of philosophy is not contingent, but rather the necessary development of the phases of this science. Finally, most recent philosophy in any given historical period is the development and truth of its spirit.

Hegel's Historical Approach to Knowledge

Hegel's least known contribution is arguably his view of the historicity of knowledge claims. Reasons for the neglect of Hegel's view of knowledge include the suspicion that, as Kant thinks, epistemology begins and ends with his position as well as widespread hostility toward philosophical idealism. A general hostility to idealism belongs to the self-understanding of both Marxism and Anglo-American analytic philosophy. Marxism typically rejects idealism for materialism. Since G. E. Moore (1958 [1903]), analytic thinkers have consistently refused idealism even while recently returning to Hegel.

Hegel consistently maintains that philosophy is itself intrinsically historical. Like the other post-Kantian German idealists, Hegel participates in the ongoing effort to develop critical philosophy beyond Kant. His argument for the historical character of knowledge claims derives from a rethinking of critical philosophy.

Kant's interest in knowledge is a central theme in his position. There are two different, incompatible approaches to knowledge in critical philosophy, which we can call representationalism and constructivism. Representationalism consists in some form of the view that a claim to knowledge must correctly represent a mind-independent external object as it is. In taking up a representationalist approach, Kant further develops, but later abandons, a main modern epistemological strategy. After a period of initial commitment, which is clearly indicated in the important letter to Marcus Herz (February 1772),¹ Kant came to reject this strategy on the grounds that, if the cognitive object were really independent, then it could not be known. One way to put this point is to note that, according to Kant, we cannot reliably claim to know things in themselves. His second epistemological approach, which is constructivist, consists in claiming that we can only reliably claim to know objects we in some sense construct. This approach is the central insight in Kant's so-called Copernican revolution in philosophy. Kant never uses this term to refer to the critical philosophy, but in his own time it was already utilized to describe his position by several of his contemporaries such as Reinhold and Schelling.

Constructivism comes from mathematics, especially Euclidean geometry. Euclidean geometry constructs plane figures with a compass and ruler. Constructivism takes different forms, all of which are based on the insight that we can only know what we in some sense construct. It is independently introduced into modern philosophy by Hobbes, by Vico, who follows Hobbes, and by Kant. Kant argued for an a priori form of constructivism. There are various difficulties in Kant's conception of constructivism,

including his inability to explain the activity through which the subject constructs its cognitive object. In attempting to improve critical philosophy according to its spirit, later German idealists reformulate it in a posteriori form Kant would have rejected. In a fuller account, it would be necessary to describe the phases in the transformation of the critical philosophy leading up to Hegel's position. In simplest terms, one can point to Fichte's interest in practice, and both Fichte's and Schelling's concern with history. These and other insights come together in Hegel's conception of the knowing process, hence knowledge, as intrinsically historical.

The conviction that knowledge is historical arises out of a further development of the constructivist insight that the subject must construct what it knows. The historical form of this insight includes at least the following subclaims: first, since knowledge is human knowledge, the knowing subject is one or more finite human beings; second, the activity through which human beings construct what they know takes the form of different human practices; third, human constructive practices always occur within a social context; and, fourth, since social contexts change over time, human practices leading to the construction of cognitive objects occur in a social, hence historical space.

Hegel illustrates his version of the general constructivist approach to knowledge in a variety of ways. One of the most helpful examples is his general account of the process of knowledge in the introduction to the *Phenomenology of Spirit*. Hegel, who argues against immediate claims for empirical knowledge, is often wrongly accused of ignoring experience. On the contrary, through his deep concern with concrete phenomena of all kinds, he takes experience very seriously. Hegel is not concerned with the relation of phenomena to the mind-independent external world outside experience, which is unknowable. He is, rather, concerned with knowing what occurs in conscious experience. He depicts knowledge as a process of trial and error consisting in the comparison, within consciousness, of concepts, or views about experience, and objects of experience, or the contents of consciousness. Concepts are theories, which arise out of experience, which they are intended to comprehend. Knowledge consists in the grasping whatever is given in conscious experience through concepts, that is theories. In any comparison between concepts and cognitive objects, there are only two possibilities. Either the concept and the object correspond, that is, are identical, or there is a difference between what one expects and what one finds. In the former case, the theory correctly grasps the object, and the process of knowledge, whose terminus ad quem is truth, comes to an end. In the latter case, when the theory turns out to be different from what one finds in practice, one needs to reformulate the theory.

Hegel innovates in his view of the relation of theories to their cognitive objects. Since early Greek philosophy, a frequent theme is the conviction that to know requires us to grasp the mind-independent world as it is, in a word to know reality. Hegel abandons the pretense of grasping the world as it is as for the world as we experience it. According to Hegel, what we experience depends on the conceptual framework we utilize, hence changes as the framework changes. Thus someone who knows some chemistry might "perceive" H₂O instead of water. Accordingly, it is incorrect to hold that we formulate different theories to grasp the mind-independent world as it is; rather, we formulate successive theories in view of grasping the world as given in experience, which changes as the theories about it change. Knowledge is not the

result of a direct grasp of what is. It is, rather, the result of a historical process in which successive theories are formulated to grasp the phenomenal contents of consciousness.

Note

- 1 See Immanuel Kant, *Prolegomena to Any Future Metaphysics* and the *Letter to Marcus Herz, February 1772*, trans., intro., and notes James W. Ellington (Indianapolis: Hackett, 2001), pp. 117–22.

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Neo-Kantianism

CHARLES BAMBACH

Nineteenth-century German philosophy – especially the philosophy of history – was defined in and through its multi-layered, sometimes ambivalent relationship to the thought of Hegel. For Hegel, schooled in the post-1789 revolutionary vision of historical progress, history becomes a metaphysical pageant of human freedom that needs to be understood as a science (*Wissenschaft*) of spirit (*Geist*). Human activity, Hegel contends, presents itself as a ceaseless struggle with and against nature, a struggle whose irrational paradoxes and aporia could ultimately be sublated in an overarching system of dialectical development that would lead to the final triumph of reason in world history. This metaphysical vision of historical progress would founder, however, in the laboratory of historical experience in the nineteenth century. In the work of historians and philosophers trained in both the new historical and scientific methods, there emerged a critique of Hegel's speculative philosophy of history. The empirical approach of the German Historical School (von Savigny, Ranke, Droysen) sought to eliminate speculative metaphysics through rigorous research methods. Yet it was in the epistemology of Neo-Kantians, such as Eduard Zeller, Otto Liebman, and Kuno Fischer, that there emerged a deep-rooted critique of Hegelian speculation carried out in a new programmatic enterprise. In this overarching turn to a thoroughgoing epistemic investigation of the actual practice of research in the diverse fields of university education, Neo-Kantianism attempted to bridge the chasm between the bankruptcy of the old idealism and the excesses of the new materialism. Before exploring the specific effects of this Neo-Kantian turn to epistemology on the philosophy of historiography in late nineteenth-century Germany, however, we need to understand both the context and question-frame of German philosophy as it comes to redefine itself after the collapse of Hegel's system.

The Setting and Development of Neo-Kantian Thought

German Neo-Kantianism develops in and out of the historical situation of the German university in the nineteenth century. Unlike the great independent thinkers of the nineteenth century (such as Kierkegaard, Marx, and Nietzsche) whose philosophical work, though growing out of the German university, can ultimately be seen as a fundamental

rejection of its basic style and practice, Neo-Kantianism develops as a reaction to the situation of disciplinary research carried out at the university. Going back to the historical foundations of the University of Berlin organized in 1809 by Wilhelm von Humboldt, all fields of research had been taxonomically divided according to the reigning model of scientific inquiry. Hence, in addition to the traditional fields of law, medicine, mathematics, and natural science, Humboldt's University defined the study of history, art, music, philology, theology, and classics all as "sciences" or *Wissenschaften*. With the development of a more self-conscious inquiry into methodological procedures of humanistic study (evident in such texts as J. G. Droysen's *Historik* or August Boeckh's *Enzyklopädie und Methodologie der philologischen Wissenschaften*), contemporary scholars began to understand that the normative idea of *Wissenschaft* derived from the natural sciences might be inapplicable to the practice of philological and historical research. With the rise in the prestige and accomplishments of historical-hermeneutic scholarship, especially philosophy, in the nineteenth-century German university, a new model of *Wissenschaft* emerged.

At the same time, empirical scientists challenged the legitimacy of philosophic inquiry. As Hans-Georg Gadamer put it, by the mid-nineteenth century, "philosophy as a whole had gone bankrupt and the breakdown of the Hegelian domination of the world by Spirit [*Geist*] was only a consequence of the bankruptcy of philosophy in general" (Gadamer 1981: 24). What emerged out of this situation was a fundamental "perplexity over the proper task of philosophy . . . because the sciences have occupied all fields of reality. Thus nothing was left for philosophy except to become the science of the sciences" (Heidegger 1988: 10). In response, philosophy defined itself once again, in the tradition of Descartes and Kant, as *scientia scientiarum*, *die Wissenschaft der Wissenschaft*, raising questions about the underlying relationship of philosophy to science (as a methodological model) and to the sciences (as specific forms of research). Within this context Eduard Zeller wrote his programmatic essay, "Über Bedeutung und Aufgabe der Erkenntnistheorie" (1862), that turns to epistemology as a way of thinking through the proper relation between science and philosophy. Zeller's approach will press for a fundamental reassessment of scientific philosophy as a "return to Kant" that provides the way for a new theory of knowledge. The Freiburg philosopher Wilhelm Windelband (1848–1915) took up this epistemological reworking of Kant's project in the 1880s. Yet, despite the "back to Kant" rhetoric, Neo-Kantianism altered the original work of Kant (in the contributions of Hermann Lotze, Kuno Fischer, and Otto Liebman). We need to understand Neo-Kantianism less as a "return to Kant" than as a generational approach to philosophy that seeks to employ Kant's language and conceptual emphasis on a priori, transcendental structures of thinking. Windelband claimed that Kant's decision to privilege logic and epistemology over the empirical world of research led to a genuinely "critical" method of philosophical thinking.

Windelband maintained that Kant formulated in rigorously critical fashion the fundamental problem of philosophy: how, from the merely arbitrary and conditioned experience of individual perception, do we arrive at necessary, unconditioned, and universal truth? During the 1880s and 1890s both Windelband and the Marburg Neo-Kantian Hermann Cohen stressed the primacy of Kant's epistemological project, which sought to establish critical limits to human knowledge while securing its logical validity. Both men rejected the contingencies of the historical and psychological

subject in favor of the timelessly valid, transcendental subject of logic. Cohen proposed a mathematical model of logic as the ground for a theory of the sciences. Windelband argued that in 1781 Kant had embraced the “mathematical method” that dominated continental philosophy since Descartes. Yet with the unprecedented flowering of new sciences in the nineteenth century such as philology, anthropology, ethology, sociology, and historiography, a new model of epistemological rigor was called for. Applying Kant’s critical method to the new sciences of history and culture, Windelband sought to develop a logic of both the natural and historiographic sciences that would restructure the old boundaries, grounds, and principles of scientific classification according to the new realities of scientific research. Such a program, Windelband claimed, would extend beyond the *Critique of Pure Reason* with its epistemological focus on mathematics and natural science to include problems of ethics, aesthetics, religion, historiography, and anthropology that Kant addressed in the *Critique of Practical Reason*, *Critique of Judgment*, and his later Königsberg lectures. Following the logic of this later work, Windelband asserted that the task of the modern philosopher lies in transforming and modernizing Kant’s original project. As he put it in *Präludien*: “to understand Kant is to go beyond him” (Windelband 1924: iv).

Windelband’s Division of the Sciences: Nomothetic and Idiographic

In the pivotal “The Relation of the Natural Sciences to Science in General” (1862) Hermann Helmholtz divided the sciences into two distinct groups – *Naturwissenschaften* and *Geisteswissenschaften* – based on two different forms of induction, logical and artistic-instinctive. Philosophers such as Wilhelm Wundt and Wilhelm Dilthey argued for a strict demarcation of the sciences in line with Helmholtz’s division of *Geist* and *Natur*. In the midst of this debate, Windelband delivered his Strassburg Rectorial Address on “History and Natural Science” (1894) that significantly altered the basic terms of this problem. Windelband challenged Helmholtz’s traditional division of the sciences, arguing that it depended more on custom and tradition than on rigorous conceptual scrutiny. “Nature and mind [Geist] is a substantive dichotomy.” (Windelband 1980: 173–4) This classification is based on the object being investigated. Yet, what ultimately separated the *Naturwissenschaften* from the *Geisteswissenschaften* is not their “substantive differences” but the aim of their research. Each realm of knowledge required its own logical form and could never be subsumed under the positivist claim for a meta-logical unity in the sciences:

In their quest for knowledge of reality, the empirical sciences either seek the general in the form of a law of nature or the particular in the form of the historically defined structure. On the one hand, they are concerned with the form which invariably remains constant. On the other hand, they are concerned with the unique, imminently defined content of the real event. The former disciplines are nomological sciences. The latter disciplines are sciences of process or sciences of the event. The nomological sciences are concerned with what is invariably the case. The sciences of process are concerned with what once was the case. If I may be permitted to introduce some new technical terms,

scientific thought is *nomothetic* in the former case and *idiographic* in the latter case. (Windelband 1980: 175)

Windelband did not envision his classification of the sciences into generalizing-nomothetic laws and singular-idiographic structures to be prescriptive. The categories “nomothetic” and “idiographic” were methodological distinctions, not absolute ones. What determined their specific character was “the ultimate aims of scientific research” (Windelband 1980: 180). All of our judgments about the world could be divided logically into two realms of *being* and *validity*: “the world of reality” (*Wirklichkeit*) and “the world of value” (*Wert*). History, according to this reading, is not what is “real,” but what is “valued.” Hence, there are no intrinsically “historical” facts or events, no historical “thing in itself.” Rather, what renders something historical is “its relation to some high standard of value in life” (Windelband 1921: 205). In this way the values that the researcher brings to the study of the past lend significance to a specific event that subsequently gets related to a higher value. What determines historiography then is not ontology, but axiology.

The same object (*Gegenstand*) can be viewed from a nomothetic or an idiographic perspective; what determines the approach is not the ontological status of the object, but the value-perspective of the researcher. And yet Windelband wished to make it clear that value could not properly be grasped in terms of the psychological or historical subject. Psychologism and historicism posed the gravest dangers to philosophy, Windelband claimed, because both sought to reduce the Kantian principles of reason to mere temporal and psychological conditions independent of any transcendental source. In keeping with this emphasis on transcendental validity, Windelband rejected both Dilthey’s hermeneutic appeal to the historicity of understanding and Wundt’s social-cultural form of *Völkerpsychologie* as inadequate for grounding the ahistorical-transcultural principles of formal logic that a genuine Kantianism required.

By attempting to provide a Neo-Kantian solution to the scientific character of historiographic research, Windelband powerfully shaped the way turn-of-the-century philosophers thought about the philosophy of historiography. Yet for all his painstaking efforts at working out the taxonomy of scientific methods, Windelband never really produced a new logic of historiography worthy of its Kantian heritage. His programmatic division of the old *Natur-* and *Geisteswissenschaften* into the new nomothetic and idiographic sciences provided only the skeletal outlines of a fully developed theory. In one sense his genuine contribution to the debate over the sciences was to identify the formal-logical limits of research and to sharply define the negative contributions of historicism, psychologism, and *Weltanschauungsphilosophie* as inadequate to the task. Despite its shortcomings, however, the Strassburg lecture provided the basic question-frame for the Baden Neo-Kantian critique of historiographic science, a critique taken up with more epistemological rigor by Windelband’s younger colleague at Freiburg, Heinrich Rickert (1863–1936).

Heinrich Rickert’s Theory of Historical Knowledge

Rickert took up the challenge laid down by Windelband of developing a systematic epistemology of historiography in his magnum opus, *The Limits of Concept Formation in Natural*

Science. Originally published in 1896 (just two years after Windelband's rectorial address) *The Limits of Concept Formation* went through five different editions (1896–1929) as Rickert expanded his analysis, responded to critics, and refashioned his arguments. Rickert acknowledged that Windelband's basic claim about the nomothetic or generalizing method of natural science and the idiographic or individualizing method of historiography was fundamentally sound and useful. But he also observed that this distinction was too broadly drawn and inexact to serve as the grounding principle of a rigorous Neo-Kantian epistemology of historiography. His response was to present a new schema of *Natur-* and *Kultur-wissenschaften*. If Kant had defined nature as "the existence of things so far as they are determined according to universal laws" (Kant 1950: 42), Rickert now sought to define culture as the existence of things so far as they are determined by values. On this reading, the ultimate difference between the natural and historiographic sciences could not properly be framed as a problem of taxonomic classification (as Windelband had done) but as one of logical "concept formation."

Concept formation, as Rickert understood it, denoted the process of structuring, ordering, and making rational the sensory data provided to consciousness in the world of reality. In this sense "empirical reality proves to be an *immeasurable manifold* which seems to become greater and greater the more deeply we delve into it and begin to analyze it and study its particular parts. For even the 'smallest' part contains more than any mortal man has the power to describe" (Rickert 1962: 32). How then, Rickert asks, can we come to any genuinely scientific knowledge of the world if, in our attempts to discover universal laws, we only come to grasp an "infinitesimally small" part of reality? How, in a world where "everything is different from everything else" and heterogeneity confronts us everywhere, do we come to grasp scientific-universal truth? Rickert claims in good Kantian fashion that we can never know the thing in itself – hence, any attempt to set up a correspondence between knowledge claims and reality is doomed to failure. Scientific knowledge then cannot reproduce reality, but, rather, selects out of the infinite stream of possibilities some aspect of that reality that accords with values that the researcher deems worthy of pursuing. These values are, however, not coextensive with the reality that they organize and imbue with meaning. Values do not "exist" in any concrete sense, he argued; they neither possess material substance nor occupy space, but are based on formal principles. Their ground is logical or axiological, not ontological. Beings "are," values are "valid" (*Seiendes "ist," Werte "gelten"*). Human beings make sense of the immeasurably complex heterogeneity of the world by reducing its complexity via concepts which, in turn, are formed by values. Concepts simplify reality; they reduce to manageable proportions the mass of phenomena that the mind encounters, transforming the "real" into the "valuable" via a concept formation that distinguishes the essential from the inessential.

In Rickert's scheme the process of concept formation depends ultimately on the goal that one seeks in selecting essential elements from the stream of reality. This goal, in turn, shapes the objects that one selects. In every interpretation of reality, he claimed, there obtains a formal difference: either one focuses on the general characteristics that one particle of reality has in common with another – or – one focuses on the differences between particles. In *The Limits of Concept Formation*, in *Science and History*, and in his seminal article, "Zwei Wege der Erkenntnistheorie," Rickert repeatedly stressed

that only two paths of knowledge are open to us. We can either transform the heterogeneous continuum of reality into a “homogeneous continuum” or we can make it a “heterogeneous discretum” (Rickert 1962: 34). The first approach defines the logical aims of the natural sciences, the second, the axiological procedures of historiography. Science can either be generalizing, nomothetic, and law-seeking or it can be individualizing, idiographic, and particular. The task of the philosopher is to explain the problem of concept formation as it helps to shape the principles of selection in the natural and historiographic sciences.

Rickert insists that the differences in method must be understood in terms of values and not merely as a taxonomy of disciplines. The natural scientist has an interest in the individual phenomenon only as an example of a universal law. But the historian seizes upon this individuality because of its relation to a cultural value. As he explained it in *Die Probleme der Geschichtsphilosophie*:

The individual concrete meaning of [an object] . . . stands in the closest relation to universal concepts of value such that no historically meaningful object, to which we attach individuality, can attain historical meaning and significance without reference to a universal value. The concrete meaning is first “constituted” historically through the universal value. The concrete meaning that is found in the real objects, as well as the historiographic principle of selection, lies not in the sphere of real *being* but in that of *value*, and it is from here that the connection between the individual value-related method and the meaningful material of historiography must be understood. (Rickert 1924: 70, trans. mine)

Yet, Rickert realized that by grounding his logic of historiography in the problem of values he was opening himself to charges of historical relativism. How could science, which strove for objectivity, be grounded in value, which reflected subjective aims and ideals? How could one reconcile the subjective realm of historical experience with the objectivity of historiography? Determined to secure the objectivity of historiography by means of his conceptual logic, Rickert focused on two problems. First, he claimed, we need to logically distinguish the valuation (*Wertung*) of facts from the scientific task of relating such facts to values (*Wertbeziehung*). Valuations are unquestionably subjective; relating facts to values, however, need not be – “on the contrary, practical valuation and theoretical reference to values are two logically *distinct* acts” (Rickert 1962: 89). A historian can identify a specific action e.g., the execution of Louis XVI as essential to the history of the French Revolution without acknowledging that such an act either promoted or impeded the attainment of a specific cultural value (political freedom). “Valuations must always involve *praise* or *blame*. To *refer* to values is to do neither” (Rickert 1962: 90). Secondly, he claimed, we can guarantee the objectivity of historiography if our theoretical value-reference is undergirded by referring it to transcendental values that obtain independently of any subjective evaluation. “The fact that *cultural values are universal* in this sense is what keeps concept formation in the historical sciences from being altogether *arbitrary* and thus constitutes the primary basis of its ‘objectivity.’ What is historically essential must be *important* not only for this or that particular historian, but for *all*” (Rickert 1962: 97).

Within Rickert’s logic of concept formation, universal-transhistorical values served as transcendental norms (Kant’s *Sollen*) by which to measure cultural reality (*Sein*) in

terms of its scientific relevance. As valid (*geltend*) rather than real (*seiend*) values do not exist as such, but find expression in cultural spheres such as religion, art, the state, community, economic practices, ethical precepts, and the like. In this distinction between cultural values – whose ground is transcendental – and cultural objects – whose expression is historical – Rickert was convinced he had found the means to overcome the split that reigned in historiographic methodology between subjective, value-laden culture and objective, value-free cultural science.

In his late work, which attempted to derive a full-blown philosophy of historiography from his logical analysis of historiographic-cultural concept formation, Rickert sought to demonstrate how ahistorical values could serve as the logical ground for a transcendental subject who stood opposed to the empirical-psychological subject of the historical world. In pursuing an ahistorical solution to problems of historical and cultural relativism, Rickert turned to the Kantian notion of an ethical imperative that transcended history. As he always maintained, “history was in no position to solve philosophical problems for itself” (Rickert 1929: 697, trans. mine). In practice, Rickert maintained, the historical subject attempts to concretize ethical demands by bringing to fruition certain projects and aims that we call “culture.” In all of these endeavors we seek ultimately to reconcile the gap between the “is” and the “ought,” between meaningless being and meaning-laden history. Such a reconciliation can never be completed, however, because of the unbridgeable gulf between the reality of history and the ideality of value. And yet the meaning of historical life is grounded in the attempt to overcome this gulf and achieve a universal value – a task that is “insoluble,” according to Rickert. Consequently, the philosophy of history never succeeds in proffering final truth. The values that it attempts to systematize serve as exemplars of a “Kantian *Idee* whose realization becomes the goal for all cultures that must, nonetheless, labor under the knowledge that theirs is a never-ending task” (Rickert 1924: 119, trans. mine). The historian’s goal of relating facts to universal values, then, becomes a Kantian expression of freely pursuing an ethical imperative that seeks to bridge the chasm between what is and what ought to be. In recognizing the primacy of freedom as an ethical imperative within science itself – and in freeing science from the realm of strict necessity – Rickert believed he had transformed Windelband’s methodological inquiries about historiography into genuine questions of value philosophy.

Cassirer’s Logic of the Cultural Sciences

Philosophers such as Dilthey and Heidegger would come to criticize Rickert’s “solution” to the problems of historical relativism. They were convinced that in his attempt to measure “the values of the past against what *should* be” (Rickert 1924: 131, trans. mine), Rickert had appealed to *suprahistorical* values that denied the historicity of human experience. They insisted that Rickert overlooked that history was essentially in the *past*, not something “there” at hand in the present, waiting to be “known.” The reality of temporal distance did not constitute a logical problem in Rickert’s theory of concept formation. On the contrary, he hardly seemed to notice that the historical individual was not a given “fact of knowledge” modeled on the Marburg Neo-Kantian notion of

mathematical-logical time, but was itself part of the hermeneutic context of historicity and facticity that shaped its concern for knowledge in the first place. In the years that followed, Ernst Cassirer (1874–1945) took up the challenge of Rickert's position and offered a penetrating critique of the Baden Neo-Kantian stance.

Unlike Rickert, who devoted most of his career to writing on questions of logic, methodology, and the division of the sciences, Cassirer believed that to do exemplary work in epistemology and logic one needed to have studied deeply the history of philosophy. Trained in Marburg under the direction of Hermann Cohen and Paul Natorp, Cassirer wrote his dissertation on Descartes' critique of mathematical and natural-scientific knowledge (1899). Beginning with his first major book, *Das Erkenntnisproblem in der Philosophie und Wissenschaft der neueren Zeit* (1906), he examined the logic of science by tracing its methods historically. He would later distinguish himself by writing formidable works on Leibniz, Renaissance cosmology, the Enlightenment, and Kant while attempting to ground his own systematic work in a dialogue with the history of philosophy, something that he believed was lacking in both Baden and Marburg Neo-Kantianism. At home in the Marburg school's philosophy of mathematics and natural science, Cassirer sought to expand the boundaries of Marburg Neo-Kantianism to include the whole realm of cultural expression that comes to us in language, myth, and symbol.

In *The Logic of the Cultural Sciences* (1942) Cassirer attempts to overcome the nomothetic-idiographic split to develop a unified, systematic philosophy of the sciences. Like Windelband and Rickert, Cassirer begins with Kant's decision to delimit the realm of metaphysics by focusing on the relationship between concepts and percepts in our experience of reality. Moreover, Cassirer shares with the Baden school its rejection of positivism and the metaphysical excesses of speculative philosophy. Yet the basic thrust of Cassirer's work is to correct the false dualism that he perceives in Windelband and Rickert's logic that he traces back to the growing fragmentation of philosophical and scientific knowledge since the death of Hegel. He attempts to do this by grounding the different structures of inquiry pursued by the sciences of nature and culture in the structure of perception itself. In this way, he claims, we can establish a phenomenology of perception to overcome the bifurcated spheres of nature and culture. Perception is, for Cassirer, always the perception of that which stands over against us as a "Gegenstand" or "object." Yet within this perception, he insists, there is a "twofold direction": a direction toward the "it" and a direction toward the "thou" (*Du*). What emerges from this analysis is a split between an "object-pole" and an "I-pole," a world of objects in space-time that we perceive as "things" and a world of persons who we understand as beings "like ourselves." In each case the "I" confronts the alterity of its object, but with a marked difference: "The 'it' is quite simply an other, an *aliud*; the 'thou' is an *alter ego*. It is unmistakable that perception takes on another meaning for us and as it were a distinctive coloring and tone depending on whether we move in one or the other direction" (Cassirer 2000: 39).

This difference in perception between the "it" world of things and the "I" world of persons becomes for Cassirer the phenomenological basis for an understanding of the natural and cultural sciences. The natural scientist investigates a world of things that stand apart as mere objects, whereas the cultural scientist explores a world of expressions that reflect the subjectivity and intersubjectivity of an "I" engaged by

culture, language, and community. In this difference between thing-perception (*Dingwahrnehmen*) and expression-perception (*Ausdruckwahrnehmen*) Cassirer finds the basis for his new logic of the “cultural sciences” – a term that he chooses as a homage to the Warburg Library for the Science of Culture set up by the art historian of the Renaissance, Aby Warburg. Cassirer’s intuitive attraction to the realms of art, religion, myth, language, and history thus emerge here in sharp contrast both to the mathematical-natural scientific approach of Marburg Neo-Kantianism and the logical-epistemological direction of the Baden school. Indeed, Cassirer’s criticism of Windelband and Rickert’s was that it “remained at the level of general conceptual distinctions” without ever having involved themselves in the “richness of concrete research” in the areas about which they wrote (Cassirer 2000: 37). Within their work, history was perceived merely as a “thing” in space and time, an “it” guided by properties, structures, and relations that were borrowed from the realms of logic and mathematics. But a cultural object is not simply “there” for us as a thing, waiting to be analyzed like a physical object in nature. Culture comes to us as a “symbolic form” that has meaning only as an *expression* within an intersubjective world.

Given Cassirer’s insight into the phenomenology of perception, we can see why he rejects the whole Rickertian appeal to logic and method as a way of resolving the crisis of the sciences. “Historical and scientific thought are distinguishable not by their logical form,” he claims, “but by their objectives and subject matter” (Cassirer 1944: 176). All attempts to found the logic of both historiography and science are “doomed to failure” because they focus on formal rules and not on the fundamental experience of nature and history that comes to us in the different ways we perceive things and expressions. Cassirer claims this division in the sciences can be traced back historically to the development of the mechanical worldview in Descartes, Harvey, and their seventeenth-century contemporaries that separated form and cause in scientific thinking. As a result of this split, “causal thinking” came to be identified with mathematics and natural science, while “form thinking” became associated with the study of culture. With the introduction of Einstein’s physics, Gestalt psychology, and organic biology at the turn of the nineteenth century, however, Cassirer identified a positive breakthrough that he believed had “eliminated a separating barrier that until now has stood between . . . the sciences of nature and the sciences of culture” (Cassirer 2000: 96). With this recognition of the unity of each sphere “form analyses and causal analyses now appear as orientations that do not conflict with each other but rather complement each other, and that must interrelate in all knowledge.”

Finally, Cassirer turns to the realm of symbolic forms as a way of overcoming what Georg Simmel considered “the tragedy of culture” – namely, the alienation of the human spirit from itself and from its own cultural products. Comparing Weismannian inheritance theory (which points to “the non-transmissibility of acquired characteristics” in biological development) to the realm of “cultural phenomena,” Cassirer comes to understand the role of religion, language, art, and myth as modes of human freedom that lead not to self-alienation but to self-knowledge. “What individuals feel, will, and think does not remain enclosed within themselves; it is objectified in their work. And these works of language, poetry, plastic art, and religion become the ‘monuments,’ the signs of remembrance and recollection of humanity. They are ‘more lasting than ore’ . . . the creative will, and creative power from which [they] emerged, continues to live

and be effective within [them] and to lead to ever new creations” (Cassirer 2000: 126–7). In this understanding of culture as an ever-renewing process of preservation and transformation that shows itself clearly in the development of a national language and an artistic style, Cassirer believed he had found a way of affirming the variability and instability of cultural form as a way of understanding cultural science. Culture is not a mere thing, but an expression of human temporality and of the human being’s striving for futurity that can never be encapsulated in the temporally static forms of Rickertian logic.

By turning his attention to the temporally dynamic expressions of language, art, and myth, and by freeing the cultural sciences from their entrapment in the atemporal realm of taxonomy and methodology, Cassirer succeeded in transforming the old Neo-Kantian theory of the sciences into a phenomenological account of the human experience of nature and history under different forms of perception. In so doing, Cassirer came to grant the *phenomena* of nature and history a philosophical significance, something that Rickert and Windelband in their categorizing of the *methods* of science and historiography had overlooked.

Within the phenomenological tradition, Heidegger’s attack on the basic question-frame of Rickert’s logic appears decisive. On Heidegger’s reading, Neo-Kantian philosophy of history covers over the fundamental experience of history in terms of its historicity, obscuring “the authentic meaning” of the past as a temporal phenomenon and not a scientific fact. Rickert’s “sole theme is the question of the logical structure of scientific representation. This is carried to such an extreme that in Rickert’s philosophy of science the sciences under study are no longer even recognizable” (Heidegger 1985: 17). This “distortion and trivialization” of historical being marked for Heidegger the end of the Neo-Kantian tradition. Hermeneutics and phenomenology then reframed the very question of history.

Outside of Germany, Neo-Kantian philosophy of history was developed by émigrés such as S. H. Bergman and his student Nathan Rotenstreich in Jerusalem. Austrian economists like Hayek appealed to Neo-Kantian philosophy in their attempt to construct a human science of economics. Within the positivist circle, philosophers like Popper and Hempel were familiar with the works of Windelband and Rickert and situated their own philosophy in relation to them, most notably in borrowing the “covering law” from Windelband. This school had, however, a greater influence over the formation of questions for the philosophies of history and historiography than on their answers.

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Marx

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Marx's philosophy of history stands on three pillars: a critique of Hegel, a critique of orthodox or so-called bourgeois political economy, and an alternative theory of modern industrial society or capitalism (Rockmore 2002). Since Marx did not describe his philosophy of history in detail, it has to be reconstructed from his texts. It has been the practice over many years to read Marx through Marxism, often in substituting texts by Engels or other Marxists for Marx's own writings. On the contrary, Marx will be read here in terms of his own texts and against the historical background in which his position emerged.

On the Marxist Reading of Marx's Philosophy of History

Like Hegel, Marx is a deeply historical thinker, who understands ideas, concepts and theories in the historical context. It seems, to be consistent, that we should read Marx in the historical context out of which he emerged and to which he reacted. This "contextualist" approach is denied by Marxism, which is "officially" "anti-contextualist." Since it was invented by Engels, a long line of Marxists have always argued for what Althusser has called an epistemological break (*coupure épistémologique*) between Marx and his social and intellectual surroundings (Althusser 1970): everything happens as if Marx arises in, but is independent of, hence unaffected by, his historical moment.

Engels' Marxist approach to Marx reflects a widespread belief that Marx, like Hegel before him, was the last philosopher. Generations of Marxists have repeated variations on this theme. It can be exemplified by Lukács, who is arguably the ablest of the Marxist philosophers. Lukács reformulates Engels' view. Hegel's mythological view of the historical subject is inadequate to grasp real social problems, which cannot be solved on the basis of classical German philosophy. Marx shows us the way to solve the problems of philosophy through his discovery of the proletariat as the real historical subject. Lukács illustrates his faith in Marx in claiming that at this point in history there is no problem that cannot be solved on the basis of the analysis of commodities (Lukacs 1971).

Marx, who was trained as a philosopher is arguably best understood as a philosopher working within the wider framework of German idealism. Marxists, who deny this

point, tend to distinguish rigorously between science and philosophy in considering Marx's position as a form of science, Marxism as itself science, and by implication science as the sole source of knowledge. Engels, who was interested in the science of his day, is close to what today would be called positivism. Like the positivists of the Vienna Circle, he thought that knowledge of all kinds can and must be formulated as scientific laws. He recommends substituting science, which yields truth, for philosophy, which yields only mythological and finally mistaken views. In this way, he anticipates scientism, for instance in W. Sellars' distinction between folk views and science (Sellars 1991: 1–41). Engels, who compares Marx as a scientific figure to Darwin, claims that Marx discovered the laws of history (Engels 1978: 681). This approach later influenced generations of Marxists, who have continued to read Marx through Engels' eyes.

Engels describes Marx's view as historical materialism. In the 1892 introduction to *Socialism: Utopian and Scientific*, Engels writes that historical materialism "designate[s] that view of the course of history which seeks the ultimate cause and the great moving power of all important historic events in the economic development of society, in the changes in the modes of production and exchange, in the consequent division of society into distinct classes, and in the struggle of these classes against one another" (Engels 1984: 386–7). Gerald Cohen reformulates a form of this view in sophisticated analytic fashion. Like Engels, Cohen mainly disregards the Hegelian context in which Marx's position arose in depicting it as a social science. Cohen influentially depicts Marx's philosophy of history as a form of functionalist explanation, based on a model in wide use in biology, in which the concept of function plays a causal role (Cohen 2001).

This approach to Marx's theory of history is influential, but controversial. It has been countered by Jon Elster, who calls functional explanation into question in raising questions about appealing to "purposes" in history that are not the purposes of anyone" (Elster 1985). Another problem is that it turns away from Marx's philosophical anthropology in substituting science for philosophy. Like Fichte, Marx understands human beings as basically active and human society as the result of human activity. Like Vico, he suggests that we can only know human history since we in some sense construct it. A final difficulty is that this approach disregards the way in which, like other philosophers, Marx reacts to, evaluates, criticizes, reformulates, and carries forward themes in the then contemporary debate, including the discussion of history.

Marx's Philosophy of History

Marx's position encompasses philosophy, political economy, as well as such allied disciplines as history and political science. Marx's overall position is based on his theory of finite human beings. This theory depends on a series of basic distinctions encompassing the difference between work or labor (*Arbeit*) and what I will be calling free human activity, a form of activity that can only occur beyond constraints imposed by the economic process; capitalism or communism; history and human history; reproductive needs and species needs; alienation and fulfillment.

Activity, not work, is Marx's basic interpretive category. His overall position can be sketched in terms of his "Fichtean" theory of human activity. Like Fichte and Aristotle, Marx approaches human being through human activity. As early as the *Paris*

Manuscripts, he asks, “For what is life but activity?” and he answers his rhetorical question: “My *own existence* is a social activity” (Marx 1964: 158).

Marx distinguishes between two types of activity linked to the developmental stages of society. Work or labor (*Arbeit*) is the form of activity manifested by a person within the productive process characteristic of modern industrial capitalism. It requires the use of preexisting material, which is acted upon and transformed as part of the process. Work is productive, as opposed to creative, quasi-physical as opposed to mental, and basically active as opposed to passive.

Work is epoch-specific to capitalism, which is only the latest in a series of phases of the development of the means of production and, as consequence, of social relations. If and when capitalism is replaced by communism, work in the traditional sense will cease to exist. Marx occasionally stresses this point, as in the following passage from the *German Ideology*. “In all revolutions up till now the mode of activity always remained . . . whilst the communist revolution [which] is directed against the preceding *mode* of activity, does away with *labor*” (Marx and Engels 1970: 94). It follows that in communism there will be a different form of activity. But, unfortunately, just as Marx is rarely explicit in reference to communism, he only occasionally refers to this second form of activity, in a sense the goal of human history as Marx understands it, though its real possibility is everywhere presupposed as the perspective from which to criticize capitalism.

Marx’s Fichtean approach to human beings as basically active is the basis of his theory of finite human beings who construct or produce objects, themselves, the surrounding social world, and finally human history. In working out his analysis of modern industrial society, Marx applies a specifically Hegelian analysis of objectification through productive activity in an economic setting. In the *Philosophy of Right*, Hegel describes the self-objectification of workers in and through the economic process, in the course of a wider account of the basic concepts of political economy.

In his analysis of capitalism, Marx builds on Hegel’s own account of basic economic categories. Hegel provides the basis for Marx’s theory of alienation in his description of an economic process in which products and individuals are alienated (Hegel 2005: 97–8). Marx brings together Hegel’s analyses of objectification through work of all kinds and modern industrial capitalism in a general model of modern industrial society.

According to Marx, finite human beings have needs, which can be divided into two main types. Reproductive needs typically include food, clothing, shelter, and other necessities of life. There are also human needs, which must be satisfied in order to develop as an individual human being. According to Marx, in capitalism, which is typified by private ownership of the means of production, most human beings do no more than meet their reproductive needs, but cannot develop in ways necessary to meet their human needs.

Human beings meet reproductive needs through work, which is accordingly the master interpretive category for the capitalistic stage of human development. Humans produce a series of “products” including at least commodities, social relations, society, themselves, and human history. A commodity is a product destined for sale in the market place. Human beings, who work within the economic process, produce relations between individuals and, more generally, the entire social context. “By social relations we understand the cooperation of several individuals, no matter under what

conditions, in what manner and to what end" (Marx and Engels 1970: 50). Social relations include at a minimum, relations between people and things and among people. Society as a whole is merely the ensemble of different social relations of which it is constituted. A given person has a distinct role within the social world as identified by a given economic function, or form of work, such as a bricklayer, head of the household, university professor, or capitalist. Human history is a further "product" composed of the actions of human beings within the social context over time.

One of the most interesting aspects of Marx's position is his emphasis on historical change, unlike philosophies of history that do not explain historical. Recent examples include Heidegger's view that human history emerged out of an earlier turn away from being, Foucault's positivistic account of history as composed of disconnected epistemes, and Lyotard's idea that postmodernism differs from modernism in the rejection of overarching explanations. Marx's philosophy of history is based on economic development. He assumes a fundamental distinction between superstructure and base. The base refers to the economic organization of the means of production, and the superstructure refers to all other, non-economic, "cultural," phenomena, including philosophy, law, and so on. The well-known relation of superstructure and base has two interpretations: One is as a unilateral relation, in which the base is said to determine the superstructure. The other is as an interaction in which each determines the other. In both cases, Marx holds that changes in the economic base lead eventually to changes in its superstructure. Marx also assumed that economic development leads to social conflicts and crises, which transform society and, as a direct result, human history.

By the term "conflict" Marx, following Hegel, endeavors to think social contradictions. According to Marx, social conflicts arise when the development of productive forces comes into conflict with the existing relations of production, leading to social revolution. By "revolution," Marx means adaptive social change, which stops short of deeper social transformation, for instance in the transition from capitalism to communism. Marx is realistic in suggesting that a social order, which is based on a particular constellation of social forces, never disappears before all its productive forces have developed. Additionally, periodic crises may result from underconsumption. Marx sometimes "romantically" suggests that capitalism will finally founder on such a crisis. One cannot rule it out, but there seems no particular reason to support such an inference.

Marx's theory of capitalism, the driving force of the modern world, is in effect a theory of the modern world. It would be a mistake either simply to accept or to reject Marx's theory of capitalism without criticism. There are numerous questionable points in Marx's theory of capitalism. One, which has attracted much attention, is his theory of surplus value and his general theory of value (Böhm-Bawerk 1949). Another is his theory of economic crisis.

Marx on History and Freedom

Marx's view of the historical realization of freedom does not differ from Hegel's in the belief that real human freedom depends on economic factors, nor even in dismay at

the results of the industrial revolution (Lukács 1975). It is different in the explanatory priority accorded to economic factors over all others, as well as in a more critical attitude toward political economy.

Hegel, who partly measures freedom in terms of recognition, accords more weight than Marx to legal recognition. Marx subordinates legality and everything else to economics. Like Hegel, Marx is concerned with progress, hence with social freedom. Marx, more than Hegel, interprets freedom as a function of the development of the economy. He sees the need to liberate individuals from the economic yoke of modern industrial society, in a word to bring about a post-economic realm of freedom, in order to develop their capacities in ways unconnected with economics. His attitude toward capitalism is always balanced, never unbalanced, mixing praise with blame, criticism with acknowledgment of positive features. The persistently negative aspect of his discussion of modern industrial society derives from his clear view, which remains up to date, of how capitalism functions in practice. The private ownership of the means of production leads to individuals being forced into undesirable roles which neither they nor anyone would freely assume. The pressure to accumulate capital, which is built into capitalism, often carries with it a horrendous social cost. Yet he also points out that suffering in modern industrial society is balanced through the development of the means of production. And he holds out the prospect that the supposed intrinsic instability of capitalism will lead to a post-capitalist society in which the means of production will no longer be privately owned, hence eliminating or at least reducing economic pressure to accumulate capital.

Marx's binary model is most clearly visible in such early writings as the *Paris Manuscripts* and *The German Ideology*. It is less visible in later writings, where the emphasis increasingly falls on understanding the functioning of modern industrial society.

Marx's model presupposes a distinction in kind between forms of society, correlated with two broad historical periods. Prehistory is the series of social formations ending in capitalism, the stage in which economic imperatives subordinate everything else, including any realistic perspective of meeting human needs surpassing mere existence needs – which are often euphemistically referred to as food, clothing, and shelter – to the accumulation of capital. Marx further envisages a post-capitalist society, or human history, which, in early writings, he calls communism, a term with no more than a purely linguistic relation, or the word in common, to forms of “official” Marxism. In this future stage, human beings will supposedly retake control of the economic sector of society, which from that time on will be subordinated to the needs of all people everywhere. In the Marxian scheme, capitalism is justified, despite its social cost, as the only way to bring about the development of the means of production required for the transition from capitalism to communism in which, as the slogan goes, all contribute according to their capacities and receive according to their needs.

In Marx's position, human freedom requires the prior development of the means of production. It also requires human beings to acquire control of the economic process as a result of which they will be freed, or at least made relatively freer, from the economic yoke of modern industrial society. Marx is not making the utopian claim that when the capitalists lose power, we can forget about economics. Basic human needs will still need to be met. But when they have been met, time will be available for other, non-basic needs, such as poetry and so on.

What kind of freedom can we expect in a post-capitalist society? Marx cautiously entertains various ideas in his writings. In the early *Paris Manuscripts*, he argues for the “reconciliation” of human beings with nature, which is described, romantically enough, as man’s other body. Marx’s suggestion is that when people are freed from the constraints of earning a living the various senses will develop in new and different ways, all of which will lead to bringing out the individuality of each of us. His central idea seems to be that capacities, which are not necessarily economically useful, and which, on grounds of division of labor are not developed in capitalism, could be developed in a post-capitalist society. Slightly later in the *German Ideology*, in an equally romantic passage, he takes up the idea of the many-sided individual (implicit in the *Paris Manuscripts*) in a future society in which there would be no division of labor. In such a society, Marx imagines that each person could do whatever one wanted at different times without regard to competence or training. Still another suggestion from his later period emerges late in the third volume of *Capital*, which appeared after his death, in an important passage worth evoking here.

Those inclined to doubt, as well as those who assert, continuity in Marx’s position need only glance at chapter 48, “The Trinity Formula.” Like the *Paris Manuscripts*, which many years earlier began with consideration of the wages of labor, the profit of capital and the rent of land, this chapter starts with the three categories of capital, profit, and land, or ground-rent, from which it takes its name. According to Marx, freedom, which only begins where forced labor ceases, consists in establishing control over the economic process in conditions favorable to human beings. Although real needs must still, and will always need to be, met through the economic process, that is, within the realm of necessity, beyond it lies what Marx now calls the realm of freedom. In suggesting that its prerequisite lies in shortening the working day, he implies that as the goal of history real freedom lies in free time. Freedom no longer lies in a break with a previous stage of society through revolution. It rather lies in a basic improvement in the conditions of life, or in reform. Marxism has traditionally been hostile to mere reform (cf. Bernstein 1961). Yet Marx seems to hold out hope that modern industrial society and real human freedom are in principle compatible if and only if human beings can reestablish control over the economic process, which is the real master in capitalist society. In denying that human ends can be identified with the accumulation of capital, Marx suggests that people must be freed for development beyond the economic process.

Marx’s Historical Approach to Cognition

Marx’s view of history leads to a historical approach to cognition. Marx defended a form of the identity theory of knowledge, espoused by idealist thinkers from Kant through Hegel. The single most useful passage in Marx’s writings for his approach to knowledge occurs in the Introduction to the *Grundrisse*, a connected series of texts outlining an enormous project only partially realized in *Capital*.

Marx formulates his approach to knowledge by commenting on Hegel’s complex approach to knowledge. Marx claims, very much like Hegel, that we cannot grasp economic (or indeed other) phenomena directly. We can, on the contrary, only grasp

them indirectly through the economic categories utilized in modern political economy, in a word against the background of a conceptual framework, which changes as the social world changes. On this basis, Marx rejects abstract identities, as well as ordinary empiricism, for which he substitutes categories, which mediate the relation to experience. Categories, which depend on, and serve to grasp the historical context, are not fixed, but change in history. Complex categories refer to simpler categories, and the simplest categories, which appear as relations, imply a concrete substratum.

Though it seems best to begin from population, since this is the real and concrete prerequisite of political economy, this is, according to Marx, a mistake. Population, which is an abstraction, depends on classes, which in turn depend on exchange, division of labor, and so on. To begin with population is to begin with a general idea of the whole, or a merely imaginary concrete, which is analyzable into simpler ideas. The correct approach is illustrated by recent political economists, starting with Smith, who began from simple conceptions such as labor, demand, exchange value, and so on, before concluding with state, international exchange, and world market. The category of labor, which was only discovered by modern political economy, implies the existence of highly developed forms of concrete labor, independent of the individual, hence in need of explanation. According to Marx, Smith made a great advance in defining labor in general as the source of wealth. This simple abstraction, which is used by modern political economy as its starting point, is truly realized in the most modern society.

This categorial approach leads in two directions, toward the critique of Hegel and toward a theory of knowledge very similar to idealist constructivism. Marx typically objects to what he regards as Hegel's tendency to substitute abstract analysis for the concrete social world. This objection can be compared to the difference between Hegel's *Logic*, which discusses the movement of categories within thought, and the *Phenomenology* in which he considers different, alternative conceptual frameworks. In the latter, he argues that there can be no immediate knowledge, or sense certainty. What we comprehend (now using the words "abstract" and "concrete" in ways opposite to normal usage, in which thought is abstract and direct experience of the world is concrete) is "concrete" since it is mediated through the conceptual process. In rejecting the view he identifies with Hegel, Marx in fact only rejects his view of Hegel's *Logic* in favor of his view of Hegel's *Phenomenology* (Lukács 1978).

According to Marx, the approach leading from the abstract to the concrete, or the same approach described by Hegel in the *Phenomenology*, is the way thought, in fact, unfolds. But since the conceptual process does not generate the concrete object, Hegel supposedly conflates what happens within a person's mind, mere thought, with what happens in the mind-independent, external world. Marx, who observes it is a mistake to take the movement of categories for the real act of production, apparently mistakenly attributes this confusion to Hegel. Yet Marx follows Hegel in claiming that what we know when we know, is the product of the mind, which reconstructs what its cognitive object as a condition of knowing it. Marx desires to cognize the social world we in fact experience, as he says "what is given in the head as well as in reality" (Marx 1973: 106).

Marx takes up the same problem, in almost the same words, in the famous Afterword to the second German edition of *Capital*. He stresses the need to describe social development, not in terms of the historical sequence of economic categories, but

rather in terms of the relation among categories in modern bourgeois society. One seeks to describe the subject matter as if it followed from an a priori construction (Marx 1967: 19). Marx, like Fichte, treats the a priori and the a posteriori as two perspectives on the same object. Yet in equating reality with what we experience, he overlooks the distinction, basic to all the German idealists, between the mind-independent external world and phenomena, i.e., the basic difference between what is in itself and what we in fact experience. Marx simply conflates one with the other in failing to note there is no way reliably to know we know the world, or even the social world, as it is. To think otherwise is to think, as Kant is sometimes read, that the observer constructs a representation of the mind-independent world as it is (cf. Hanna 2001: 22). For a representationalist approach, in which there is no other access to reality, there is no way reliably to know that representations correctly represent. In reacting to Kant, Hegel, the phenomenologist, stresses this point in his description of knowledge as a process of trial and error. Marx, who overlooks the difference between his project and Hegel's, is doubly incorrect. First, he incorrectly accuses Hegel of transforming the real world into an idea. Second, he incorrectly contrasts our conception of the world with the material world, which, through a categorial framework, he seeks to "translate" into, or again to grasp through, thought. Yet if the world as we experience it depends on our categorial framework, then categories and cognitive objects are interdependent and a clear distinction between them cannot be drawn.

This problem is reflected in Marx's epistemology. Attention is sometimes drawn to the anthropological element in Marx's position. In writing that "in all the universe man cannot find a well so deep that, leaning over it, he not does discover at the bottom his own face" (Kolakowski 1968: 66). Kolakowski suggests that for Marx we inevitably sense, perceive and know from a human point of view. It follows that the Kantian project of isolating the transcendental logical conditions of knowledge from its psychological conditions simply fails.

In *Capital* (vol. 1, 372, n. 3), Marx refers in passing to Vico's conviction that human history differs from nature in that we have made the former but not the latter. Marx, like Vico, thinks that human beings literally "make" history. He further thinks like Vico that we can only know what we make, according to Marx by reconstructing it on the level of mind. If there is no prior object to be known, then it cannot be reconstructed, and construction is not a priori. Rather, it takes place on the a posteriori and social planes, in the context of an interaction between human beings and between human beings and nature.

Marx's specific form of this claim is problematic. If the social context were in fact wholly "transparent" to mind, then we could indeed reliably claim not only to construct it through the actions of men and women in the social context, but also to reconstruct it reliably within the cognitive process on the level of mind, hence in fact to know it as it is. Marx unquestionably provides a powerful conceptual model of modern industrial society. Yet even on a charitable interpretation, Marx cannot reliably claim to grasp the social world as it is for at least two reasons. First, at most he grasps no more than what at any given time appears to us in experience. Second, Marx proposes one among a series of possible reconstructions of the social world. At least implicitly, there is always a distinction, which cannot be measured or otherwise evaluated, between what we experience and the social world as it is. Since we cannot

reliably claim to encounter the social world as it is in itself, we also cannot reliably claim to reconstruct it. To think otherwise would be to conflate the subjective and the objective, what we seek to know with what is. At the limit it may sometimes appear as if the subject matter we seek to know were ideally reflected as in a mirror, as if it were only a mere a priori construction. But, since we cannot reliably claim to know the world as it is, we cannot reliably claim to know this is the case.

Marx's social epistemology is very different from the Marxist approach. Marxism, which claims to speak in Marx's name, advances a so-called reflection theory. Cognition, according to Engels, consists in a correct reflection of independent reality. In his study of Feuerbach, Engels asks rhetorically if we can produce "a correct reflection of reality" and answers that in philosophy, this question is called the "question of the identity of thought and being." Dialectical philosophy, he maintains, is the reflection on the level of mind of the transitory processes of successive historical systems. For Engels and for Marxism in general, to know requires a reliable reflection of mind-independent reality on the level of mind.

The reflection theory of knowledge has remained popular over many years. It was adopted as early as Bacon, was restated in a different form by Wittgenstein, and recently criticized by Rorty. The basic difficulty of the reflection theory of knowledge lies in the inability to demonstrate a reliable reflection of mind-independent reality.

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Collingwood and Croce

STEIN HELGEBY

The Italian thinker and leading liberal politician Benedetto Croce (1866–1952) and the British philosopher and archaeologist Robin George Collingwood (1889–1943) placed thought about history and historiography at the center of philosophy. Their work has influenced much thinking about history and historiography since the middle of the twentieth century. It continues to stimulate work in areas such as moral, social and political philosophy, metaphysics, the nature of philosophy, and the relationship between historical thinking and action.

In a brief introduction I outline the distinctive characteristics of their work. The second section sketches the place of historiography in relation to other elements of their philosophies. The third section considers their approach to historiographic knowledge, while the fourth draws out the content of history as they conceived it. The conclusion returns briefly to the character of their contributions and its potential for philosophy in the future.

Croce and Collingwood developed a tradition of thought about history that goes back to Vico, Kant, and Hegel. This tradition sees history as a product of reason and produced grand visions of the dynamic and sweep of history. The philosophy of history, an account of how reason unfolds in time, came, therefore, to be seen as fundamental to philosophy, the study of reason in general. Earlier thinkers in this tradition held, for example, that reason produces history through the workings of concepts or peoples. Such thinkers tended to distinguish between history and the philosophy of history. They attributed a higher level of wisdom to the philosophy of history.

The distinctive contribution of Croce and Collingwood was to fully locate reason in history in the individual actions of finite historical actors. For them, history is concerned with human action, and is created by liberty and freedom. A theory of action therefore underpins their accounts of history as an object of study (the metaphysics of history) and their accounts of historiographic thought (the epistemology of history). In discussing their accounts of history, we are therefore drawn into the core of an overarching philosophical approach.

For both thinkers, historiography involves understanding the present, and the importance of historiography lies in terms of the present possibilities it creates. The philosophy of historiography therefore has a practical dimension, and is not simply theoretical. For Croce, historiography enables us to prepare for action. For Collingwood,

historiography shapes our choices and actions. Although there were personal and philosophical connections between the two thinkers, their work needs to be discussed in parallel rather than as a unified whole.

Philosophical Context

For Croce, the context for his philosophy of history and historiography is his philosophy of mind or, as he termed it, his "philosophy of spirit." The "philosophy of spirit" is an attempt to provide a connected and general account of the cognitive activities which characterize mind, such as art and philosophy. For Collingwood, the context is his philosophy of mind and moral choice. Collingwood's philosophy of mind extended to underlying mental activities as well as to cognitive activities such as art, science, religion, and philosophy. For both Croce and Collingwood, historiographic thinking arises from present problems and plays a key role in dealing with those problems.

Croce developed his philosophical system early in his career but continually revised it. He followed Vico in holding that the cognitive activities of mind form a recurring cycle. Croce reshaped this idea into the view that the aesthetic, logical, practical, and ethical activities of mind form a single cycle; first one activity dominates the conscious life of individuals, or even of historical epochs, then another. Croce was influenced by idealism because he held that the activities of mind or spirit represent a single, unified, reality. That is, all aspects of reality, and all knowledge and action, can be resolved into activities of mind. In this respect, Croce was a humanist. In his philosophy, there is nothing but "the eternal alternation of the eternal values or categories or activities of spirit" (Croce 1951: 18). He applied his thought as a powerful tool of aesthetic, historiographic and political criticism and became a leading opponent of irrationalism in all its forms, particularly fascism.

For Croce, the activities of mind can be theoretical or practical. On the theoretical side are art (or aesthetic) activity, and logical (or philosophical) activity. On the practical side are economic (or useful) activity and ethical (or moral) activity. Historiography plays a special role in the cycle of these activities, because historiography combines the elements that define art and philosophy respectively. That is, historiography combines the intuition of art with the logic of philosophy. This idea merits elaboration.

Croce distinguished art from the logical activities of the intellect. Art involves intuitive knowledge of the individual, and concrete knowledge, through the imagination. The activities of art are universal, rather than the specialized or unusual activity of a few. That is, art is an activity of mind which occurs whenever there is expression or language, and which has its origin in our emotions.

The intellect, on the other hand, involves knowledge of universals and relations, and it produces concepts. For Croce, concepts are always concrete and inside history, not beyond it, as for Hegel. This idea ultimately led Croce to identify philosophy with history. Croce consequently preferred to describe his philosophy as "absolute historicism," rather than as idealism, emphasizing that historicism "is the affirmation that life and reality are history and history alone" (Croce 1941: 65). There is no need to

search for meaning, concepts or causes outside of history: "There is nothing more to seek" (Croce 1960: 76).

For Croce, historiography brings together art and philosophy, intuition and concept, because historiography illuminates concepts through individual facts. By illuminating concepts in this way, narratives about history may clarify and help to resolve philosophical problems. Philosophy, in turn, enables us to interpret and narrate history. Croce's commentaries on Marx, for example, placed great emphasis on the value of Marx's economic thought for interpreting contemporary societies. Nevertheless, Croce rejected Marx's determinism as well as all other accounts which understood history as being the product of concepts or ideas which operate separately from the activities of reasoning individuals, including Vico's idea of providence and Hegel's account of reason.

It is important for Croce that the activities of mind are not simply theoretical, but also practical. It is not enough to develop knowledge. When we have done that, we may want to take action. But action does not follow automatically from knowledge. For Croce, practical and ethical activity should be distinguished from the theoretical activities of art and philosophy. That is, practical or ethical action does not simply flow from either intuitions or concepts, but is a distinct activity of the mind.

In Croce's philosophy, historiography has both a theoretical and a practical importance. Historiography is a theoretical activity that plays a cathartic role in preparing for action. That is, we understand problems as a prelude to dealing with them:

Historical philosophy or philosophical history is modest because it continually brings man face to face with reality; having made him achieve the catharsis of truth it leaves him free to seek and find out what his duty is and to create his activity. (Croce 1941: 149)

When we subsequently act, there is also feeling and passion, and therefore the material of new artistic activity; in this way, the cycle continues.

Collingwood's work was implicitly systematic, but focused on key problems. He sought to show how activities such as historiography, art, metaphysics, religion, and natural science were possible. He thought that providing an account of the activities of the mind could help address a crisis in European civilization that had found stark form in the rise of fascism and nazism. Collingwood's argument was that the attempt to state what is most fundamental about the life of the mind in a particular civilization is a key to sustaining that civilization. He described such attempts as metaphysics and understood metaphysics to be a historiographic inquiry.

For Collingwood, history is concerned with human action. When we talk about history as an object we are talking about the realm created by human acts of reason and choice – or, as he put it, "mind." For Collingwood mind is a set of activities, not a substance. He held that mind is activity, and develops through activity, so that it is not always the same. The capacity of people to make rational choices therefore develops over time, and this means that morality, society, and politics also develop.

Historiography enables us to know the realm of human action. By reflecting on the common practices of modern historiography we can develop a philosophy of historiography. At the same time, we are developing a general account of how actions are known. For Collingwood, we know actions by reasoning to the historical situation and choices that agents made.

In Collingwood's view, historiography exists for the sake of rational action. Historiography is involved in all aspects of thought, practical as well as theoretical. Such thinking shapes how situations come to be understood, how choices arise and how decisions are made. This view is unusual and merits explanation.

We can choose only when we are first conscious that we have alternatives. For Collingwood, as for Croce, knowing our emotions and desires is a key to our consciousness of alternatives, because we always desire something that is distinct from another thing. It is expression, and therefore artistic activity, which enables us to know our emotions.

For Collingwood, there are three grounds upon which we can make reasoned choices between alternatives. Collingwood's account of moral action and his characterization of history result from his articulating these three grounds. Firstly, a choice can be for the sake of utility. Secondly, a choice can involve following a law or a rule. Finally, a choice can be an act performed out of duty. Each of these grounds is present in any given act, but only the concept of duty enables us to fully understand an act. We need to understand each of Collingwood's terms in order to grasp his view of history.

We can choose an act because of its utility, such as when it is economic to pursue a certain action. If we want to understand the act fully, however, an explanation in these terms will always be deficient – meaning that there must be other dimensions to the choice. The deficiency can be seen when we realize that a person's desires may focus on a particular kind of action, but their choices are always particular and specific. We may satisfy hunger in many different ways. Understanding why someone chooses to do so in one way rather than another requires more than an understanding of the utility of the means to the end.

We can choose to follow a rule or a law. We can try to understand the act in similar terms, such as when we explain it as demonstrating regularity or a sociological law. But this kind of explanation is deficient also. A rule or a law is not sufficient to determine our actions. In each and every particular case we may view a situation conscientiously, and thereby render rule-following either "unnecessary" or even "vicious." Explanation in terms of rules or laws also fails to account for the specificity of our actions. Collingwood therefore criticized Kant's "categorical imperative" as being a theory of the rules that govern action, which fails to account adequately for actions.

For Collingwood, our acts are always concrete and particular – we always perform this act, and not just one act of a certain kind. Collingwood's idea of duty is an idea of concrete action. It is Collingwood's alternative to Kant's "categorical imperative." On Collingwood's view, it is our duty to perform a particular act in a particular situation. Collingwood reinterpreted a term – duty – that others have applied to action in obedience to rules.

In *An Autobiography* (Collingwood 1978: 100–6), Collingwood observed that we act without rules in two kinds of circumstances, each of which is common and requires that we have a strong sense of our situation. In the first group of cases we have no choice but to act, but have no rule on which to base our acts. This is particularly so in new situations, or when we are inexperienced.

A second kind of situation occurs when we believe that acting according to rules would be inappropriate. This second group of actions involves situations we take very

carefully, rejecting desire, self-interest and rules in order to act appropriately. In such circumstances, rule following would fail to deal adequately with situations in which we find ourselves.

We can act appropriately only if we see our situation clearly. Historiography gives us the necessary trained eye for the situations in which we need to act. To act appropriately, we need the insight of historiography, rather than rules. From this observation, Collingwood concluded that historiography is a key to the diagnosis of moral or political problems.

There is another sense in which historiography is necessary to allow us to act appropriately. For in order to act we need to know what options we have available to us. To know our options we need to know what we are capable of. Collingwood argued that the only way we can truly know what we can do is to understand what we have become. In turn, knowing what we have become involves historiography. Similarly, we can understand others through historiographic accounts about what choices they have to make and what they have become.

To summarize Collingwood's view, we need historiography in order to know ourselves and others. We understand the situations in which we find ourselves by thinking historiographically. Practical reasoning involves seeing ourselves as characters in particular historical narratives. In practical reasoning, the question we ask ourselves is which amongst our competing desires we will pursue. We eliminate various options because they suit our interests less, or go against principles we follow. Beyond such considerations, one act is necessitated by our conception of our situation and ourselves – this is what we choose. In Collingwood's terms, duty is reason, obligating action. The principle of choice from duty enables us to understand not just ourselves in the past and present, but also the acts of others, and so to understand history, the realm of human action.

Knowing History

Croce and Collingwood both believed that historiography could lead to knowledge; they were not relativists in any normal sense of the term. Their theories of historiographic knowledge were, however, significantly different. Croce practiced a form of historiography heavily dependent on textual interpretation, and wrote particularly about the "moral-political" sphere of action. In a famous, and easily misunderstood, phrase, Croce said that "all true history is contemporary history." This does not mean that all historiography is of the recent past, but that historiography to be more than mere chronicle, "must vibrate in the mind of the historian, or to put the matter in the professional language of historians, there must be intelligible documents" (Croce 1966: 497–8, 501).

In his *Aesthetic* (Croce 1953: 29), Croce argued that the evidence to be criticized in historiographic judgement was the evidence "of the best observers, that is, of those who best remember." In his *Logic* (Croce 1917: 156), he aligned historiography and perception. Collingwood was rightly critical of Croce's analogies of memory and perception to account for the knowledge of history, because the knowledge of history involves reason.

Unlike Croce, Collingwood was an archaeologist and historian of Roman Britain, who drew upon a wide range of approaches in his historiography. Modern, scientific, historiography is, for Collingwood, historiography that takes as its only authority the argument and reason of the historian. There is no possible appeal to an external reference point of fact which could guarantee particular conclusions about the past. Collingwood's theory is that historiographic knowledge involves inference, based on the interpretation of what the historian accepts as evidence. Historiographic interpretation and inference are governed by principles that ensure that knowledge becomes possible. The historiographic imagination interpolates and connects actions so that narratives are coherent, but it is governed by the demands of evidence, criticism and the conception of what history is. The regulative "idea of history" is an understanding that history is concerned with action which has developed from ancient Greece to modern times.

For Collingwood, historiographic narratives are historical conclusions expressed as narratives of human action. They make the human past intelligible because they account for acts through the reason of historical agents. The narratives gain their merit by being fully open to and subject to criticism in light of other evidence and inferences. In reconstructing the practical or theoretical arguments of historical agents, historians *re-enact* those thoughts. Collingwood's famous theory of re-enactment is a theory of reason and the conditions of knowledge. It is not, as has often been claimed, a theory of empathy or intuition or simply a methodology of historiography.

The Content of History and Historiography

The accounts of historiography given by Croce and Collingwood imply that history and historiography have a specific kind of content with a contemporary significance. This is a fundamental feature of their views and sets them apart from subsequent thinkers who have sought to make the philosophy of historiography a purely formal analysis. In Croce's own assessment, "historiographic thought, for Collingwood and for me . . . was based . . . on contemporary politics to which it gave sharpness of concept, light of hope and firmness of decision." For Croce and Collingwood, the First World War, in particular, showed that the hold of historiographic thought in European civilization was weak, relative to scientific naturalism (Palmer and Harris 1975: 57–8, 62–3). Their philosophical works sought to redress that weakness.

For Croce and for Collingwood, history is the realm of moral acts or acts of choice. This makes all historiographies specific to an action in a time and a place. This also led both Croce and Collingwood to reject speculative philosophies of history that draw grand narratives from the shadow play of concepts. Because historiography involves concepts from the start, it is already, in Croce's terms, philosophical. Therefore, there can be no philosophy that takes historiography as a raw material on which it superimposes other concepts and categories.

Nevertheless, since historiography is concerned with the realm of action, and arises from contemporary concerns, an account of history also underpins political and social philosophy. In this respect, Croce advanced a theory of liberty and liberalism which became a beacon for intellectual resistance to fascism. Collingwood developed

a theory of civilization and barbarism as the work of mind. Whereas Croce, however, maintained that theory and practice were distinct, such that historiography prepares for action, but does not determine it, Collingwood sought a rapprochement between them.

For Croce, history is created by “liberty,” the moral dimension of human activity. Because liberty creates history, it explains history, and it is the subject of historiographic thought. But liberty is also an ideal which is actively pursued in some times, by some people, more than in others (Croce 1941: 59).

Liberty or moral activity has given rise to the political orientation of liberalism. For Croce, the history of Europe in the nineteenth century was fundamentally concerned with the pursuit of liberty, and opposition to it. The liberalism of the nineteenth century needed to be seen in relation to democracy, economic liberalization, nationalism, communism and socialism, reaction and authoritarianism – but it was distinct from each of these. For Croce, in contrast with the majority of nineteenth- and twentieth-century political thinkers, liberty is just liberty – it is not the same as the concepts of justice or of economic liberty, and he made it his task to restate and develop a philosophical theory of liberty on this basis.

European history was not, however, shaped by the clearest and purest forms of liberalism alone. Rather, liberalism was distorted in a number of ways, including by an assimilation of the concept of liberty to the practical economic and industrial dimensions of nineteenth-century life. In particular, Croce attributed the origins of the First World War to a distorted “ideal of activism” whose “original impulse was nothing other than the principle of liberty” but which was liberty “deprived of its moral soul . . . detached from the past . . .” and lacking “the purity of the end.” Activism had become a “mournful parody . . . of an ethical ideal . . . the celebration of a black mass, but still a mass” (Croce 1933: 342).

Collingwood rejected the distinction between theory and practice. The distinction between theory and practice depends on the assumption that all knowledge is like knowledge in natural science. In natural science, a distinction is drawn between the object and the study of that object. Natural science presupposes the existence of an external natural world, which it “contemplates.” In the case of historiographic knowledge, however, the distinction does not apply. History, the object of historiography, is known from the inside – it is understood by agents who are within it and who create it, including by the way in which historiography shapes future actions.

The practical implications of Collingwood’s philosophy of historiography are visible in his account of society and civilization. These terms became very closely intertwined in Collingwood’s thought. For Collingwood, society is brought into being not by a “consensual social contract,” but by the decision or will of free agents to initiate the contract. The members of a society share the “social consciousness” of those “who are free and know themselves to be free.” Social consciousness can be characterized as “an act of deciding to become a member and to go on being a member” through common undertakings and actions (Collingwood 1992: 139).

There is a continual process by which people who simply have something in common become self-conscious and so will to become a society. In doing this, they civilize themselves. The process can, however, work in two directions. In the constructive vein, civilization is created by will. To be civilized is to live in such a way as to “endeavour

to convert every occasion of non-agreement into an occasion of agreement." Contrasting with civilization, there is a "will to barbarism," that is, "a will to do nothing, a will to acquiesce in the chaotic rule of emotion which it began by destroying" (Collingwood 1992: 183, 307, 309, 326).

To civilize is to undertake a process through which agents come to possess and exercise free will. Civilization is at its most fundamental when parents develop relations with their children that incorporate them into a society. Collingwood, therefore, considered that education was far too important to be left to professionals, and saw their rise as representing a decline in the ability of modern Europeans to civilize themselves. As he put it, a world of "office-drudges" and "factory-drudges" is "a world consuming its own capital of civilization through having wantonly thrown away the power of educating its young." Parents can, however, recover their roles in civilizing children, through play. A civilization can recover its vigor through expression, or art, because this brings the civilization back in touch with the emotions which are the foundations of reasoned choice (Collingwood 1992: 308–17).

A civilization can see itself as pursuing a useful end or as bound by rules, but such a civilization will fail to fully understand itself. Just as choice moves from utility and rule to duty, so a civilization can transform itself by deepening its understanding of history and strengthening the role of historiographic thought in its culture. This idea brings together all of the key elements of Collingwood's thought.

Since historiography is fundamental to practical action, a society that consistently understands itself as historical would develop a corresponding civilization. That is, the members of such a society would see themselves and others as making choices from duty. They would not claim to explain their own actions, or those of others, solely in terms of their usefulness or their adherence to a rule, but in terms of their individuality. A civilization that truly understands itself is one that can more fully address its moral and political problems.

Conclusion

Croce and Collingwood represent a tradition of thought about the philosophies of history and historiography that makes historiography central to philosophy, by seeing history in terms of the activity of reason. Croce developed a unified account of mind with history at its center. Collingwood developed a more critical approach which made the understanding of history as subject and as object central to the task of philosophy.

Both thinkers departed from much earlier thought about historiography and history. Because, for them, the philosophies of history and historiography reflect on the concept of history and the methodology of historiography, it does not offer a superior level of wisdom. Each rejected speculative philosophies of history that posit a grand schema or design superior to the events of history. Each also rejected a moralising approach to history, or the idea that the study of history is a guide to ethics.

Croce's and Collingwood's approach to historiography differs also from later analytical philosophies of historiography. Their work has a metaphysical or substantive dimension that is quite alien to much analytical philosophy. Similarly, their view of

the philosophies of history and historiography as being at the center of philosophy stands in sharp contrast with later thinkers who have viewed the philosophy of historiography as a branch or corner of philosophy, concerned with a discrete field of study, conducted in a theoretical manner, and neutral as to its significance.

Taken together, Croce and Collingwood show not only how a key tradition of thought about history could take new forms, but also how thought about historiography could provide a powerful alternative to scientism in philosophy and culture more generally.

Much of the debate about the philosophies of history and historiography in the past twenty years has been concerned with the writing of historiography, the historiographic narrative. Writing is, though, only a finished product of a process of thinking. The process of historiographic thinking is not one for the academies but, on Croce and Collingwood's accounts, is integral to the activities of the mind. Instead, then, of debating what it is for historians to write historiography, a more rewarding debate would be about who thinks historiographically, when and in what circumstances they do so, and what this way of thinking implies for their actions.

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Phenomenology

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In this chapter, we are concerned with “Phenomenology” as the philosophical method introduced and championed by Edmund Husserl in 1900–1 as well as its later transformation by Husserl himself and by other thinkers into a philosophical school and tradition. Initially, phenomenology did not deal with the problem of history, but history became important, even crucial, for the later Husserl, and for philosophers, like Heidegger, who were greatly influenced by him.

The method of phenomenology does not lend itself so much to substantive questions about the direction of history or to methodological and epistemological issues about our knowledge of history, historiographic explanation, confirmation, and so on. Rather, phenomenology’s primary engagement with history comes in the form of the following types of questions: (1) Is cognition and, more generally, experience, always historically conditioned and shaped? If so, what implications does this have for philosophy? Can the phenomenological method, in particular, accommodate the historically shaped nature of cognition and consciousness? (2) Are human beings fundamentally historical beings and what does it mean to insist on this “historicity”? (3) Does historiographic knowledge depend on our pre-theoretical historicity? If so, how?

We will first survey the contributions of the classical phenomenologists before turning to the present state of, and future prospects for, such research.

Husserl’s Phenomenology

Edmund Husserl (1859–1938) introduced phenomenology as a method, not a doctrine. Phenomenology is a radically new way of practicing philosophy with the aim of showing how the world is “constituted” by conscious subjects. Husserl sought to show why our knowledge can be considered truly objective and not merely a function of the way human beings happen to think (at a given point in time). Phenomenology consists of a painstakingly detailed and rigorous account of our consciousness and experience. It describes acts of consciousness and their objects, but it does not do so empirically by tracing their causes and effects according to scientific laws. On the contrary, phenomenology explicitly brackets or sets aside the assumption that there is an external world governed by the laws of science. This crucial step is called the phenomenological

reduction. The method consists in analyzing only what presents itself to consciousness and to focus on it precisely in the way it is given to us. Thus, phenomenology strives to have no “presuppositions” about anything outside of consciousness. The descriptive method does not amount to mere introspection, that is, simple observations about one’s consciousness. Rather phenomenology attempts to grasp the essence of our acts of consciousness and their objects through reflection by means of a technique whereby we entertain variations of consciousness to see what is common and essential to all such variations. Previous philosophers have often taken the “I,” in which consciousness occurs, to be an entity in the world. Husserl, though, regards the “I” not as part of the world, but as that which makes possible, “constitutes,” and “builds up” the world.

According to Husserl, consciousness is always directed at objects. Some objects are physical objects, such as table and chairs. We do have direct consciousness and experience of such objects, but not all their aspects are presented to us directly. For example, when we perceive a table, we do not see the rear side of it, yet we experience it as having one. Thus, many aspects are experienced only indirectly or peripherally as belonging to an object’s “horizon.” This is one example of the results Husserl arrives at by means of his careful description of how we experience things. Significantly, there is also a temporal flow to consciousness such that things are grasped not in just in the way they are at present, but also in terms of our past experience of them (retentions) and in terms of the way they are anticipated to be in future (protentions). For example, we hear a musical tone as being in or out of tune depending on the tones that have preceded it and the tones we anticipate hearing. Also of tremendous importance is the fact that consciousness is intersubjective insofar as we experience the world as experientiable by others whose consciousness resembles our own

Phenomenology and History

Beginning in the 1910s, Husserl shows increasing interest in the historical dimension of human life, yet only after 1930, especially in *The Crisis of the European Sciences* and in posthumously published manuscripts, does history become central to his investigations as a philosophical problem. This re-orientation is fundamental and in many respects surprising. After all, Husserl’s bracketing of empirical matters and causal explanations would suggest that history cannot enter into phenomenological inquiry. More broadly, it is often supposed that transcendental philosophy (Husserl regarded phenomenology as transcendental) in its concern with the possibility of *any* experience *überhaupt*, is incompatible with historical change and temporality. On the other hand, there are internal, phenomenological reasons for taking history into account. If phenomenology deals with what is presented to consciousness, then if consciousness is shaped by its contingent location in history, so is the way in which things are presented to consciousness. In other words, history is always already phenomenologically present and effective in our experience. There are two further sources in Husserl’s earlier thinking for a specifically *phenomenological* problem of history (Carr 1974). First, Husserl’s theory of the inner temporality of consciousness regards consciousness as essentially shaped by retentions from the past leading to persisting beliefs and habitualities which importantly shape our experience. This is Husserl’s *genetic* phenomenology; it

is committed to the presence of the past in any given present experience. Second, Husserl's theory of intersubjectivity recognizes that we adopt much from the community, which surrounds us. Thus, conscious acts grow out of culture and become, to some extent, "sedimented" in cultural attitudes and predispositions. What we see and think is a matter not only of the present moment and one's own individual experience, but also very much bound up with the culture and point in history in which we find ourselves.

All of this motivates the development of the concept of what Husserl calls the life-world (*Lebenswelt*). The life-world is not the world as the totality of all that exists or the physical world as studied by science. It is the world in which we live and carry out our various activities. It is shaped by our culture and particular form of life. This taken-for-granted world is the framework and "horizon" of all consciousness, without exception. It is "the pre-given world in which science and every other life-praxis is engaged" (Husserl 1970: 121). For Husserl, the life-world is not a distortion of the physical world. Rather, the life-world is the most real world, i.e., more real than the world "constructed" by science, which is built upon and logically "grounded" by the life-world.

The life-world is sharply distinguished from the world as understood by science. Yet, at the same time, Husserl acknowledges that scientific accomplishments flow back into the life-world. So, does the life-world encompass scientific theory or does it not? We must stick with the world and its objects as they are really given, namely as having certain qualities initially learned through scientific activity. Indeed, many of the objects we encounter exist only thanks to science such as cars, refrigerators, and the like. But our life-world encounters are only informed by science in the most rudimentary way; the full-blown theory itself may be unknown and, in any case, remains unnoticed in ordinary, everyday experience. Analysis of the life-world does not presuppose the truth of any scientific theory or even the existence of the world. Most important, the life-world is thoroughly historical. Human beings live in ways that change over time and, as a result, so does the life-world. In Husserl's late work, far from being a separate problem for phenomenology, history is at its core.

Husserl died soon after his historical turn. Nevertheless, the project of constructing a historically aware phenomenology on the ground of the given life-world remained. We take our consciousness as it is, historically shaped, through and through, but without any further theoretical framework or assumptions. We carry out Husserl's painstaking, phenomenological description of a life-world embedded consciousness. This would encompass all the standard intentional acts and objects such as perception, memory, awareness of other conscious beings, but now with attention to the fact that these acts and objects are given to us as embedded in history. All systematic theory, whether scientific or philosophical, grows out of the life-world. So there is a further project consisting of the examination of how primordial life-world experience at a given time leads to "constructions" and "idealizations." Instances of such constructions might include religious or artistic thought, but the scientific construction of the world is the one Husserl pays most attention to (since it is often regarded as the single correct representation of reality). For Husserl, not only is the scientific construction of the world an essentially historical phenomenon, it is also one that can only be understood through its historical becoming or genesis – starting with the life-world, passing through its idealization in geometry and mathematics, arriving finally at the methods and principles in force today. It would seem that genetic or historical treatments might

be applied to other fields such as law, the arts, social practices, and even to philosophy itself. In the *Crisis*, Husserl maintained that philosophy, too, is a historical phenomenon requiring a “historical, backward” self-reflection. He argued that a certain unity and *telos* could be discovered underlying its entire development. Whether or not this is so, Husserl’s discovery of the phenomenological method and his later grounding of this method in the historically embedded life-world set an important agenda for future research.

Heidegger

Martin Heidegger (1889–1976) was Husserl’s assistant and even used the term “phenomenological” to describe his famous *Being and Time* (1927) (henceforth “BT”). Yet his philosophy diverged from Husserl’s almost from the beginning. His aim was less epistemological and more ontological, his philosophy was not a theory of knowledge but a theory of being, i.e., what it means to *be* in the first place. Like Husserl, Heidegger wanted to account for phenomena as they are given to us. But Heidegger departed from Husserl’s cognitive focus on judgment-producing thoughts and rejected Husserl’s assumption of a mental realm separable from all the rest. For Heidegger the starting point was not consciousness but *Dasein* (German for “existence”), i.e., human being as an entity defined by its practical and existential engagement with the world – doing things rather than just perceiving and arriving at judgments about them.

While BT aims at the question of being in its widest sense, the parts that were published (before the complete project was abandoned) deal with an account of what it is to be *Dasein*. Heidegger lays out the *existential* structures of human existence, meaning the universal and necessary structures that underlie all knowing and acting. These add up to a unitary picture of *Dasein*, as “care” – a being that inherits a situation (thrownness), tends to be lost in everydayness (falling) and constantly projects itself into not-yet-realized possibilities. One of the key existential structures in BT is historicity (*Geschichtlichkeit*). According to Heidegger, we are essentially historical beings. This does not mean simply that we always find ourselves at some point in history. It is not that we are in history, but that history is “in” us. But what does this mean?

We must start with Heidegger’s ideas about temporality on which historicity depends (the account of historicity appears toward the end of BT). Influenced by Husserl, Heidegger rejects the common, atomistic understanding of time as a sequence of now-points in which the past and future are not real because they are never actual. Rather, *Dasein*’s temporality must be understood as a unity of all three temporal aspects, and not just through conscious remembering and expecting. Heidegger adds to Husserl the recognition that such temporal unity issues from doing and acting. Heidegger refers to such temporality as a “stretched-out-ness” (*erstrecktes Sicherstrecken*). *Dasein*’s existence comes in the form of a life stretched out between birth and death, where one’s birth is always still in a sense actual and one’s death is always in some manner present.

Historicity concerns the deeper meaning of this process of being stretched out between birth and death. This, Heidegger argues, is different from the “vulgar” conception of history that is ambiguous and confused, sometimes understood as what lies

in the past (leaving out the present age), and sometimes as what is epoch-making (leaving out most ordinary people and events). Heidegger says that history and historicity must be conceived as the very “subjectivity of the historical subject.”

His exposition of historicity in BT is brief and begins not from the point of view of the way we always are, or the way we typically are when lost in the everyday, but from the point of view of *authentic* existence – the way we are when we are most true to ourselves. In its authentic mode, *Dasein*’s way of existing historically is to grasp its unique or “ownmost” possibilities resolutely (or “pro-actively” as we now say). Because *Dasein*’s authenticity is indicative of its deeper nature, this account of *Dasein*’s historicity amounts to telling us about the deep meaning of what it means to be historical through and through – a meaning that is hidden from us when, in everydayness, we are blind to our ownmost possibilities. Heidegger’s exposition has both a normative aspect, the authentic mode, as well as a descriptive aspect, the invariant features revealed by this authentic mode (though Heidegger himself denied any normative implication to the authentic mode of *Dasein*).

Heidegger’s account of historicity is based on his key concepts of *thrownness*, the unchosen circumstances one finds oneself in, and *project*, the future possibilities one envisages out of such circumstances. Where do these possibilities come from? The first step is the recognition of what Heidegger calls one’s “heritage” (*Erbe*), i.e., the inherited circumstances and traditions that one always takes up in one way or another. Next comes the formation of a goal that is both shaped by the past and freely chosen. He calls this “fate” (*Schicksal*). Such fate is neither accidental nor predetermined (Heidegger is no fatalist), but self-generated. One is constrained and shaped by the past, but free to appropriate it in this way or that, and form the future on the basis of that appropriation. This whole process is carried out not in isolation from others, but in the context of one’s membership in a community and, more specifically, in a particular generation or cohort. The social character of fate makes it into what Heidegger calls a “destiny” (*Geschick*). This ever-present process of envisaging and bringing about a shared future on the basis of a shared past is at the core of what it is to be *Dasein* and it contains the deeper meaning of what it is to be in history, or better, for history to be “in” us.

The philosophy of historiography has often been concerned with epistemological questions. For Heidegger, acquiring knowledge of history is a derivative problem because it depends on the prior temporal and historical nature of *Dasein*’s existence. We are historical even when uninterested in or oblivious to the possibility of historiographic inquiry or the impact of the past. Nevertheless, Heidegger does have something to say about historiography as a scholarly discipline or inquiry (*Historie* as opposed to *Geschichte* and *Geschichtlichkeit*). First, he argues, echoing Dilthey, that historiographic inquiry is only possible in virtue of the temporal and historical structure of our thought and action. Second, he argues that the real subject matter of historiographic inquiry is not a series of facts or events or the discovery of laws; rather historiographic inquiry must be directed at the possibilities that human beings have envisaged on the basis of their particular situations. One might say that history is all about past futures, i.e., about the manner in which, in the past, *Dasein* has projected itself in response to its past.

Much of Heidegger’s later work was on the history of being, the history of the ways in which being unfolds and reveals itself to *Dasein*, for example, for modern *Dasein*

everything appears as a raw material, standing in reserve. Ours is a technological age in the history of being (see Guignon 2005). This line of thinking amounts to a philosophy of history that explains the meaning, direction, and shape of history. In sharp contrast to progressive philosophies of history, Heidegger sees the history of being as in decline, a distancing of *Dasein* from Being, from truth. This decline started with the invention of metaphysics by Plato. Heidegger's history of being begins with a fitting awe and mystery in the face of being that eventually gives way to calculative thinking, the domination of beings by *Dasein* and ultimately to a kind of self-destructiveness from whose grips perhaps "only a God can save us." Whether or in what sense this history of being is truly phenomenological rather than speculative or mystical is a further question. Heidegger appealed to the need for the *destruction* of the history of ontology in order to reconnect with the lost sense of Being and to uncover unrealized possibilities for philosophical understanding. This project in a different, linguistic guise, influenced the project of the deconstruction of metaphysical language by some of Heidegger's followers.

Later Developments in Phenomenology

Until about 1945, the work of Jean-Paul Sartre (1905–1980) was clearly intended as phenomenological. However, during this time he did not devote himself systematically to the philosophy of history, as he did in his later Marxist-oriented writings. Still, in his *War Diaries* from 1939–1940 (1999: 294–302), Sartre argues that historical explanation typically involves incommensurable levels of analysis (e.g., psychological, cultural, political) and suggests that these disparate levels can only be unified by starting not with the facts as such, but with human beings and their self-projections in concrete situations in order to reconstruct courses of action and arrive at what he calls the "event." While Sartre's *Being and Nothingness* (1943) does not discuss history as such, it offers some innovative conceptual tools for its analysis. Sartre accounts for human existence in terms of a tension between facticity (what is given to us) and transcendence (what we are free to do with what has been given to us). It is this tension that underlies the possibility of either authentic action or bad faith in which we deny our freedom or our inherited circumstances. In the section entitled "The Look," Sartre explains the social nature of our existence as issuing from the fact that each of us becomes an object for other subjects in a way that lies beyond our ultimate control. Thus, many social and historical categories and identities, such as "bourgeois," "French," "woman," "Jew," are foisted on us in ways that we can neither prevent nor disavow. Historical events are often rooted in just such predicaments and challenges. Sartre's post-war writings on history are more systematic, but they abandon phenomenology in favor of other methods.

Like Sartre, Maurice Merleau-Ponty (1908–1961) turned to the philosophy of history largely outside of his phenomenological work. In his *Phenomenology of Perception* (1945), he tried to show that human experience at all levels is a fundamentally holistic and bodily experience so that there is no divorcing consciousness from the body as we live in and through it. He also takes our intersubjectivity and sociality into account. He argues that historical materialism need not be reductionist and purely economic

but should include motivational, experiential and existential analysis (2002: 198ff). In his chapter on freedom, he insists, implicitly against Sartre, that freedom is not an all-or-nothing affair. He maintains that history must be understood not merely from the outside, but from within lived experience (2002: 514ff). Thus, being working-class is not merely a matter of structural relations, but a “way of being in the world.”

The work of Alfred Schutz (1899–1959) is a rigorous attempt to apply phenomenology to the social world. Schutz’s *Phenomenology of the Social World* (1932) addresses the meaning of human action and the social world, both central to human history. Schutz explains human action as a conscious directedness toward determinate future goals involving both “in-order to motives” and “because-motives.” He discusses our ability to understand the subjective meaning behind others’ actions (called *Fremdverstehen*). He then comes to the constitution of the social world, built on our immediate surroundings but expansive enough to include the world of predecessors, contemporaries and successors as well as the experience of a “we-relationship” in terms of a “face-to-face relationship” of mutual participation in each other’s lives.

Paul Ricoeur (1913–2005) perceived himself as a phenomenologist throughout his career, though he blended it with the insights of hermeneutics, the theory of interpretation. What results is an investigation into the constitution of meaning that regards the process as historical and oriented on real-world experience. He came to a phenomenological-hermeneutic philosophy of history via an original theory of narrative. On his account, narrative is central to both fictional and historical discourse because it is already implicit in the way we grasp our lives. However, in *Time, Narrative and History* (1986), David Carr (1940–) criticizes Ricoeur on the grounds that narrative appears merely as a description or ordering of our experience rather than as the structure of our experience itself. Carr argues, on phenomenological grounds, that our experience has a fundamentally narrative structure prior to our description of it as such. Carr’s position is unique in the philosophy of historiography in attributing narrative structure not just to the historiographic narrative, like many others, but also to human historical experience itself. He argues for commensurability between the structure of narrative and experience. The details of that commensurability, and how multiple narratives can relate to the same experience are the main challenges to this approach.

Prospects for a Phenomenological Philosophy of History

Much work today consists of commentary on and reconstruction of the arguments of the leading phenomenologists of the past (Husserl, Heidegger, Sartre). While interesting and important, this scholarship typically remains within the orbit of a past thinker’s system rather than dealing with the philosophical problems independently. Further, many of those historical figures provide rich, but not always phenomenological, analyses of history (e.g., Heidegger’s history of being or Sartre’s Marxist analysis). A distinctively phenomenological approach to history has yet to be fully worked out.

This gaping hole at the center of our question may represent either a tremendous opportunity or a symptom of the impossibility of a phenomenology of history. The pessimist might say, in the spirit of the early Husserl, that phenomenology and history are simply mutually exclusive. Yet an optimist could hold, in the spirit of the later Husserl,

that phenomenology is one of the most promising ways in which to approach history from a genuinely philosophical point of view. Phenomenology could offer a philosophy of historicity, neither a philosophical account of history, nor of historiography – our knowledge of history and its limits – but of what it means to say that we are historical beings in the first place, or that we experience ourselves, others, and objects around us, as having an essentially historical dimension. As Carr says: “[A] phenomenology of history would ask how history is experienced, that is, it would inquire into history as a phenomenon, and into the experience of the historical. In what forms does history present itself to us, how does it enter our lives, and what are the forms of conscious experience in which it does so?” (Carr 2000).

We are historical beings through and through; we experience the world as being historical, regardless of whether we write or even study historiography. Thus our historicity characterizes our existence prior to any systematic historiographic research. This makes the phenomenological approach distinct from the epistemological approach. It is about what precedes, exceeds and, perhaps even eludes, our knowledge of historical events. History is part, and a deeply entrenched part, of our mundane, pre-theoretical consciousness and experience. But what exactly does *this* mean, and what might phenomenology tell us about the historically structured nature of ordinary experience?

By and large, the past is given to us as real and knowable and we must proceed from there to see how it is given and how it functions in perception, thinking, language, and action, how it shapes our very reality. The world is given as pre-dating one’s own birth. It is also given as intersubjective or social. However, is our experience always given in the form of a narrative, with a beginning, middle and (anticipated) end, and if so, why and how? Is the past necessarily given as a fount of tradition to which we feel allegiance? Are objects and institutions given to us as having a certain meaning in virtue of their place in history (as it is with, say, artworks), and if so, why and how? Is history a point of orientation for ourselves in a way analogous to spatial orientation? Is history always experienced in terms of periods or eras, even prior to systematic inquiry? Is it always experienced in terms of progress or regress? Can we say that the very perception of, say, a dollar bill, not as a physical object but as a currency unit is a historically shaped perception? And what is the nature of such historical shaping?

There looms one very large background question concerning the nature and method for answering the questions just suggested. Will these questions be framed and answered in a universal, transcultural and (paradoxically) ahistorical fashion or in terms of the respective historically contingent circumstances of a given history? For example, can one say something phenomenological about the role of tradition or progress/regress in general or can one only talk about such questions with respect to specific historical situations? It would seem to lie in the very nature of *transcendental* phenomenology as conceived by Husserl that it should concern historical consciousness abstracted from all particulars. Yet it is not clear whether this is possible, especially with such empirically changing phenomena as tradition and progress. Perhaps, it is fair and legitimate for phenomenology to strive for the transcultural, realizing all the while that such a goal can only be a regulative ideal.

On the other hand, it seems that it would be worthwhile to look at the way this or that community contingently exemplifies historical consciousness. For example, one might look at the particular historicity of say German culture today. What does

allegiance to tradition mean there? Clearly such questions are already explicitly debated, however, what would make such an investigation phenomenological is that it would look at the unthematized assumptions about historicity operative in a given cultural situation. As Carr puts it, the task would be “to articulate the collective memory, to raise it from the level of tacit assumptions, even practices and attitudes, to that of an explicit account” (Carr 2000). Yet because of its attention to empirical background circumstances, for many this project might not count as truly philosophical. Still, it would belong to the realm of applied phenomenology as an endeavor squarely at the crossroads of historiography and phenomenology.

Unfortunately, this last issue of whether historicity itself is transhistorical or not leads to fundamental difficulties in the very program, construed either way. Can structures of historicity be ahistorical? With something like perception, there is good reason to believe that all human beings, for example, perceive objects as having back sides and other such features. But with historiographic categories, there may be too much variance among different human beings and different historical cultures. Granted, perhaps all human beings tacitly believe that the past is real, somewhat knowable and temporally, even narratively, held together. But is there any reason to think that there are further transhistorical features of historicity than just these?

Yet, if we say that historicity itself is thoroughly historical and variable, then another problem appears. Is such historical variance tractable by means of phenomenological methods? To return to the example of the pre-theoretical place of tradition in contemporary German culture, can this be gotten at through reflection on Husserlian essences? Perhaps it cannot, due to phenomenology’s method of starting from individual consciousness. It might be better to get at this through interviews or reading cultural products or other non-phenomenological methods.

What this means is that phenomenology may intersect with historiography in important ways, but in ways that implicate a rather impure sense of phenomenology. Phenomenology may have to accept that, when it comes to history, it must be culture-specific. Phenomenology may have to work in tandem with methods of inquiry that are not phenomenological such as those of the social sciences and textual analysis. In the end, history and historicity may be most fruitfully understood not by means of phenomenology with a capital ‘P,’ i.e., Husserl’s original method, but by phenomenology with a small ‘p,’ meaning here an eclectic analysis of the way in which history is given pre-theoretically and pre-thematically to a specifically determinate “us.”

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Jan Patočka

IVAN CHVATÍK

Jan Patočka (1907–1977) was a Czech philosopher whose work has aroused worldwide interest. Since the past 1970s, he has been translated into many European and several non-European languages. After a hardly enviable career as a philosopher banned from publishing during most of his working life, Patočka became an important public personality. Together with Václav Havel, the future President of the country after the fall of communism in 1989 and Jiří Hájek, former Minister of Foreign Affairs during the period of the “Prague Spring” (1968), he was one of the three first spokespersons of the human rights dissident movement of Charter 77. After signing Charter 77 in early January 1977, Patočka wrote several short samizdat proclamations explaining the meaning of the Charter. Patočka’s last political performance cost him his life. As spokesperson for the Charter, he met with Dutch Minister of Foreign Affairs Max van der Stoep, then officially visiting Czechoslovakia. Though private, the encounter in fact took place as a press conference with western journalists present and it made Charter 77 internationally known. Less than two weeks later, exhausted by police interrogations, Patočka died.

Though seemingly apolitical, Jan Patočka was well prepared for this radical commitment, particularly through his philosophical work, an important part of which was concerned with the philosophy of history.

Jan Patočka’s philosophy included a strong component of philosophy of history from the very beginning of its development. While studying Edmund Husserl’s phenomenology and reflecting on Husserl’s idea of “the lifeworld,” Patočka concluded that it is not a universal fundamental stratum of human existence, as originally conceived by Husserl. The lifeworld has been changing through history. Next, Patočka came under the influence of Heidegger’s *Being and Time*. In his first book, *The Natural World as a Philosophical Problem* (1936), Patočka outlined his inquiry as a continuation of Husserl’s phenomenological programme, to obtain access to the structure of “concrete human life in the world.” From the very beginning, he believed that this calls above all for a “meditation on human history,” a phenomenological philosophy of history.

Patočka, like Heidegger, understood the human way of being as a unique temporal entity that “is” inasmuch as among other things it understands its situation rooted in the past. It projects on the ground of this understanding its own future possibilities,

and acts presently in this opened horizon. In his paper, "Some Comments on the Concepts of History and Historiography" (1934: 35–45), Patočka quotes Heidegger to criticize the Husserlian concept of the soul as a series of intentional acts and experiences for still treating consciousness as a thing like other things. In his later project of "a-subjective phenomenology," Patočka (1970, 1971) proclaimed Husserl's method of absolute reflection an illusion – it would mean limiting ourselves to merely monitoring our own personal facticity. We could then in no way grasp "the relation of life's many moments to the *oneness*, which is never in life ready to hand, but is continuously being recreated" (1996: 41). Patočka also agrees with Heidegger's *Being and Time* that man – in contrast to things and animals – *is* in such a way that he always understands his Being in one way or another: he "knows himself human," "knows that we are in the world."

The human being is never alone in the world. To be in the world means to be with things and with others. "Our being-in-the-world is being in a world of people; the human being is a *zoon politikon*" (1996: 42). Human freedom is transcending facticity through projecting possibilities. However, this freedom is always situated. Distance from facticity does not mean that facticity has been done away with. Our facticity – i.e., our being thrown amidst things and situations created by the acts of those who projected their possibilities in the past – limits our freedom. Our free potential, which is up to us to grasp, is rooted in these limitations. Accordingly, the human manner of being has a historical character.

Patočka had therefore taken a deep philosophical interest in history since his 1935 essay "Some Comments on the Concept of World History." He distinguished "superficial" or "surface" historiography, "where events and their protagonists are not described with a view towards grasping their *meaning* for life, i.e., towards grasping the energy which fuels them" from what he describes as an "inner" or "deep" historiography, "formed by the development and conflicts of these creative energies, of these powers which we deliberately render our life tributary to" (1996: 51).

"Deep" historiography seeks to trace "flowing life with its possibilities, the convergence of which in simultaneous unity forms the world" (1996: 55). It is the "historiography of the *life-form* or *world* of a specific historical community" (1996: 52). Patočka aims here at the *world* as the structure of our understanding of ourselves and things: a structure which our acts clarify, enlarge, or narrow. Historiography of this universal structure, of *the world philosophically understood*, is what Patočka means by the *philosophy of history*. This has two faces: A philosophical historiography of the world in general, "the analysis and constitution of world and time from the view of philosophical reflection" (1996: 55), meaning the history of humanity's understanding of the essences of itself and of things in different periods of time. It also means the philosophy of history as world historiography, the reconstruction and interpretation of forces and powers, active in history, which interest us inasmuch as their meaning and effect persist up to the present day. Only such a dualist "philosophy of history can lead to a fundamental clarity which is at the same time metaphysical" (1996: 55). By stressing this fundamental clarity, Patočka, of course, acknowledges his debt to Plato, Husserl and their concept of society "based on the life of the spirit" (1996: 22), through philosophy – "a science rooted in absolute accountability, a science about Being as such, about the ultimate grounds of all value." Patočka explicitly quotes Husserl's definition

of the task of philosophy to create a universal and definitely rational consciousness of humanity, to put man on the road towards genuine humanity (1996: 23). This Platonic accent on clarity is, according to Patočka, what gave birth to the historical world in the European sense, and thus to Europe.

As stated above, our present situation is always co-determined by past powers. "History is an objective power beyond our control, though, on the other hand, we alone keep it alive here and now" (1996: 39). History determines us and we determine history. "The past is thus an urgent challenge to our freedom" (1996: 44). Historical man wants to understand his situation, "to clarify his relationship to the wave on which he is riding." To be active, to make decisions as a free historical human being, means therefore to freely accept our ontological status as situated being, to explicitly become who we are and, in this clarity about ourselves, to grasp the possibilities which open up to us. Patočka then formulates his understanding of Heidegger's concept of authentic existence in historical terms: "We comprehend freedom by grasping it in a historical situation, by becoming ourselves, unshakable, stronger than the world; by means of his decision, man thus transcends the world without leaving it." This also gives us an answer to the question of meaning: "Our understanding of history produces the meaning contained therein . . . The meaning of history is nothing universal, precisely achieved once and for all, an idea perhaps carried out in the history of a specific nation. On the contrary, it is the continuously happening and struggling repeatable possibility of free being" (1996).

Between these early positions and the better-known texts from the 1970s, Patočka has gone through the the Second World War and the experience of both the Nazi and Communist totalitarian regimes. During this time, he continued to develop his philosophy, including of course his philosophy of history, as he planned in the 1930s. The results of these inquiries are now gradually being published for the first time, from the manuscripts, in the Prague edition of his *Complete Works*. The texts concerned with the philosophy of history are mainly in the first three volumes of the *Complete Works*, under the heading of *Care for the Soul*. This title is a translation of the Greek *epimeleia tes psyches*, the most pregnant abbreviation for Patočka's philosophical concept of history.

For Patočka, history in the proper sense, the subject of his philosophy of history, is about changes in the human search for the meaning of history. Philosophy of history is in that respect about the history of philosophy of history. History and philosophy are aspects of each other. Geographically, history emerged and took place in Europe and through Christianity in the near East. What happened before the emergence of philosophy and the search for meaning, or in regions where no philosophy has emerged, is all pre-history for Patočka. History began in Ancient Greece, reached its acme in the attempt to realize a Christian *sacrum imperium* in the Holy Roman Empire of the middle ages and ended in the twentieth century with the two world wars and the self-destruction of Europe. It is not clear what shall follow now, after the fall of Europe; Patočka tried to formulate the philosophical-historical legacy of Europe to the rest of humanity and to determine whether humanity still has or *wants* to have history.

The most concise expression of Patočka's idea of Europe and its history is in his last essays from the 1970s, mainly in the six *Heretical Essays in the Philosophy of History*

(henceforth HE in reference to the 1996 English translation). In the German variant of the fourth essay entitled “Europe and the European Heritage,” Patočka explicitly formulates the central idea of his philosophy of history: In the European world, “radical reflection, which came to be only in Greece, fights against, forms and attempts again and again to come to terms with unreflective, non-philosophical experience” (2002: 241).

In Patočka’s view, history begins with a shock. The so-called pre-historic period declined with the gradual collapse of the former closed world of myth, where man lived in the safety of a “pre-given meaning,” taken for granted (HE 12). Such a world, Patočka writes, “is meaningful, i.e., intelligible, inasmuch as it houses powers – demons and Gods – that are superior to humans, rule over them and decide about their fate.” It is “the world prior to the discovery of its problematcalness” (HE 12). Although there is much mystery in it, there is no room for questions, since “questions assume a possible liberation, a distance from what we are and what surrounds us” (2002: 28a). History begins, in Patočka’s opinion, where this “distance” is established and this “liberation” takes place, where man explicitly starts asking questions that were unnecessary while he was living in myth. There are, however, no ready made answers to these new questions. “The problematic character, not of this or that, but of the whole as such” (HE 25) emerges. The discovery of this new, all-inclusive problem is seen by Patočka as a shock which fundamentally changes the prior human way of life, the human world and man himself. These new events alone are, according to Patočka, what makes up “history.”

In short, Patočka considers the beginning of history “as a rising above decline, as the realization that life hitherto had been a life of decline” (HE 102). Whether man was concerned merely with securing his basic survival needs, or whether he succumbed in ecstasy to orgiastic rituals, his life was decadent insofar as it was life lost in everydayness without awareness of its problematcalness asking about its meaning. “The Greek polis, epos, tragedy, and philosophy are different aspects of one and the same élan which means rising above decline” (HE 103).

The aim of free life is thus free life itself, whose meaning is to maintain its freedom. Freedom is attained primarily in a life-and-death struggle against those who wish to deprive the free of their freedom. This struggle, which is ultimately about coming to terms with the problematcalness inherent in the situation of free people, gives rise to the “spirit of the polis,” which is a “spirit of unity in conflict” (HE 41). The struggle, “far from being the destructive passion of a wild brigand, is, rather, the creator of unity. The unity it establishes is more profound than any ephemeral sympathy or coalition of interests; adversaries meet in unique accord about the destruction of a given meaning and so create a new way of being human – perhaps the only mode that offers hope amid the storm of the world: the unity of the shaken but undaunted” (HE 43).

The novelty of political life lies in the understanding that free life can only be maintained in a community where the free create and preserve for their rivalry an open space inside which they are united. This space is the polis, the Greek city-state, as it arose at the dawn of history as something unprecedented, a community of the free who agreed on laws, on *rules*, on something that they explicitly viewed as *right* and that they were prepared to respect from then on as *law*. The Greek polis had a *politeia*, a constitution engraved on stone and on display in a public place, a set of laws by which

the community was instituted and which emerged, in controversy and dispute, as something which ultimately stood above the parties to controversy and made their freedom possible.

However, the struggle does not end with the founding of such a community. Freedom's arena is permanently endangered, more from within than from without. Historical action consists of resisting these dangers and pursuing possibilities that seem to strengthen freedom. One must always be vigilant and ready to take action when necessary. But how is one to know when that is? This point too will necessarily be disputed.

According to Patočka, this is where philosophy comes into being. For the free and responsible, it is not enough to have agreed on something, to have given themselves laws recognized as just and right. Aware of the frailty and controversy from which the community's laws has arisen and which remain a danger to them, they aspire to know whether they are *truly* good, what *in truth* threatens them – they aspire to know how matters *truly* stand, regardless of the situation, regardless of who has won in dispute. They seek knowledge of *the truth as such*. But truth, which is independent of context cannot be truth about the particulars of the situation. The question of universal non-relative truth that cannot become falsehood as situations change can only be put if we leave behind our situation and confront the whole. It is a question which seeks “the criterion of what is and what is not, what allows us to determine, by means of something constant and manifest, what there is” (HE 12). This desideratum is what leads to the birth of philosophy which thematizes the being of what-there-is in general.

The philosopher has left his situation behind, and the question is now how he, who encounters things no longer as components of a situation in which they are meaningful or meaningless, but as “what-there-is,” i.e., as far as their being is concerned, will answer the question of truth. The philosopher does not encounter “particular things,” but rather their “whole,” which Patočka terms “the world.” He confronts the fact that all things “in the world” – in this whole – somehow appear and disappear. He confronts their manifestation as such, as “what opens up against the black backdrop of closed night.”

The Greek state “builds an arena for an autonomous, purely human meaning, the meaning of mutual recognition in activity” which goes beyond “merely preserving bodily life” (HE 63). The community provides likewise a basis for possible rivalry of philosophical insights which thematize the problemativeness of Being and the meaning of existence in general, and seek to pierce “the darkness out of which shines a splintered enlightenment, yet unable to change night into day” (2002: 71a). This change is, of course, what both politics and philosophy want to bring about. We long to attach the relative meaning the situation has for us – this meaning which must be acquired, produced, or conquered again and again – to something constant, something above all situations, which would not leave us begging, should our situation lose its meaning. This tendency is nothing other than an attempt to find a source of absolute meaning.

This search leads to the discovery of *fysis*, divine nature in which all things emerge and decline, but which itself never passes away. The *kosmos*, the order that remains constant in this emergence and decline of things is then taken as a model for building the *polis*. The attempt to understand this order, to scrutinize it with clarity leads also to the birth of systematic mathematics and philosophical metaphysics. The possibility

of insight into unchanging truth in mathematics gives birth directly to three great metaphysical systems with Plato, Democritus, and Aristotle, each of whom claims to offer the final truth of the *kosmos*. “Thus philosophy in its metaphysical form does shed that mystery which was the origin of the shock which gave rise to it – but mystery catches up with it in the form of the mystery of the plurality of metaphysical concepts, fundamentally different perceptions of the nature of what there is as such” (HE 65–6).

All three philosophies have in common the explicit understanding that both the political effort to build a just society and the philosophical striving to shed light on the world as a whole can be accomplished only if we explicitly take care to preserve a rationally reflecting soul. “Hence the heritage of classic Greek philosophy is the care for the soul. Care for the soul means: truth is not given once and for all, nor is it a matter of mere intuition and acceptance in the consciousness; it is a life-long existential and mental praxis of self-checking and self-unifying enquiry” (2002: 87).

The plurality of philosophical concepts along with the historical circumstances of the Hellenistic period later led to skepticism; it became apparent that philosophy based on cosmic order “cannot provide human life with a wholly positive higher meaning” (HE 66). Christianity arose in this situation. Inheriting, in a universalized form, the previously mentioned tasks from the Roman Empire, Christianity’s solution was the belief in an absolute transcendent basis for life, to be found in a merciful, omniscient, omnipotent God. However, the Christian God is by no means self-evident. God comes to us, inasmuch as we *believe* in him. God is a new answer to an old question – an answer given only to those who wish to hear it, to those who have faith (2002: 345–6).

The Christian response was enormously successful. Reliance on God, combined with themes borrowed from ancient metaphysics gave birth to modern European civilization. Christianity begot a new, scientific rationality, which in turn gave rise to modern, mathematical natural science while at the same time encouraging the development of rational theology. “Modern man conceived new hope that the new, rational method would be able to prove once and for all, a priori, with mathematical certainty, the basic truths of the Christian faith, the *praeambula fidei*” (2006: vol. 1). The same hope gave meaning to the new science researching God’s creation. Mathematical science and rational theology were intimately linked in the project to achieve, with apodictic certainty, the knowledge of nature *qua* created by God. The theologians’ metaphysical endeavors reached a high point in rational demonstrations of the existence of God himself.

This rationalistic turn in the evolution of theology made it increasingly clear that the task it had set for itself involved rationally insoluble antinomies. By attempting to prove articles of faith with the same logical and mathematical cogency as was being achieved by scientific research, human reason was brought to perceive the hopelessness of its own strivings. Immanuel Kant eventually formulated this state of affairs in a rigorous and explicit way. The purpose of his “critical,” i.e., “discerning” philosophy was to free reason from tasks that exceed its abilities in order to allow it to devote its entire potential to scientific understanding in fields commensurate with its cognitive faculties.

The Kantian turn resulted in reason being severed into two halves: on the one hand, theoretical reason deals with knowledge of nature as governed by exact laws, a realm in which the question of meaning has no meaning; while on the other hand, practical

reason postulates meaning. The existence of God, the immortality of the soul and human freedom are things we can only *believe in*, things that cannot be *proved* in the mathematical sense of proof. Rational faith, the rationalism of the postulates of practical reason, was called upon to supply meaning to scientific research into nature.

Kant's system however was unsustainable. His postulates were soon presented by Dostoevsky as immoral, his supreme values unmasked by Nietzsche as empty. Their transcendent support collapsed – but the idea of the need for absolute meaning remained, and since it was nowhere to be found, people began to talk of nihilism: the world lacks a total, absolute meaning, hence nothing – *nihil* – has any meaning.

Positivism does not admit the question of nihilism and blindly concentrates on gathering knowledge; consequently, it amplifies prevalent nihilism. Husserl distanced himself from positivism. He conceived the crisis of society as a result of the crisis of science, to be solved by a new science, a new objectification, founding all knowledge on the clarity of objective evidence (2002: 392). Heidegger shows that this objectification is insufficient; the problem lies in a realm where objectification is not possible – the realm of our understanding of Being, which governs all objectification.

Patočka adopts Heidegger's diagnosis. In the modern technological age, man understands all that is, including himself, as a mere stock of manipulable raw material, an accumulation of power at his disposal at all times, continuously consumed in order to further accumulate available supplies. As Being manifests itself exclusively in this technologized form, man is prevented from perceiving the structures that make manifestation as such possible. Reduced to the level of a simple state of fact, humanity is prevented from accessing that which makes it human (2002: 388). Modern man does not see that he himself, through his freedom and understanding, transcends all that is, and that he is therefore a prerequisite for the manifestation of that which is a necessary condition for any clarity and, hence, any kind of meaning in the world. Modern humanity is not endangered by technology itself, machines, and technical inventions, but, rather, by the concealment of the ontological state of affairs brought about by the exclusivity of the technological understanding of the world.

Patočka formulates this problem as a contradiction, a conflict within Being itself which has now escalated to the utmost. Being, while making things appear, conceals itself and as a result “life negates itself in its being” (1996: 394). The task before us is therefore: “how to make this concealing manifest in its concealment” (1996: 411). It is obvious that the task so defined concerns not only the philosophy of history, but philosophy proper as well. Patočka's philosophy of history is an integral part of his specific philosophical position within the phenomenological tradition. He himself called this position “a-subjective phenomenology.”

Though he accepted Heidegger's diagnosis, Patočka did not consider Heidegger's expectation of the mercy of Being and search for hope in art radical enough solutions. The conflict within Being can only be solved through conflict. Admiration for the dissident stand of Solzhenitsyn and Sacharov in the Soviet Union brought Patočka to introduce the idea of sacrifice. The technological understanding of Being endangers our life, while keeping us prisoners of this understanding, since technology governs everything that satisfies our needs (1996: 391–2). The struggle against this hegemony must hence take the form of a fight “to the bitter end – which may come at any moment” (1996: 416).

Patočka introduces here an explicitly Christian religious theme. The élan towards the bitter end is at the core of the moral-religious concept of salvation. “In this radical emptying of life, which in the end loosens every bond to life, life celebrates its triumph, overcoming the inner conflict which constitutes the essence of humanity. . . . It is a conflict that sends us to battle and consumes us therein” (1996: 393). Patočka quoted Jesus’ last words from the Gospel of St. Matthew: “Why hast Thou forsaken me?” When a participant in a private seminar remarked that the whole conception seemed a bit mystical, Patočka answered: “There is nothing mystical about it, it is quite simple. The answer is in the question. What would have happened if Thou hadst not forsaken me? Nothing. Something can happen only if Thou hast forsaken me . . . If Thou hast forsaken me so that nothing remains for me to hold onto” (1996: 413). Only then can the “divine,” *manifestation itself*, become manifest as stronger and more powerful than all the rest. Whereas in technological understanding there is an utter levelling, here a hierarchy comes to the fore: a difference appears between what *is* and what, though *non-existent*, rules over all that which *is*. This difference is absolute.

At one point in his *Heretical Essays*, Patočka says that Christianity “remains the greatest, unsurpassed but also un-thought-through human endeavor which has enabled us to oppose decline” (HE 108). His proposal for solving the crisis of modern man seems to be something like “thinking through” of Christianity. This “thinking through” is however, as we have already seen, clearly aware of its un-Christianity. That is why Patočka terms it *heretical*. Primarily, it abandons the concept of God. There is no longer an absolute eternal outside support that prescribes rules of behavior, determines order, sets goals, and guarantees the possibility of salvation. The absolute is wholly contained in the finite, more precisely in human freedom by means of which we transcend the world and in which we encounter appearances. Nothing is left of Christianity except for the question – taken as an answer – of Jesus’ last words on the Cross. Jesus’ call remains, calling us to commit ourselves completely to rising above decline, achieving and maintaining understanding of the absolute difference between the existent and Being. This understanding, if it came to be on a world scale, would then only make it possible for mankind to seek and find relative, situational meaning, in a truly human, reasonable way.

Patočka thus offers a possibility. The possibility that history *will* have meaning, that humanity *will* succeed in bringing about a meaningful existence, depends, in his view, on a particular condition, formulated in various texts and various terms from the second half of the 1960s on. This condition is “the solidarity of the shaken; the solidarity of those who are capable of understanding what life and death are all about, and so what history is about” (HE 134).

Our current situation is similar to what it was at the beginning of history – a situation of problematcalness. Patočka recommends not to repeat metaphysical or religious attempts at finding a total positive meaning but, on the contrary, to endure and accept problematcalness as such. He invites us to venture out on the path of seeking, to live determinedly “in permanent questions and permanent uncertainty,” to accept “this fundamental ground, which is perhaps what makes us human, i.e., . . . awareness of problematcalness” (2002: 349). So history, far from ending with nihilism, will continue as long as it explicitly rests on the principle from which it arose – as long as

it does not seek to avoid problemativeness. For Patočka, to accept problemativeness means “to embrace history” (HE 118).

Patočka applied his philosophy of history in a remarkable way in his long essay on Czech history *What Are the Czechs?* The essay is a collection of private letters to his German lover. The form of the manuscript indicates clearly that it was meant for publication. The essay puzzled many historians as his *Heretical Essays* puzzled philosophers like Paul Ricoeur and Erazim Kohák.

Although the essay was apparently motivated by the facile capitulation of the Czechs to the Soviet invasion in 1968, it is consistent with his general philosophy of history: During the Middle Ages, Bohemia took upon itself the historic task of serving as the vanguard of the western Roman Empire in its expansion eastward under the banner of civilizational Christian universalism. “[Czech] kings knew they must either become emperors or die – and they died magnificently” (2006: vol. 2, 257). Only the Czech king Charles IV became Holy Roman emperor and made Prague the capital of a unified, spiritual Europe. “For a century and a half [thirteenth–fourteenth century], Bohemia and its dynasties served as the main basis for organizing Central and Eastern Europe” (2006: vol. 2, 257). The Hussite rebellion in the fifteenth century meant that Bohemia was the origin and center of a Reformation, a vanguard of Europe in attempting to solve the ripening problem of the transformation of western Christianity towards laicization.

The decline of the Czech state following the Thirty Years’ War lost Bohemia its nobility. After the war, it was separated from the leading new trends in Europe, and Czech “greatness” transformed into provincial pettiness. The Czech national “re-birth” in the nineteenth century was spent on a petty project of making the Czech language equal with German, in the absence of a political elite and nobility capable of heroic leadership. The result was T. G. Masaryk’s establishment of the sovereign democratic Czechoslovakia on the ruins of Austria after the First World War. The plebeian origins of the Czech national movement led to absence of national “inner power” and consequently to the collapse of the state in 1938 following Nazi and international pressure, without a fight (2006: vol. 2, 261). Even during the interwar democratic period, Bohemia was unable to unite its Czech, Slovak, German, and Jewish populations to resist the Nazi aggressor. The post-war disasters of the Czechs that are not analyzed in the essay, the Communist putsch in 1948 and the capitulation of the Prague Spring under the Soviet tanks were apparently only a logical continuation of this historical “falling.”

Aviezer Tucker (2000: 105) found a contradiction between Patočka’s “anti-democratic elitism” and his anti-nationalist democratic values. Tucker interpreted Patočka’s text as an attempt to understand the failure of the Czech nation by classifying it as petty (*kleines Volk*) as opposed to German greatness as a *grosses Volk* bent on expansion in territorial and historical space (*Raum*). However, this is the wrong issue. Patočka was not interested in whether his nation displayed a master or slavish “mentality.” This would be the nationalistic perspective. Instead, Patočka was interested in what role the nation plays in a given historical moment. The nation is “great” if it engages in a great universal task, surpassing its own individuality such as the universal project of the Holy Roman Empire. It is “petty” when it looks inwards into its individual

problems, ignoring “what history is about,” and does not take part in a risky attempt to solve ever anew the problem of historical meaning.

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Hermeneutics

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Originally, hermeneutics or the theory of interpretation was a religious discipline attempting to make sense of sacred texts and oracular pronouncements. The term *hermeneia*, thought to be related to the messenger god Hermes, stands for the art of mediating between heaven and earth, the divine and the human. Insofar as it makes speech intelligible, hermeneutics can also be related to philology or the study of human language as the vehicle for literature. In this way, hermeneutics could be extended to the interpretation of all linguistic products, especially those that stem from remote places and times. Thus, more generally, the theory of interpretation came to concern itself with the exegesis of unfamiliar historical texts to translate them into a more familiar vocabulary.

With the rise of Christianity as a world religion, textual interpretation came to serve the primarily doctrinal function of determining which parts of the Bible are canonical and how conflicting claims can be reconciled. Here the task was not merely to restore the original meaning of the Bible, but to assure its continued historical efficacy. Whereas the Catholic Church tended to defend the authority of institutional traditions in determining the meaning of scriptural passages, Protestantism attempted to set exegesis free from some of these constraints. Individuals must become capable of interpreting the Bible in light of their own faith. Greater emphasis began to be placed on the spirit that pervades scripture as a whole than on resolving literal conflicts. The Pietist manuals for interpreting the Bible were less about preserving a historical tradition than about drawing out the affections that separate the saved from the damned. Philological and historical methods remained in play to help define the context that authors such as the apostles had in common with their original public. But these methods could also be used to show that parts of the Bible were corrupted by historical accommodation. The more it was acknowledged that the divine inspiration of the Word is mediated by human beings in time, the more urgent it became to assess the legitimacy of its claims. Enlightenment readers of the Bible, like J. S. Semler and Kant, were able to assign it a new validity by extracting a permanent moral spirit from the transient letter of the text.

Friedrich Schleiermacher (1768–1834) transformed hermeneutics from a special discipline coping with specific obstacles facing understanding to a universal discipline about understanding as such. This allows hermeneutics to assume philosophical

significance. Interpretation is needed not merely for overcoming misunderstandings, but for the deepening of understanding and the improvement of communication. Interpretation has the task of better understanding and is claimed to involve “the historical and divinatory, objective and subjective reconstruction of given speech” (Schleiermacher 1977a: 93, 1977b: 111). This would seem to give historical interpretation a kind of primacy in Schleiermacher’s system. But he goes on to clarify that historical interpretation is mainly about language. The “objective-historical” approach considers “speech in relation to language as a whole and recognizes the knowledge contained in it as itself a product of language” (Schleiermacher 1977a: 94). Historical interpretation is really part of what Schleiermacher calls “grammatical interpretation” which deals with all aspects of language (not merely grammar). The historical is only part of the objective and has its counterpart in the “objective-divinatory,” which is defined as the capacity “to intimate how speech can stimulate further developments in language” (Schleiermacher 1977b: 112). The objective-historical considers language retrospectively as a product; the objective-divinatory considers language for its productive potential. Being a dialectician at heart, Schleiermacher also gives us “the subjective-historical” and “the subjective-divinatory.” The former locates speech as a fact in someone’s mental life and the latter considers its thought-content for its further influence. They involve what Schleiermacher calls “psychological interpretation,” whether in a historical backward-looking mode or in a future-directed divinatory mode.

The important distinction for Schleiermacher is between grammatical and psychological interpretations. Hermeneutics must balance these two approaches. Grammatical interpretation looks to shared language as the presupposition of communication. For it, “each person represents one locus where a given language takes shape in a particular way, and his speech can be understood only within the context of the totality of the language” (Schleiermacher 1977a: 78, 1977b: 98). Psychological interpretation, by contrast, examines how individual subjects creatively develop the language they inherit. This gives Schleiermacher two polar ways of estimating human speech. “It is maximally significant for the grammatical side when it is most productive (*produktiv*) and minimally repetitive: the classical. It is maximally significant for the psychological side when it is most distinctive (*eigentümlich*) and minimally commonplace: the original” (Schleiermacher 1977a: 83, 1977b: 102). Productivity is a classical ideal that stays within the bounds of common language to achieve communicative proficiency; originality moves beyond these bounds to achieve psychological profundity. The absolute ideal for Schleiermacher is reserved for the synthesis of the productive and the original and constitutes genius or the archetypal.

From this final Romantic standpoint of the genial, the historical serves merely to revive the classical. The task of understanding the distinctiveness of a text goes to psychological interpretation whose aim is to locate the text’s genetic origin in the mental life of its author. Schleiermacher refers to this genetic moment as the author’s “seminal decision (*Keimentschluß*)” (Schleiermacher 1977a: 189) which is then allowed to germinate and grow like a seed. The attempt of psychological interpretation to reconstruct the seminal decision conceives it as a free act that can only provide the “direction of the work” (1977a: 183) not its final outcome or goal. For the latter,

Schleiermacher appeals to technical interpretation, which is more calculative than a simple psychological interpretation.

Technical interpretation distinguishes between the meditative beginning or germination of a work and its compositional completion. It considers how the author's efforts at composition for the sake of communication, that is, his efforts to organize the thoughts involved in his meditation into an organized whole, may transform these meditative thoughts (1977a: 215). Technical interpretation can thus be considered a focused kind of psychological interpretation (1977a: 179). Whereas the latter conceives the creation of a work in relation to the implicit, even unconscious, richness of a life and its spontaneous transitions, technical interpretation starts with explicit thoughts and traces them methodically to their end. Technical interpretation also considers the creation of a work in relation to the historical resources of its genre.

The goal of both psychological and technical interpretation is to understand the individuating style of a work. Again, divination is appealed to, but this time it is balanced by the comparative method. Whereas divination involves the immediate identification of one self with another, comparison defines the other through the mediation of universal concepts. Without appealing to a universal human type, divination intuits a congeniality among particular human beings. It is based on the assumption that each human being, "in addition to being himself distinctive, possesses a receptivity to the distinctiveness of all others. This in turn seems to presuppose that each person contains a minimum of everyone else. Accordingly, divination is aroused by comparison with oneself" (1977a: 169–70). If the interpreters of a historical work are to understand it better than its author understood it, then they must find those congenial seeds in themselves that are comparable to the genial seed that constitutes the work's seminal decision.

August Wilhelm Boeckh (1785–1867) studied with Schleiermacher, but assigned more importance to historical interpretation than his teacher and less to psychological interpretation. The part of his *Enzyklopädie und Methodologie der philologischen Wissenschaften* devoted to hermeneutics distinguishes interpretation from understanding as the "consciousness of that through which the meaning and significance of the thing communicated are conditioned and defined" (Boeckh 1968: 49). Hermeneutics is critical in considering the conditions for understanding and as part of philology it involves a recognitional moment as the knowledge of what is already known (Boeckh 1968: 9).

Boeckh distinguishes four kinds of interpretation, the first two of which are based on the objective conditions of what is communicated and the second two on their subjective conditions. While (1) grammatical interpretation proceeds from the literal meaning of the understanding of objective texts, (2) historical interpretation proceeds "from the meaning of the words in reference to the material relations and context" of the text. (3) Individual interpretation begins with "the subject itself," and finally (4) generic interpretation deals with this subject "in reference to subjective relations which lie in the aim and direction of the work" (Boeckh 1968: 51).

Historical interpretation outranks grammatical interpretation because the meaning of a text is not exhausted by the words themselves, but lies in ideas referring to actual conditions. Boeckh claims that any

writer presupposes . . . that the people to whom he is addressing himself will not only understand his words grammatically, but will in connection with them think of more than they actually say, because their content stands in real connection with historically given circumstances and thus reminds every thinking person of these relations. (Boeckh 1968: 77)

Criticism complements hermeneutics by assessing the appropriateness and authenticity of works. Only through the cooperation of hermeneutics and criticism can historical truth be ascertained.

Wilhelm Dilthey (1833–1911) broadened hermeneutics to consider not merely literary works but all human objectifications, including historical deeds. Dilthey's 1860 essay, "Schleiermacher's Hermeneutical System in Relation to Earlier Protestant Hermeneutics" argues for a hermeneutics oriented to "judgment formation" that can relate concepts, not just to each other, but to the actual particulars of historical life (Dilthey 1996: 133). This is the first indication given by Dilthey that hermeneutics is not restricted to reconstructing authorial meaning, but can be made useful in relation to the project of historical understanding and the human sciences in general. However, between 1860 and 1900, he chose to develop historical understanding by focusing more on the experiential basis of the human sciences than on their interpretive framework. The guiding assumption in such works as the *Introduction to the Human Sciences* (1883) and *Ideas Concerning a Descriptive and Analytic Psychology* (1894) is that our lived experience provides an inner connectedness that also gives us access to the human-historical world.

Whereas the main task of the natural sciences is to find synthetic laws that explain the connectedness of elements that are initially represented as disconnected, the human sciences must analyze the indeterminate connectedness offered by lived experience into more determinate structures. In his psychology, Dilthey speaks of understanding as a process of the gradual articulation of the continuum of lived experience. Each time we respond to a situation, its significance will differ in light of the experience that we have acquired in the meantime. Lived experience is processed temporally, and this provides a tacit understanding of our larger historical involvements. Historical understanding is conceived as the explication of the implicit intelligibility of lived experience.

Dilthey saw his overall project as a critique of historical reason and its final formulation is found in *The Formation of the Historical World in the Human Sciences* (1910). It contains his most important hermeneutical essay, "The Understanding of Other Persons and Their Manifestations of Life." The declared task of the essay is to consider how the understanding of others contributes to historiographic understanding. It starts by establishing that the givens of history are always manifestations of life (*Lebensäußerungen*), but not necessarily expressive of the lived experience (*Erlebnisausdrücke*) of others. Dilthey writes,

By manifestations of life I mean not only . . . expressions that intend something or are meant to mean something but also those that without any such intent to express spirit nevertheless make it understandable. Understanding will differ in kind and scope in relation to different classes of manifestations of life. (Dilthey 2002: 226)

Dilthey distinguishes three classes of life-manifestations which we will initially characterize as theoretical, practical and disclosive. The first class consists of concepts, judgments and other pure thought-formations. If anything is expressed in these theoretical manifestations, it is not anything more than logical meaning. "Here understanding is directed at the mere logical content, which remains identical in every context, and is more complete than in relation to any other manifestation of life." These life-manifestations are expressions insofar as they are intended to communicate meaning contents, but they are not expressive of the otherness of those who put them forward. Thought-formations "have been detached from the lived experience in which they arose, and they possess the common basic trait of having been adapted to logical norms. This gives them a selfsameness independent of their position in the context of thought."

Actions constitute the second class of life-manifestations. They are not intended to communicate, but the way they relate to the practical ends of human life can nevertheless communicate something to others. Actions express only part of the inner life of the actor. The significance of actions tends to be one-sided because they are practical responses to particular situations. The third class of life-manifestations is

[T]he expression of lived experience. A special relation exists between it, the life from which it stems, and the understanding that it brings about. An expression of lived experience can contain more of the nexus of psychic life than any introspection can catch sight of. It draws from depths not illuminated by consciousness . . . [I]ts relation to the spiritual or human content expressed in it can only be made available to understanding within limits. (2002: 227)

These expressions will range from gestures and facial expressions to more considered expressions like works of art. A scribbled note or a random remark in a private journal can reveal someone's life-concerns in a way that is truly eye-opening. We may even be startled by our own spontaneous expressions, as they often disclose unexpected characteristics of ourselves. Expressions of lived experience are able to open up not only the otherness of the other, but also the otherness of the self. Self and other come to stand in a hermeneutical circle of interdependence.

Because expressions of lived experience draw on the full scope of psychic life, they offer the greatest challenge to understanding and the most room for misunderstanding. They are more difficult to interpret than the other two kinds of life-manifestations because they are not reigned in by the theoretical and practical interests of ordinary life. They tend to reflect the interestedness of life in less constrained ways.

Dilthey appropriated the concept of objective spirit from Hegel, but reconceived the universal speculative character it assumed in an idealistic philosophy of history in terms of a more empirical and verifiable commonality. Objective spirit embodies the shared meanings that we inherit from our own tradition. It establishes the context whereby the past becomes an "enduring present" (2002: 229).

[E]ven the work of a genius will reflect a common stock of ideas, attitudes, and ideals characteristic of an age and a region. From earliest childhood, the self is nurtured by this world of objective spirit. It is also the medium in which the understanding of other persons and their life-manifestations takes place. For everything in which spirit has objectified itself contains something that is common to the I and the Thou.

Objective spirit is the medium of commonalities that orients what Dilthey calls “elementary understanding.” This common but local medium provides the initial meanings that we grow up with. Elementary understanding requires no “conscious inference” or at most an “inference by analogy.” Dilthey’s important claim here is that a manifestation of life can “express something about human spirit” because it finds its place in the already “articulated order” of objective spirit.

This way of conceiving the expression of meaning allows us to reconsider the nature of understanding and interpretation. Dilthey still speaks of understanding as a process that proceeds from something externally presented to something inner. But this is not to be interpreted as going back to the psychological state of mind of the author. Instead, the primary sense of inner in the realm of meaning is the intrinsic connectedness of the parts that make up the overall expression. The relation of expression to what is expressed is an inner connection due to the expression’s inherent place in the articulated order of objective spirit. All that elementary understanding looks for is the common meaning of expressions. To be sure, interpretation rarely stops here, but the commonality of objective spirit provides the initial orientation for human understanding.

We move to higher understanding when the common or familiar meaning of an expression does not seem adequate. Higher understanding is not concerned merely with meaning, but raises the issue of truth. This happens when we confront ambiguities and inconsistencies in a text, or when we suspect the possibility of deception. Then we must compare the expression in question to similar expressions either in the same text or other texts, and, if necessary, in relation to other contexts.

For Dilthey, the human sciences are necessary to direct higher understanding to the appropriate universal disciplinary contexts, whether they be social or political, economic or cultural, secular or religious. Thus literary scholars can examine an ambiguous poetic passage and attempt to define its sense by interpreting it as a literary allusion to an earlier or contemporary work. Or they can perhaps clarify it by seeing it as a way of accommodating certain technical demands of the genre as such. These cases of higher understanding establish a larger context of reference and move from simple inference by analogy to inductive inference.

However, higher understanding can also focus on more specific contexts related to a work or its author. The consideration of such contexts should come only at the conclusion of the interpretive process and represents a shift from exploring the relation “of expression to what is expressed” toward the relation “of what has been produced to productivity” (2002: 233). Here we move from meaning relationships to something like a causal relationship. But when higher understanding goes back to the life-nexus of the author to gain further insight into the meaning of a work, it need not speculatively retrace some genetic moment like Schleiermacher’s seminal decision. Instead, Dilthey refers to the life-nexus of the author in a more inclusive way to articulate the unifying style of the work. In the following passage, Dilthey speaks of the individualizing unity of a life or of a work as a whole:

Understanding always has something individual as its object. In its higher forms it proceeds on the basis of an inductive gathering of what is given in a work or a life to infer the overall connectedness or unity of a work or person – a life-relationship. The analysis

of lived experience and of self-understanding has shown that the individual is an intrinsic value in the world of human spirit; indeed it is the only intrinsic value that we can establish indubitably. Thus we are concerned with the individual, not merely as an example of the human in general but as an individual whole. (p. 233)

However, the highest form of understanding is a process of re-creation or re-experiencing, which he contrasts with simple understanding: "Understanding as such is an operation running inverse to the course of production. But a fully sympathetic reliving requires that understanding go forward with the line of the events themselves" (2002: 235). Re-experiencing develops understanding by completing the hermeneutical circle. If understanding goes "back" to the overall context, re-experiencing goes "forward" by following out the parts that give focus to the whole. A re-experiencing is not a psychological re-construction but produces a better or critical understanding that refines the original.

Historians can provide a narrative re-experiencing of the past that draws on all three types of life-manifestation. To use Dilthey's own terminology, historians can formulate theoretical judgments to characterize past situations, focus on important actions, and appeal to expressions of lived experience to create a sense of what it would be like to have been there. To put it in more general terms, we can understand the past by means of (1) discursive representations, (2) the presentational remnants that past actions have left as their traces, and (3) expressions that disclose something distinctive about it. Discursive representations can describe history in universal terms; presentational remnants give access to history through particulars. Finally, objective expressions are important to help us individuate what has happened.

The move of hermeneutics away from psychological interpretation is even more explicit in the work of Martin Heidegger (1889–1976). In *Being and Time*, he writes that *Dasein*, literally being-there, which some may interpret as resembling human existence, "expresses itself not because it has . . . been encapsulated as something 'internal' over against something outside, but because as being-in-the-world, it is already 'outside' when it understands" (Heidegger 1962: 205). To be human is to be-there (*da-sein*) in the world. Humans can understand the world because they are already outside themselves and part of that world. Heidegger's ontological hermeneutics can be said to be a radicalization of Dilthey's claim that "the primary condition for the possibility of historical science is contained in the fact that I am myself a historical being" (Dilthey 2002: 298).

Understanding history is itself situated in a historical mode of being. For Heidegger, it involves a worldly projection that can be found in our moods. Moods (*Stimmungen*) are not just psychological states – they are existential modes of being attuned (*gestimmt*) to the world around us. As such, understanding is not a conceptual act but involves "a fore-conception (*Vorgriff*)" or a "fore-having (*Vorhabe*)" (Heidegger 1962: 191) that must then be articulated by interpretation. "The fore-structure of understanding" still needs to be laid-out (*ausgelegt*) for "the as-structure of interpretation (*Auslegung*)" to become apparent. But the conceptual articulation of the understanding of humans being in the world does not content itself with *actual* historical cognition. It projects something more fundamental, namely, the very *possibility* of historicity as the articulation of the temporality of being. This is not just any possibility, but one that has been handed-down (*überliefert*) by the tradition (*Überlieferung*) which we have been thrown-into. The

most controversial formulation of this approach to history (*Geschichte*) is the claim that it works out our destiny (*Geschick*) (2002: 435–6).

Whereas the conditions of historical interpretation uncovered by Dilthey are enabling as well as limiting, derived from a sense of human beings participating in history, Heidegger focuses on the existential conditions for partaking of history. Instead of developing a sense of active participation (*Teilnehmung*) that allows the human perspective to play a role in understanding history, Heidegger stresses a more passive partaking (*Teilnahme*) that leads to an ontological sense of historicity in which anything approximating humanism is explicitly rejected.

Hans-Georg Gadamer (1900–2002) expands Heidegger’s ontological hermeneutics to allow the human sciences a greater role in establishing historiographic truth. He continues to see hermeneutics as being in the service of tradition, but the outcome of interpretation is not pre-destined as for Heidegger. Gadamer’s hermeneutics posits a historically effected consciousness that continues to be effective by applying old meanings to new situations. The task of hermeneutics is not the reconstruction of some original meaning in Scheiermacher’s sense, nor a better or more critical understanding in Dilthey’s sense, but a different understanding that is relevant to the present.

Whereas Dilthey stressed the reflective, epistemic, and methodological contributions to be made by the human sciences, Gadamer focuses on their capacity to sensitize us, to develop our sensibilities. Here Gadamer returns to the classical rhetorical tradition and to the Helmholtzian ideal of *Bildung*, which is explicated as the “self-formation, education or cultivation” (Gadamer 1992: 9) of humanity.

Gadamer criticizes Dilthey for correlating understanding with inductive methods that are objectifying, though he does not consider that Dilthey restricts such methods to the level of higher understanding. Although Dilthey thinks that inductive methods must always be adapted to their subject-matter, he would not feel comfortable claiming, as Gadamer does, that induction in the human sciences must be “artistic-instinctive” and will always require “tact” (1992: 5). For Gadamer, the contributions of the human sciences to understanding history are less at the level of explicit reflection and judgment than at that of *phronesis* (an Aristotelean concept, usually translated as prudence but resembling Gadamerian tact) and prejudice. Prejudices are important hermeneutically as a way of accessing the historical traditions we are part of. Gadamer writes: “If we want to do justice to man’s finite, historical mode of being, it is necessary to fundamentally rehabilitate the concept of prejudice and acknowledge the fact that there are legitimate prejudices” (1992: 277). Prejudices that have withstood the test of time and are able to adapt to new circumstances acquire a normative authority. The foregrounding of prejudices in the works of others is important for Gadamer because it can provoke an awareness of our own prejudices that might otherwise have remained unnoticed. Playing out prejudices against each other makes possible a historical conversation for which the truth is not some judgmental outcome, but resides in the process itself. Our relation to history is an ongoing process whose task is not the attainment of some definitive “simultaneity” with the past, but an open-ended “contemporaneity.” Gadamer rejects retrospective efforts at simultaneity for a forward-looking contemporaneity in which the horizons of the past and present become fused. Whereas Dilthey’s hermeneutics could be said to appropriate the past by means of critical judgments that differentiate it from the present, Gadamer’s hermeneutics revives

a speculative approach to history. Gadamer conceives history as the playing out of a dialogue guided by the medium of language. In such a dialogue between different generations of a tradition, no individual subject is in control.

Karl-Otto Apel (1922–) partly shares Gadamer's view that hermeneutical understanding involves a "pre-reflective engagement" with tradition that is "normatively binding" (Apel 1989: 321). Apel is willing to define the hermeneutical approach to history as dialogic, communicative and intersubjective. But this is not sufficient to produce historiographic truth as Gadamer thinks.

In every human dialogue there comes some point where one of the participants no longer attempts to take the other hermeneutically seriously in his intentions, but rather attempts to distance him objectively as a quasi-natural event, a point where he no longer attempts to sustain the unity of language in communication, but rather attempts to evaluate what the other says as a symptom of an objective factual situation which he can explain from without in a language in which the partner does not participate. This partial suspension of hermeneutical communication in favor of objective cognitive methods is characteristic of the relationship of a physician to his patient, particularly that of a psychotherapist to a neurotic. (p. 338)

This second approach of partially suspended communication makes a kind of explanation possible, but it is not the law-like explanation that the logical positivists claimed could be found in the natural sciences. Apel establishes an intermediate sphere between the "scientistics" of the natural sciences, according to the logical-positivists, and the "hermeneutics of the cultural sciences" that could be called the sphere of the psycho-social sciences. The function of these psycho-social sciences is to point to factors that are operative behind the backs of the participants of history – factors that they could not be fully conscious of. Here explanations point to historical "interest-constellations" and are at least partly diagnostic. Historical subjects become patients to some extent, but for any psycho-social diagnosis to become effective it must be re-integrated into the pre-existing hermeneutic understanding.

In *Knowledge and Human Interests*, Jürgen Habermas (1929–) explicates a similar approach to human self-understanding, but reconceives Apel's diagnostic explanations as serving a general emancipatory interest. Rejecting the illusion of pure theory, Habermas relates all human inquiry to three general "knowledge-constitutive interests" that have a transcendental legitimacy (Habermas 1971: 22). Habermas distinguishes the *technical* interest of the empirical analytic sciences, the *practical* interest of the historiographic-hermeneutic sciences and the *emancipatory* interest of the critically oriented social sciences. The historiographic-hermeneutic sciences have an interest in "the preservation and expansion of the intersubjectivity of possible action-orienting mutual understanding" (1971: 310). Critical social sciences are concerned to move beyond the consensus that can be attained on the basis of applying the wisdom of tradition to the present. The social sciences, as such, look for invariant regularities of economic and political interaction, but they can only become critical if they proceed to reflect whether these invariances are necessary and desirable. Only when these sciences take over the philosophical task of "self-reflection" can historiographic understanding go over into historiographic enlightenment and release the human species "from dependence on hypostatized powers." Whereas historiographic understanding is rooted in a

linguistically based hermeneutic appropriation of the past, historiographic enlightenment can emancipate us from power relations that stand in the way of a better future.

Paul Ricoeur (1913–2005) uses structural linguistics to specify the multiple roles of hermeneutics. He delineates an opposition between two modes of hermeneutics: a restorative hermeneutics which develops interpretation as a kind of enriched understanding, and a demystificatory hermeneutics in which interpretation is a kind of explanation (Ricoeur 1970: 27). The primary exemplars of restorative hermeneutics are to be found in Dilthey, Heidegger and Gadamer; the best instances of demystificatory interpretations are the unmasking efforts discernible in Marx, Nietzsche, and Freud. Ricoeur also calls this kind of unmasking a hermeneutics of suspicion. He even proposes that the binary opposition of demystification and restoration drives interpretation as such and speaks of the “double motivation” of hermeneutics, “willingness to suspect, willingness to listen; vow of rigor, vow of obedience.” While this binary scheme for explaining what motivates hermeneutics tends to polarize hermeneutics, it does allow for hybrid forms. Thus Ricoeur notes paradoxically that “it may be that extreme iconoclasm belongs to the restoration of meaning” (Ricoeur 1991: 289).

However, by placing rigor and suspicion on one side, and listening and obedience on the other, Ricoeur may create the impression that true listening is unrigorous, and by implication, uncritical. Surely, it is possible to listen critically. Ricoeur also skews our understanding of rigor by conceiving it negatively as a mode of demystification. The solution is not to mix reverential restoration with a dash of iconoclasm, but to find a better model for hermeneutics. There is something negative about both restoration and demystification – one turns back the clock, the other strips away. Why cannot interpretation create new meaning?

Although the hermeneutics of suspicion in some respects mimics the emancipatory critique of Habermas, it is surprising that in the essay “Hermeneutics and the Critique of Ideology” of 1981, Ricoeur instead points to a gulf that “divides the hermeneutical project, which puts assumed tradition above judgment, from the critical project, which puts reflection above institutionalized constraint” (1991: 290). Whereas Habermas is willing to consider the critical project as at least partly hermeneutical – a kind of metahermeneutics aiming at depth interpretations – Ricoeur now excludes critique and judgment from hermeneutics. He thus seems to assent to Gadamer’s conception of hermeneutics as a mode of service to tradition, seemingly forgetting his own earlier position that hermeneutics is about both restoration and demystification. Hermeneutics is regarded as retrospective and critical theory as governed by future-directed regulative ideas. They can no longer be fused into one system according to Ricoeur: “each speaks from a different place” (1991: 294).

If critique is assumed to be dismissive of the past and as standing outside what is to be understood, then hermeneutics would be right to keep it at a distance. But there is no need to reduce critique to what Habermas called the emancipatory interests of the social sciences and their goal to overcome established power relations. A more encompassing sense of critique will leave room for such efforts but should be first of all orientational and reflective so that it can coordinate and evaluate the different places from which we speak. Critique in its most fundamental sense involves the capacity to establish limits and bounds. For a hermeneutics engaged with historical understanding, this

means being able to delimit what various disciplines can be expected to contribute. A critical hermeneutics will then be both interdisciplinary and self-disciplined, responsive to the richness and complexity of historical life and responsible in evaluating and judging its significance.

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Postmodernism

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“Postmodernism” is the archetypal chameleon concept of the later twentieth and early twenty-first centuries, variously perceived, in relation to the philosophy of historiography, as a potential liberator from modernist constraints and orthodoxies, and as an inducement to nihilistic cynicism that threatens to bring the discipline of history itself to an end. Its provocation of sometimes highly emotional reactions, typical of times of cognitive dissonance when new forms of thought are challenging traditional horizons, is indicative of its far-reaching implications; and its effects have already been considerable on both the practice and (especially) the philosophy of historiography.

“Philosophy of historiography” will be taken to include both the theorising of their own practices by historians themselves, and theoretical discussions of the subject by philosophers. Both strands date back to the origins of historiography in classical antiquity, and both have recently been profoundly affected by postmodernism.

More problematic is the term “postmodernism.” Here it will be taken to embrace attempts to theorize our condition in postmodernity – where “postmodernity” is used, relatively uncontroversially, to describe our own chronological position or era “after modernity.” “Postmodernism” is thus an ongoing process rather than a definitive position; but it includes most importantly those aspects of philosophy which have challenged, and maybe already superseded, the foundations of a “modernity” derived from the early-modern intellectual revolution and Enlightenment. So far as philosophy of historiography is concerned, postmodernism can conveniently be represented by such thinkers as Hayden White (despite his own disclaimers), Jean-François Lyotard, Michel Foucault, Jacques Derrida, and Frank Ankersmit; and it focuses on such related topics as narrative, centrality, language, epistemology, and truth.

Postmodernism’s Challenge

As traditionally conceived, historiography is simply a re-presentation of what actually happened – an account made as accurate as possible through the historian’s selfless and detached investigation of surviving evidential traces. From Thucydides in the fifth century BC, historians have modelled their procedures on those of the sciences, sharply

differentiating themselves from writers of poetry or fiction; and influentially reinforced by Leopold von Ranke in the nineteenth century, that conception of the subject has largely persisted to the present time. Historiography as thus understood has been widely valued as providing supposedly reliable foundations for personal and national identities and, where religious belief has been eroded, as supplying a secular replacement for the conveyance and confirmation of existential meaning.

The stakes, then, are very high, but it is the validity of claims to the truth and meaningfulness of such historiographic narratives that has been an early casualty in postmodernism's assault on traditional historiography. In a rare attempt at definition, it was by its incredulity towards grand or meta-narratives that, as one of its leading progenitors, Jean-François Lyotard characterized postmodernism. By that he implied in particular a repudiation of the progressive stories that had been so long central to philosophies of history (in the metaphysical sense). Where both philosophers and historians had made sense of the past by presupposing some direction – whether ordained by God or reason, or made possible through political reform or technological developments – postmodernism has questioned the validity of any such belief. That is not to say that historians have not continued to descry and describe purposeful movement through the past, but that the theoretical justifications for their doing so has been undermined.

Shots were fired across the bows of traditional narrative historiography in 1973: in his hugely influential *Metahistory*, Hayden White clarified how historians necessarily *chose* the narrative form in which to emplot their accounts of past events. Of themselves, White argued, events have no *inherent* meaning; meaning can be ascribed to them only within the value system of a certain context; and that context in turn takes its meaning likewise from another context; and so in an infinite regress. Furthermore, language and the rhetorical conventions of their time will necessarily circumscribe the modes of representation available to historians; so that any meaning-giving story, or narrative, will be, if not arbitrary, at best contingent – not *found* in the past but *imposed upon* it.

That narrative has, further, been imposed from a particular and necessarily limited and partial perspective. There can be no pretense about one over-arching story assumed to emanate from a single privileged center; there can be no more claims to occupy an external Archimedean point or independently existing foundation from which a narrator can seemingly narrate “from nowhere.” Whatever attempts may be made to objectify, depersonalize, and distance, or to avoid the appearance of personal intrusions, it is not possible to hope that *the truth* about history has been, or ever could be, attained and re-presented – or even to believe that that is a meaningful goal. The choice between contending narratives, which may be equally valid empirically, becomes then a matter of aesthetics and/or morality.

The consequent demise of “grand” universalizing narratives has been succeeded by a proliferation of stories inspired by the numerous and varied special-interest groups which have latterly attempted to reclaim their own place and shares in a past from which they had (to a greater or lesser extent) been previously (whether deliberately or unthinkingly) excluded. At this key point for the philosophy of historiography, postmodernism is revealed as an umbrella term that embraces as constituents a number of such smaller movements. These most importantly include post-colonialism, and feminism, which develops into a wider consideration of gender.

Post-colonialism, with its fundamental challenge to a long dominant Eurocentric historical tradition, itself has a long ancestry. From antiquity, criticisms have been voiced of narrow historiographies in which each state or other social group claimed the truth of its own particular and, in both senses of the word, partial version of events. Yet it long remained the case that, in the western world, historiographic perspective was for the most part derived from a center firmly embedded in Europe. It was from that fixed geographical point that the past was perceived, assessed, and evaluated; that the winners and losers, imperialists and colonists, were defined and described. And it was the comparatively sudden injection of a whole range of alternative perspectives that, under the rubric of "post-colonialism," contributed to the wider loss of belief in any one grand narrative.

An early and influential example of post-colonial critique was Edward Said's *Orientalism* (1978), in which it was argued that "the Orient" itself was a western construct, bearing only limited (if any) resemblance to what it purported to describe. The term "orientalism" had been used to embrace characteristics of a darkly mysterious "other" from which Europeans wished to distance themselves; so that it played an important role in constituting the identity of a supposedly superior west, which was then justified in its domination and manipulation of what it perceived and described as an inferior culture. Said's argument was clearly relevant in many ways to the philosophy of historiography, challenging the terminology, structure, and above all "objectivity" of previous narratives, and foregrounding such characteristic postmodernist themes as perspective, linguistic representation, bipolar categorization, ideological intrusion, and power.

Post-colonialism's (and so postmodernism's) most obvious implication, of a radical de-centering, has itself proliferated in ways of great practical, as well as theoretical, importance. First, previously unquestioned hegemonic narratives were found wanting, and in need of supplements from a potentially infinite number of alternative perceptions and partialities. Space had to be found for, or hearing given to, the voices of hitherto silent, or silenced, competitors; or at least some notice had to be taken of their existence, even in stories that remained predominantly European. Second, a whole range of alternative and equally cogent narratives could be offered in their own right, representing – and in this case re-presenting – those who had previously been ignored or excluded, or who had at best been seen and incorporated as "subaltern," inferior "others." Third, in a more recent and perhaps yet more positive development, some attempt could be (and is being) made to interweave narratives of groups previously considered as mutually exclusive and hostile, in ways that might encourage recognition of a shared past. The purpose of such newly illuminated commonalities is to lay foundations for a future that might (indeed, must) likewise be shared, but henceforth with equality and mutual respect; and it is notable that these attempts which, in modernist terms, would have been discounted as unacceptable ideological intrusions, have been made possible through postmodern attitudes to historiography and philosophy of historiography.

Postcolonial challenges have been paralleled by feminist critiques. Initially, these focused on women's comparative absence from the historiographic record, but increasingly sophisticated analyses have revealed more fundamental problems within the discipline of history itself, with its institutional structures and even linguistic

usages heavily biased towards the male and masculine. This is related again to the scientific pretensions of the subject: from early modernity, it has been claimed, emphasis in science itself, and hence also in aspirants to scientific status, has been placed on qualities and procedures – objectivity, detachment, rationality, logic – that are associated in western culture with maleness; and there has been a corresponding disparagement and marginalization, if not outright exclusion, of the female and feminine. Women's accounts of the past – women's historiographies – have often been accused of subjectivity, emotional involvement, and excessive attention to and reliance on “feeling” as opposed to “reason”; and so, failing to conform to the criteria specified in the dominant modernist model of the subject, they have been relegated to a class of writings inferior to “proper” historiography. It has been another major achievement of postmodernism that such historiographies have been retrieved for serious consideration: postmodern philosophies of historiography take due account of the qualities enshrined in them, and embrace their more “romantic,” subjective, emotional, and poetic approach.

It is, though, not only femininity but masculinity too that has come under critical scrutiny; so that the whole wider issue of gender has been opened up for study and debate. Anthropological studies have provided additional input concerning the cultural (as opposed to natural) determinants of gender discriminations; and these, as well as giving additional emphasis to the importance of cultural historiography, have fed in turn into the relativistic, anti-essentialist emphasis of postmodernism as applied not least to philosophies of historiography.

That emphasis penetrates to what lies at the very core of historiography: human nature itself. A belief in some unchanging essence of “human nature” that has, at least to some extent, been comprehended, underpins any attempt at historiographic understanding and explanation, as well as any optimism about historiography's predictive use. Some such continuity has to be presupposed to enable historians to make any sense of the past, by explaining past actions and events in terms of what they understand today. So that postmodernist questioning of the presupposition of an *essential* human nature has serious repercussions for philosophy of historiography.

Indeed relativism, not only in terms of human nature but more generally too, has proved to be one of postmodernism's most controversial aspects, largely because it is often perceived as leading to a slippery slope that terminates in foundationless nihilism – a situation where no beliefs or claims for understanding can be empirically justified. This is closely bound up with (if not sometimes indistinguishable from) a more acceptable epistemic skepticism; indeed it might well be claimed that, now as a major ingredient of postmodernism, skepticism continues to exercise considerable influence upon the philosophy of historiography.

Skepticism's main focus in this context is on questioning the basis on which claims to knowledge of history are made – or on raising doubts about whether historiography can ever be justified in claiming respectable *epistemological* status. Such doubts themselves have a very long history: it was the Renaissance recovery of classical texts that provoked not only natural philosophers but also historians to re-examine their disciplinary foundations – a process that culminated for the latter in a widely disseminated skepticism referred to (at the time) as “historical pyrrhonism” (a term derived from skepticism's Greek founder, Pyrrho). Just as in scientific studies of nature, so too in historiographic studies of the past, it was impossible to penetrate to any inner core

or essence: *appearance* was all that one could reach. Appearance (rather than “reality”) had to suffice in relation to any claims for truth; and extreme “historical pyrrhonists” professed themselves unable to distinguish historiography from fiction.

Henry St. John, Lord Bolingbroke’s recognition, in the eighteenth century, of that potential outcome for historiography has been frequently reiterated in relation to post-modernism, when once more the alleged impossibility of distinguishing historiography from fiction has been proclaimed by such theorists as Hayden White. As with Bolingbroke, so with White, any suggestion of historiography’s less than “factual” nature has been fiercely resisted, and a compromise was sought based on historiography’s claim to varying degrees of probability, to be applied according to the reliability of evidence. Some philosophers of both science and historiography have come to terms with the skeptical threat by simply making less presumptuous claims about attaining any final “truth.”

Skepticism within postmodernism, though, has been closely linked to further questions concerning language, and these have come to constitute another major challenge for philosophers of historiography. The fundamental problem is once again one noticed and discussed in classical antiquity – the problem of reference, or of the questionably tenuous connection between language and the external world it purports to describe. In this context, postmodernism has embraced the work of the earlier twentieth-century linguist, Ferdinand de Saussure, who saw language as a self-contained system of signs, internally consistent, but lacking any necessary reference to the external world. Indeed, as with skeptical philosophy, the whole concept of an external world – and this includes “the past” – becomes problematic, especially with regard to the extent to which it is, not so much an essence in and of itself, but rather an entity of our own construction; so that, when researching nature or the past, far from finding an independently existing construction outside ourselves, awaiting our investigations, we find ourselves responsible for its creation and structure – and so (as with Hayden White) responsible too for the imposition of any shape and meaning attributed to it.

Furthermore, even if for linguistic reasons alone, our descriptions are inevitably deficient and defective; for constrained as we are within what has been termed “the prison house of language” (by which is meant our own language, whatever that is), we are able to perceive, discriminate, and describe the world – and the past – only within the limited bounds of our own vocabularies and rhetorical structures. So we cannot do it all: we can never describe in full the totality of what is “out there” (or even in here); we can never ever do justice to whatever it is that we are attempting to capture within the crude linguistic net at our disposal; there will always, and inevitably, be a gap, a shadow of what remains unexpressed and inexpressible, between the word and the world.

This linguistic deficiency or indefinite postponement (this “*différance*” in Jacques Derrida’s terminology), or acceptance of an inevitable shortfall in our attempts to describe and represent, a recognition of which lies at the core of postmodernism, has particular relevance for philosophy of historiography, for historiographies are customarily in the form of literary (or at any rate word-based) texts. And if there are doubts about the adequacy of the individual components – the words – of such texts in attempted descriptions of the past, then far deeper shadows fall between that “past” (or whatever it is that has hitherto transpired) and any conceivable literary representation of it. Since it is those deeply shadowed (and deficient) texts that form the basis of, or primary sources for, subsequent historiographies, the problems are compounded

ad infinitum. Linguistics thus joins with more general skepticism to inhibit any claims regarding “historical truth,” or the possibility (or even meaningfulness) of attaining knowledge or “true” representations of the past.

Responses to the Postmodern Challenge

The various challenges to traditional philosophies of historiography, from narratologists, post-colonialists, feminists, linguists, and philosophers, are all a part of what we have defined here as “postmodernism”; and they have provoked a variety of responses from both philosophising historians and philosophers of historiography.

Historians themselves have tended to react defensively towards what they correctly see as a threat to their whole endeavor as previously defined – a threat liable to bring about the end of “historiography” as previously understood. So when unable any longer to ignore it, some have attempted to minimize postmodernism’s importance by suggesting that it holds neither anything new, nor anything that has not been, or cannot be, readily embraced and theorized within their own practices.

For that response they can claim some justification, citing examples of those who have previously provoked historians to rethink the foundations of their work. Lord Bolingbroke, as indicated above, is but one: clearly well aware of the implications for historiography of the extreme skepticism which had become newly fashionable by the early eighteenth century, his compromise solution, based on a commonsense acceptance of degrees of probability, has subsequently been widely adopted. Few philosophers of historiography now, in the face of postmodernist revivals of skepticism, would make presumptuous claims about definitive or final truths concerning the past; but even fewer would expect historians to give up their work entirely, rather than simply modifying the epistemological claims made about their conclusions.

For other traditionalists, though, postmodernism could and should not be so easily accommodated. Doubts concerning the attainability of any reliable “knowledge” of the past have been perceived as a threat to the whole historiographic enterprise as traditionally conceived; and the threat to “truth” itself as a meaningful object of aspiration has appeared as a particularly corrosive aspect of a postmodernism which endangers not only historiography but the very foundations of western civilization. Unsurprisingly, therefore, vigorous attacks have been mounted against postmodernism as preludes to determined defences of traditional positions. And equally unsurprisingly, attempts to shore up modernist concepts of truth and knowledge have relied on the scientific methods on which modernism itself is based; so that discussants have tended sometimes to talk across rather than to each other, in debates characterized by mutual incomprehension.

But there have also been more positive responses, from both historians and theorists, to postmodernist challenges. Even Richard Evans, for example, as a self-professed defender of historiography against postmodernist threats, concedes some beneficial effects of a theoretical position which has “opened up possibilities of self-renewal” for the discipline, and has “both extended the range of historical writing and breathed new life into some old and rather tired subjects” (1997: 243). More generally, an increased acceptance of the need to take some account of “theory,” together with an enhanced

self-consciousness, has been widely credited to postmodernism, even by practising historians otherwise hostile.

Within historiography, too, there has been increasing acceptance of alternative modes of presentation. Monologic narratives “from nowhere” – those single voiced statements from a seemingly privileged location that guarantees the authoritative status of supposedly “objective” and truthful representations – have been supplemented by more explicitly subjective accounts, in which authors have openly confessed their own positions and prejudices. Dialogue and poetic forms of historiography have been accepted as better suited to historiographies that recognize the force of competing allegiances, including the claims of feeling and emotion; and personal autobiographical accounts have been successfully interwoven with more public national historiographies. Furthermore, increasing recognition of, and frustration with, the deficiencies of linguistic representation has prompted exploration of non-literary forms, film in particular. While often still used to present simple narratives, film has been additionally promoted (by historians such as Robert Rosenstone) as being potentially better able than mere verbal description to represent simultaneously experienced ambiguities and complexities. It has been used, in the typically postmodernist mode of collage, to blur conventional spatial and temporal categorizations, eroding not least the previously well-policed boundaries between past, present, and future.

While practicing historians have given their varied responses, philosophers of historiography too have shown ambivalence towards postmodernism; and concerned and spirited theoretical defences of traditional models of historiography have continued to appear into the twenty-first century from, for example, M. C. Lemon (2003), C. Behan McCullagh, (2004), and Aviezer Tucker (2004). While each of these has his own take on the subject, we may note some common features in their responses to postmodernism. Most important is a continuing assumption that the acquisition of historiographic *knowledge* is still possible, so that a concept of *truth* survives towards which it is the function and duty of historians to strive. The means to attain such truthful knowledge is essentially *scientific*, empirical and rational or logical; and it can, it is argued, readily be validated by appeals to *common sense* and professional *consensus*. The traditional model of historiography thus remains intact, with its quest for truth “for its own sake” persisting as a sustainable moral imperative as well as academic goal.

Such apologies for traditional modernist historiography rely on optimistic appraisals of historians’ ability to apply their “rationality” in detecting unambiguous meanings, comprehending authorial intentions, detecting and where necessary correcting “bias,” evaluating “significance,” and so arriving at consensual truth. Essentially pragmatic and empirical, conservative philosophers of historiography continue in their well meant attempts to insert theoretical underpinning for a subject seen by them as central to the stability of western civilization, but seen too to be seriously challenged by the theoretical critiques of postmodernism.

Continuing Crisis of Incompatibilities

It is clear, then, that modernist and postmodernist theoreticians (whether philosophers or historians) are proceeding along altogether different tracks – tracks that may

periodically overlap, but are so unlikely to finally converge that respective proponents tend to engage in parallel monologues rather than shared conversation. Some basic incompatibilities between their respective positions were highlighted by Keith Jenkins in 1995. As the most influential Anglophone advocate of postmodernism in relation to historiography, Jenkins proposed a radical, and in his view long overdue, change in the focus of student courses in historiography – a change from the traditional texts of E. H. Carr and Geoffrey Elton to those of the postmodernist orientated Richard Rorty and Hayden White. Without wishing to revert to simplistic binary oppositions, a fundamental difference could be seen to lie between, on the one hand, a continuing commitment to the search for “objective” historiographic truth, defined as a correspondence with the actuality of the past, and, on the other hand, an acceptance of the need for self-conscious self-positioning in relation to the irreducible plurality of a past that admitted of innumerable perspectives and linguistic treatments. The former was perceived as certainist (still aspiring to, if not actually claiming, certain knowledge of the past), constricting, and confined to a professional élite, the latter as more open, admittedly relativist, and potentially emancipatory.

Jenkins claimed that Carr had, for personal and professional reasons, retreated from the skeptical relativism to which his own arguments were seemingly leading, in order to reaffirm such traditional values as a “common sense” that permitted of “objectivity” and continuing belief in a progressive historical trajectory. While paying some lip-service to the more radical theories of, for example, W. Dilthey and R. G. Collingwood, Carr was careful ultimately to maintain a model of the (essential) “proper” historiography that validated his own position (as a Marxist); and that model, as still embracing objectivity and truth, has continued to provide a theoretical base to which historians can comfortably subscribe.

It is hardly surprising that Carr, as an historian theorising in 1961, should have failed to come to terms with ideas that were to be later embraced within postmodernism; nor yet perhaps that Geoffrey Elton, as a pillar of the British historical establishment (albeit born Ehrenberg and raised in Prague), should have attempted to repel boarders who, by the 1990s, could be seen to be more obviously challenging the foundations of his work. But it is notable that Elton, for whom philosophy of historiography was at best peripheral to his practical concerns, did feel the need to defend his subject by a proposal (as his title indicates) to *Return to Essentials*. The essentials on which he insisted were typically modernist, presupposing a professional historian resorting, in a responsibly balanced fashion, to archival sources from which could be derived an objective historiographic account – a true representation of the past, articulated purely for its own sake. This reassertion of an essentially empirical position (his own) was assumed by him to be the only possibility; key concepts such as “representation,” “narrative,” and “truth,” all by now openly contested, remained unproblematized; and *ad hominem* attacks on postmodern theorists took the place of any philosophical argument. Yet Elton’s reactionary principles were thought by many still to merit universal assent.

As dissenters nonetheless, Jenkins introduced Richard Rorty and Hayden White. The former, as a philosopher in the pragmatist tradition, repudiates any belief in a Platonic-style “ideal,” external to and transcending what is normally accounted “reality,” by reference to which absolute standards of truth and (especially in the case of historiography) representation may be adduced. With the foundations of any such claims

(including the claims of historians) removed, thought is best promoted and provoked by discussion or conversation; dogmatism is to be replaced by an openness to alternatives; and a self-consciously ironic stance is to be adopted in relation to the contingency of one's own position. And although not specific to the philosophy of historiography, this approach clearly has important ramifications for the subject – as is clarified in Jenkins' introduction to the work of Hayden White.

For a key feature of White's philosophy of historiography is denial of any necessary connection between an historiographic narrative and the past it purports to represent. Though "real" enough, the past itself lacks narrative form; order is not inherent in it, awaiting discovery, but is imposed upon it by historians; and in the construction of their narratives, historians are constrained by the linguistic and rhetorical usages of their own culture, "emplotting" the past in accordance with their own ideological positions.

Importantly in the cases of Rorty and White, the question arises of what historiography, revealed now as a free-standing construction (rather than, as Carr and Elton would have claimed, a *re*-construction of an already independently existing past) might be *for*? Jenkins indicates that White's approach links with his (utopian) political views. Believing that traditional historiography imposes (arbitrary) closures and aesthetic order on an essentially chaotic past, White would prefer that past to be left more "open," in order to encourage similar openness in relation to the future – enabling the possibility of improvement towards which individuals might aspire.

Rorty and White's injection of linguistic, aesthetic, and ideological considerations into the philosophy of historiography mark a clear break from such earlier theorists as Carr and Elton. Indeed it implies the demise of "modernist" historiography, as long understood; but, as holding out the prospect of furthering "social hope," it provides a positive alternative model which, as Jenkins argued and is still the case, warrants serious consideration by students of historiography.

Conclusion

Postmodernism has thus undoubtedly widened the subject-matter of philosophy of historiography, so that it is no longer concerned solely with those aspects on which the Anglo-American analytic tradition has long concentrated: namely, and in particular, epistemology, involving such well-worn themes as truth, objectivity, evidence, causation, and explanation. Rather, it has redirected attention to matters previously associated more with the continental tradition – implying (as clearly shown in the work of Frank Ankersmit) a renewed problematization of narrative and representation, a receptivity to poetry and the poetic together with alternative modes of expression, a reconsideration of the sublime, or those things that lie by definition just beyond our reach, and an understanding that "historiography" itself has no natural "essence" but is a subject ever open to redefinition. Postmodernism's less circumscribed and more interdisciplinary approach has further (provoked by scholars such as David Harlan) entailed a reappraisal of historiography's relationship with ethics, and a reinsertion of moral discussion into a practice and discourse from which it has long been actively discouraged.

That renewal of an ethical dimension within philosophy of historiography has had additional implications for historians' own self-consciousness – encouraging a

corresponding recognition of their own moral (and more generally ideological) standpoints. In practical terms, that awareness has induced a greater humility in claiming “truth” or definitive closures, and a greater openness to a dialogic (rather than dogmatic) relationship to others. Although sometimes denigrated as inducing a nihilism that erodes any status for historiography, postmodernism has, then, overall had potentially beneficial effects by leaving its philosophical foundations shaking (as Jan Patočka puts it), and under constant revision and renewal – an ongoing process that provides hope and opportunity for further self-consciously adopted transformations and improvements for the future.

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Philosophy of History at the End of the Cold War

KRISHAN KUMAR

The Recovery of the Philosophy of History

Writing in the last years of the Second World War, the German philosopher Theodor Adorno wrote: "Had Hegel's philosophy of history embraced this age, Hitler's robot-bombs would have found their place beside the early death of Alexander and similar images, as one of the selected empirical facts by which the state of the world-spirit manifests itself directly in symbols . . . 'I have seen the world spirit', not on horseback but on wings and without a head, and that refutes, at the same stroke, Hegel's philosophy of history" (Adorno 1974: 55). Later, after the war, Adorno, true to his Marxist leanings, struggled to retrieve some version of the philosophy of history, in the form of a "negative universal history". But he remained committed to the notion of the "permanence of catastrophe." The human story, however much it might record the fitful march of progress, nevertheless seemed to carry the sign of universal and constant suffering. "After the catastrophes that have happened, and in view of the catastrophes to come, it would be cynical to say that a plan for a better world is manifested in history and unites it" (Adorno 1973: 320). Here he echoed the famous remarks of Walter Benjamin, in the ninth of his "Theses on the Philosophy of History," that history is "one single catastrophe," "one pile of debris," that the angel of history struggles in vain to repair and make whole (Benjamin 1973: 259). Both Benjamin and Adorno wished to hold on to the hope of progress that marked the materialist conception of history; both however seemed intent on undermining any comfortable belief that the historical record provided much evidence for such hope. The spirit, perhaps, was that of Kafka's: "there is hope; but not for us."

A belief in progress is not necessary for a philosophy of history. Oswald Spengler's *The Decline of the West* (1918–22), proclaimed to a war-devastated Europe its Nietzschean message of the inevitable downfall of their western civilization. But the philosophy of history, whether expressed in spiritualist or materialist terms, seems incapable of an absolute abandonment of hope. Even Spengler thought that by enlightening his fellow countrymen, as well as westerners generally, he was pointing the way to a future recovery. His book, in other words, was a rallying call, not a requiem.

The twentieth century managed to wring hope out of catastrophe – sometimes hope because of catastrophe. This was displayed in the many philosophies of history that,

despite the mockery and assaults of the professional historians, continued to appear in abundance in the first half of the century:

[T]he twelve volumes of Arnold Toynbee, the two of Oswald Spengler, the four of Pitirim Sorokin, Eric Voegelin's hexology, the repetitive corpus of Nicholas Berdyaev and of Reinhold Niebuhr, even the more modest works of Father d'Arcy, Christopher Dawson, Alfred Weber, and Karl Jaspers, not to speak of the English theorists of evolution and of Teilhard de Chardin, of the latter-day representatives of the idea of material and scientific progress, and of a variety of contemporary Marxists. (Manuel 1965: 136)

This serves to remind us that the idea of progress did not, as is frequently claimed, die in the mud of Flanders during the First World War. While certain, mainly literary intellectuals, certainly recoiled at the horror, others – scientists and social scientists especially – continued to find reason for hope. Marxism – in the writings of scientists such as J. B. S. Haldane and J. D. Bernal as much as in those of historians and social scientists – continued to be the main stimulus to the hopeful philosophies of history in the first half of the twentieth century. The success of the Bolshevik Revolution, and the improbable survival of the Soviet Union in the hostile environment of a capitalist world, heartened those who, whatever their disquiet about actual developments within the Soviet Union itself, thought that capitalism was doomed to self-destruction. For intellectuals such as Sidney and Beatrice Webb, Arthur Koestler, André Gide, André Malraux, Ignazio Silone, and a host of other western thinkers and writers, the Soviet Union announced mankind's future.

All the more consequential, then, was the impact of the growing disillusionment with the Soviet Union from the mid-century on. An important marker were the recantations, almost in the form of a confession of sin, by a group of prominent former communist and socialist intellectuals in a post-war volume entitled *The God that Failed* (1950). Arthur Koestler had already, in his brilliant anti-communist novel, *Darkness at Noon* (1940), announced his defection from the old cause. Even more powerful was the prophetic novel of his friend, George Orwell, whose *Nineteen Eighty-four* (1949) was widely (though mistakenly) read as a denunciation of Soviet Communism. Later, in the post-Stalinist "thaw" in the Soviet Union itself, came the revelations of the grim reality of Stalinist Communism and, by implication, all that had gone wrong with the Soviet Union. In place of glowing images of the proletarian hero of the future came harrowing stories of the *gulag*. The novels and other writings of Aleksandr Solzhenitsyn in particular – *First Circle* (1968), *Cancer Ward* (1968), *The Gulag Archipelago* (1973–5) – undermined for most people any lingering faith there might have been in the Soviet Union as the civilization of the future.

The appeal of communism to western intellectuals faded rapidly in the post-war era. For those who kept something of their socialist faith, the future lay with the ameliorism of "social engineering" and the welfare state rather than the revolutionary transformation of society and its culmination in the socialist utopia. The ideology of managerialism, and a general distrust of large-scale schemes and visions, dominated the thinking of many post-war intellectuals. For many, Karl Popper's *The Open Society and Its Enemies* (1961), with its attack on all forms of "historicism" and utopianism as harbouring totalitarian tendencies, was the Bible.

The period of the Cold War was therefore extremely inhospitable to large-scale historical and philosophical speculations such as had provided the material for the earlier philosophies of history. To the still-continuing denunciations of the professional historians and philosophers was now joined a climate of caution and moderation. The Cold War – and the nuclear “balance of terror” that sustained it – might give rise to apocalyptic visions of nuclear war and post-nuclear civilization, but it seemed to dampen thoughts about the future of mankind as seen in long-term historical perspective. There was indeed a movement of “futurology” and some announcements of “the coming of post-industrial society” (e.g., Daniel Bell) that essayed large-scale historical speculations (Kumar 1978). But, apart from the fact that works of this kind explicitly declared that they were not philosophies of history, that they were merely exercises in short- to medium-range extrapolations, they were generally too narrowly focused on western societies to present the encompassing vision of humankind’s past, present and future that was the hallmark of the classic philosophies of history.

The end of the Cold War, in the anti-communist revolutions of 1989 in central and eastern Europe and the subsequent dissolution of the Soviet Union itself in 1991, broke this impasse. These were events of such magnitude that they seemed to cry out for some sort of sustained reflection, some sort of placing within the larger scheme of things. It cannot have hurt, either, that they occurred – at least in the western calendar – at the end not just of a century but of a millennium. The imminence of the end of the millennium produced a whole host of books aiming to sum up the meaning of the past thousand years (see, e.g., Fenández-Armesto 1995). “Endism” – the sense of an ending and the prospect of a new beginning – was rampant (Kumar 2000, 2001a). It was inevitable that the fall of communism and the end of the Cold War would be seen as carrying millennial, or at the least, epochal, significance. As Eric Hobsbawm wrote,

the end of the Cold War proved to be not the end of an international conflict, but the end of an era; not only for the east, but for the entire world. There are historic moments which may be recognized, even by contemporaries, as marking the end of an age. The years around 1990 clearly were such a secular turning point. (Hobsbawm 1995: 256)

It is within this context that we should understand the significance and the impact of the writings of Francis Fukuyama and Samuel Huntington. They chimed with the mood of great hopes, but also of great fears. Neither Fukuyama nor Huntington in truth offered much comfort, though the former in particular was thought to do so. But they did renew a tradition of thought that had fallen into disrepute. They did attempt to place their times in some historical pattern, to read their significance within the wider framework of western and even world history. They did, to that extent, renew the philosophy of history.

The End of History: Hegel *Redivivus*

In the summer of 1989 there appeared an article in a Washington-based journal, *The National Interest*. It was entitled “The End of History?,” and its author was the deputy director of policy planning at the U. S. State Department, Francis Fukuyama.

The article, written in a high-profile journal much read by policy-makers caused quite a stir not just among policy-makers and intellectuals in America but in many other countries as well. It was allegedly debated by Margaret Thatcher and Mikhail Gorbachev and their advisers, among other prominent figures. Buoyed by the success of this foray, Fukuyama abandoned his State Department post and devoted himself to a full-scale book treatment of his theme. This duly appeared – with the question mark removed – in 1992 as *The End of History and the Last Man*.

Fukuyama was not just a middle-ranking State Department official. The appearance of an amateur intervening in high-level intellectual debates was deceptive. Fukuyama was a classicist and political scientist who had studied under Roland Barthes and Jacques Derrida in Paris and had gone on to specialize in Middle Eastern and Soviet politics. More relevantly he was a Hegelian, who had been introduced to Hegel, and more particularly Alexandre Kojève's persuasive interpretation of Hegel, by his Chicago teacher, the influential philosopher Allan Bloom. Fukuyama applied Hegel's philosophy of history, as understood by Kojève, to his own times (Fukuyama 1992: 65–7; Niethammer 1994: 62–8).

When Fukuyama first wrote his article, the 1989 revolutions in central and eastern Europe were only just getting under way, but change in the region was manifest. *Glasnost* ("openness") and *perestroika* ("restructuring") were in full swing in Gorbachev's Soviet Union. Encouraged – or rather, not discouraged – by Gorbachev, the east Europeans wound up communism in their societies by the end of 1989. Two years later the Soviet Union itself followed suit. By the time Fukuyama's book appeared the 75-year communist experiment in eastern Europe was over. Fukuyama felt justified in removing the question mark that hung over his original article. Many others too felt that the course of events had fully justified his prognostication.

For the substance of Fukuyama's argument was that the defeat and withdrawal of communism as a force in the world represented the "end of history." For those who did not read his writings but knew only their titles, this statement appeared preposterous, and there was a good deal of uninformed denunciation of Fukuyama. A mere glance at his work would have shown that he was using the expression in the specifically Hegelian sense. The history that had ended was a history understood as the conflict of ideologies. Hegel had thought that such a history had ended with the French Revolution, and the victory of the principles of the liberal democratic state. That view, argued Fukuyama, had been premature. Liberal democracy had had to face several challenges to its dominance in the two centuries following the announcement of its principles. There had been various forms of authoritarianism, militarism, and racism. Most critically there had been communism and fascism, two ideologies that had been genuinely modern and that had constituted real alternatives to liberal democracy. Fascism had been defeated and now, in the closing years of the twentieth century, communism too had shown that it was incapable of competing with liberal democracy. Hegel was finally vindicated. With the simultaneous collapse of right-wing and militaristic dictatorships throughout the world in the 1970s and 1980s, the contest of ideologies, the substance of history as Hegel understood it, was over. Western-style liberal democracy had triumphed and history was now at an end.

To critics who complained at the absurdity of announcing that history had ended, Fukuyama explained:

History, for Hegel, can be understood in the narrower sense of the history of ideology, or the history of thought about first principles, including those governing political and social organization. The end of history then means not the end of worldly events but the end of the evolution of human thought about such first principles . . . When Hegel declared that history had ended after the battle of Jena in 1806 [Napoleon's crushing defeat of the Prussian monarchy], he was obviously not making the claim that the liberal state was victorious throughout the world . . . What he was saying was that the principles of liberty and equality underlying the modern liberal state had been discovered and implemented in the most advanced countries, and that there were no alternative principles or forms of social and political organization that were superior to liberalism. (Fukuyama 1992: 64)

Nothing can have been plainer than that. Hegel had been right all along. The principles of 1806 – essentially the principles of the French Revolution- had been realized, even if it had taken longer than Hegel had anticipated. The revolutions of 1989 and the collapse of the Soviet Union in 1991 represented the final fulfillment of the promise of 1806 (or, put another way, the victory of the French Revolution over the Russian Revolution). This did not mean that history as a sequence of *events* would or had come to an end. That would indeed be an absurdity, if not an impossibility. Nor did the end of history promise a peaceful and harmonious future. There could be and probably would be ethnic, racial and national conflicts. Poverty and inequality would probably continue, together with conflicts over efforts to eradicate them. Even greater conflicts might be in store in confronting the gathering ecological crisis. There might even be full-scale war, including nuclear war. The point however was that none of this would involve *ideological* conflict. “All of the really big questions had been settled” (Fukuyama 1992: xii). What the west had achieved over many centuries of struggle – the establishment of liberal democracy and free markets – was now the goal of practically every society in the world. In a remarkable confirmation of this view coming from an unexpected quarter, the British Marxist left, Perry Anderson wrote:

Ideologically, the novelty of the present situation stands out in historical view. . . . For the first time since the Reformation, there are no longer any significant oppositions – that is, systematic rival outlooks – within the thought-world of the west; and scarcely any on a world scale either, if we discount religious doctrines as largely inoperative archaisms, as the experiences of Poland or Iran indicate we may. Whatever limitations persist to its practice, neo-liberalism as a set of principles rules undivided across the globe: the most successful ideology in world history. (Anderson 2000: 17; cf. Anderson 1992: 351–2, 357–8)

A note of disquiet about the future emerged more strongly in Fukuyama's book than it had in the 1989 article, where the note of western triumphalism sounded more clearly, to the distaste of many, especially on the left (Pieterse 1993). In the more somber and considered appraisal of the book, the prophet is Nietzsche rather than Hegel. Fukuyama here draws on Nietzsche's portrait of the “last man,” the man of modern democratic times who settles for comfort and a kind of animal-like contentment and is unwilling to take risks or encounter dangers in pursuit of higher goals of creativity and spirituality. Even in the 1989 article, the account ended on a wistful note.

The end of history will be a very sad time. The struggle for recognition, the willingness to risk one's life for a purely abstract goal, the worldwide ideological struggle that called forth daring, courage, imagination, and idealism, will be replaced by economic calculation, the endless solving of technical problems, environmental concerns, and the satisfaction of sophisticated consumer demands. In the post-historical period there will be neither art nor philosophy, just the perpetual caretaking of the museum of human history. (Fukuyama 1989: 18)

Dangerously flirting with a theme that threatens to undermine his whole philosophy of history, Fukuyama speculates, "perhaps this very prospect of centuries of boredom at the end of history will serve to get history started once again" (Fukuyama 1989: 18).

Whether or not such a future would mark a return of history in the old sense is not clear. It is certain however that the end of history *à la* Fukuyama will not necessarily bring the full realization of human potential that Hegel and Marx looked forward to: a world where religion and philosophy, art and culture, become the preoccupations of humans freed from the realm of necessity and material concerns. Fukuyama's announcement of the end of history is tinged with melancholy. This is the clearest difference between him and his nineteenth-century predecessors.

Shorn of the more optimistic hopes of the nineteenth-century philosophy of history, Fukuyama has offered nevertheless a self-conscious renewal of that tradition. "By raising once again the question of whether there is such a thing as a Universal History of mankind, I am resuming a discussion that was begun in the early nineteenth century, but more or less abandoned in our time because of the enormity of events that mankind has experienced since then" (Fukuyama 1992: xiv, 55–70). History, Fukuyama is convinced, does have a direction. Modern times are driven by the immense and in principle irreversible achievements of modern science. This force is the material basis of the "universal homogeneous state" that has emerged worldwide, and that has brought in its train the imperious demand for equality and democracy. It has become clear to everyone that the only viable form of society in our era is that of liberal capitalist democracy. Thus "it makes sense for us once again to speak of a coherent and directional History of mankind that will eventually lead the greater part of humanity to liberal democracy" (Fukuyama 1992: xii).

The Clash of Civilizations: The Revenge of the Past?

For Fukuyama, the two possible rival ideologies to liberal democracy were nationalism and religion. Both showed signs of persistence in the contemporary world; both had been in the past the source of great conflicts.

Nationalism, argued Fukuyama, was not really in contradiction to liberalism. It expressed the frustration of people who had been denied freedom and autonomy – in other words, the rights that liberalism itself proclaimed. Therefore nationalism "may constitute a source of conflict for liberal societies, this conflict does not arise from liberalism itself so much as from the fact that the liberalism in question is incomplete" (Fukuyama 1989: 15). The nationalism that was evident in post-Soviet eastern

Europe, Central Asia, and other parts of the world, was a transitional phenomenon. It accompanied democratization. Even though it might occasionally take illiberal forms, once the change had been accomplished nationalism would lose its power, as it had largely done in western Europe and other parts of the developed world (Fukuyama 1992: 266–75).

The religious challenge, especially in its fundamentalist form, was a more serious matter. The most important seemed to be the Islamic revival. It is true, Fukuyama conceded, “that Islam constitutes a systematic and coherent ideology, just like liberalism and communism, with its own code of morality and doctrine of political and social justice.” Moreover, “the appeal of Islam is potentially universal, reaching out to all men as men, and not just to members of a particular ethnic or national group. And Islam has indeed defeated liberal democracy in many parts of the world, posing a grave threat to liberal practices even in countries where it has not achieved political power directly” (Fukuyama 1992: 45).

Nevertheless, argued Fukuyama, in the end Islam was no real competitor to liberal democracy. “Despite the power demonstrated by Islam in its current revival, it remains the case that this religion has virtually no appeal outside those areas that were culturally Islamic to begin with. The days of Islam’s cultural conquests, it would seem, are over; it can win back lapsed adherents, but has no resonance for young people in Berlin, Tokyo, or Moscow. And while nearly a billion people are culturally Islamic – one-fifth of the world’s population – they cannot challenge liberal democracy on its own territory on the level of ideas.” Indeed, it was Islam that was under threat from liberalism: “part of the reason for the current, fundamentalist revival is the strength of the perceived threat from liberal, western values to traditional Islamic societies” (Fukuyama 1992: 46; cf. 1989: 14).

Fukuyama might not have thought that Islam was a threat to liberal values, or the liberal west, but many others. A year after Fukuyama’s book appeared, *Foreign Affairs* published “The Clash of Civilizations?” by the well-known Harvard political scientist Samuel Huntington (1993a). As with Fukuyama, the article provoked great interest and was widely debated; as with Fukuyama, it was duly converted, also with the original question mark removed, into a book, *The Clash of Civilizations and the Remaking of World Order* (1996). The book continued to be debated into the 1990s; but it was undoubtedly the shattering events of September 11, 2001 – the attack on the World Trade Center in New York and the Pentagon in Washington, DC, by radical Islamists – that gave it a new prominence. Many who had initially expressed skepticism towards its main thesis now found themselves, reluctantly and regretfully, in substantial agreement. The media, in America and elsewhere, seized upon the “clash of civilizations” as the key to September 11. The book itself became a worldwide best-seller (Abrahamian 2003; Bottici and Challand 2006: 322).

Though his primary purpose was not to counter Fukuyama, part of the impact of Huntington – especially following September 11 – was undoubtedly his resolute rejection of the optimistic picture of the evolving “universal homogeneous state” and the substitution instead of a view of a world riven by deep-seated and fundamental “civilizational” conflicts. A “one-world, universal civilization,” more particularly one based on western ideas of liberal democracy, was a dangerous myth. Such a view

is rooted in the Cold War perspective that the only alternative to communism is liberal democracy, and that the demise of the first produces the universality of the second. Obviously, however, there are many forms of authoritarianism, nationalism, corporatism, and market communism (as in China) that are alive and well in today's world. More significantly, there are all the religious alternatives that lie outside the world of secular ideologies. In the modern world, religion is a central, perhaps *the* central, force that motivates and mobilizes people . . . The more fundamental divisions of humanity in terms of ethnicity, religions, and civilizations remain and spawn new conflicts. (Huntington 1996: 66–7)

Huntington sought to restore the old concept of civilization. The conventional division of the world into nation-states was outdated. “Our world is one of overlapping groupings of states brought together in varying degrees by history, culture, religion, language, location and institutions. At the broadest level these groupings are civilizations” (Huntington 1993b: 191, 1993a: 24).

Enumerating the civilizations that have existed in world history, Huntington admits, is always difficult – Arnold Toynbee variously offered twenty-one and twenty-three, Spengler identified eight major cultures, William McNeil discusses nine. For Huntington, the major contemporary civilizations are Sinic, Japanese, Hindu, Islamic, Orthodox, western and (possibly) African (Huntington 1996: 44–8). Between these civilizations lie the major “fault lines” in the contemporary world. The coming conflicts will not be primarily within civilizations but between them. Even in 1996, Huntington viewed the conflict between Islam and the west as the most serious contemporary conflict; September 11 confirmed him in this view (Huntington 2002). But in the longer term he saw the civilizational conflict between China and the west as likely to be the most profound and far-reaching (Huntington 1996: 207–45).

The Middle East specialist Bernard Lewis, in an article on “Muslim rage” seems to have provided Huntington with the phrase “the clash of civilizations,” as well as identifying its current phase with “the historic reaction of an ancient rival against our Judeo-Christian heritage” (Lewis 1990: 60; Huntington 1993a: 32). But more fundamentally what Huntington was doing, as his references to Spengler, Toynbee, Sorokin and others made clear, was to revive the philosophy of history as practiced by these thinkers. For him, as for them, the basic unit of history is the civilization and there is no single, unilinear, direction for human history. Rather the human story is the Gibbonesque one of rise and fall, of civilizations reaching a period of efflorescence and dominance only to yield to more energetic and creative civilizations on their flanks. The pattern of history, if there is one, is cyclical rather than unilinear. Moreover, far from there being a convergence on a single, unified, global civilization, there persist division and divergence between civilizational units with their own characteristic cultures. As Huntington put it emphatically in a volume of essays that he co-edited, “cultures count” (Huntington 2000: xiii).

Unlike Fukuyama, who makes his debt to Hegel explicit, Huntington does not link himself directly with any philosopher or philosophy of history. However, it is not difficult to discern a distinct affinity with Arnold Toynbee, and behind him, Oswald Spengler. In addition to the focus on civilizations, as the real units of history, and their contact and competition as the driving force of history, the Toynbeean formula

of “challenge and response” seem appropriate as a description of Huntington’s way of conceiving civilizational conflict. There is also a striking parallel in the way both Huntington and Toynbee consider the decline of civilizations. Toynbee had argued, in his *A Study of History* (1934–61), that a declining civilization in its final stages throws up a “universal state” and proclaims that history has ended. Civilization achieves a kind of stasis; no further fundamental change is to be expected. The civilization offers itself as an exemplar and a model to the rest of the world, which indeed it seeks to incorporate within its own *ecumene*. The Roman Empire, in this as in other aspects of civilizational development, was for Toynbee the clearest expression of this tendency, but he found it also in such instances as the Arab, Ottoman, and Chinese empires.

Huntington does not have such a rigidly schematic approach to the rise and fall of civilizations. But he quotes Toynbee on “the mirage of immortality” that blinds the people of a civilization as they enter the stage of the “universal state.” They are convinced that “theirs is the final form of human society.” But “societies that assume their history has ended . . . are usually societies whose history is about to decline” (Huntington 1996: 301).

Does that mean that western civilization is in decline? Huntington is not sure. “The development of the west to date has not deviated significantly from the evolutionary patterns common to civilizations throughout history. The Islamic Resurgence and the economic dynamism of Asia demonstrate that other civilizations are alive and well and at least potentially threatening to the west.” The West’s current sense of itself as the supreme and most powerful civilization in the world could turn out to be that “mirage of immortality” that has afflicted other civilizations.

But, says Huntington, in history nothing is inevitable. If the west builds upon its common legacy – which crucially means that Europe and America work together and reaffirm their western identity against the weakening forces of “multiculturalism” – it can overcome its internal weaknesses. It must however give up its “false,” “immoral” and “dangerous” belief in the universality of western culture. We live inescapably in a “multicivilizational, multipolar world.” The only prudent attitude, the only one that can stave off “global civilizational war,” is one of the mutual acceptances of differences and a mutual commitment to negotiating them by peaceful means. Beyond that, “peoples in all civilizations should search for and attempt to expand the values, institutions, and practices they have in common with peoples of other civilizations (Huntington 1996: 318). There can, in other words, even for Huntington be the possibility of a world civilization, “Civilization in the singular,” based on commonalities of morality, religion, art, philosophy, technology and material well-being. To achieve that would indeed be to find an answer to “the greater clash, the global ‘real clash’, between Civilization and barbarism” (Huntington 1996: 320–1). On that possibility Huntington expresses agnosticism.

It has been argued that “the clash of civilizations” is one of the political myths with which contemporary world politics is strewn, a myth especially that aims to demonize Islamic civilization (Bottici and Challand 2006). Since the views of Fukuyama and Huntington stand more or less diametrically opposed to one another, no-one can argue that there is some sort of “hegemonic” western discourse at work here. Yet, for all their differences, Fukuyama and Huntington have re-launched the inquiry into long-term and large-scale historical change that characterized the traditional philosophy of

history. With the end of the Cold War, much that had remained bottled up came tumbling out – nationalism, religion, racism, and other things that had been thought to have been banished by modernity (Kumar 2001b). One of those things was history – too much of it, in the eyes of many. Ironically, even the proclamation of “the end of history” was a tribute to the idea that, for many people to whom it had been denied, history was once more something to be made, or at least to be part of. It became once more fitting and perhaps even necessary to ask the philosophical questions about our place in history and our destiny.

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KRISHAN KUMAR

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Index

- abduction 324
action, philosophy of 147–50, 418–19, 498–506
Adorno, Theodor 550
anachronism 274–82, 292–3, 348, 358, 367, 452
Ankersmit, Frank 68, 155–6, 157, 159, 183, 185, 259, 402, 548
Annales 376–7, 401
anti-realism *see* realism
Apel, Karl-Otto 537
apocalyptic literature 429–31
Aristotle 470, 472
asymmetry of overdetermination 58–61
Augustine 34, 431, 470

Bayesian logic and probability 57–8, 89–96, 238, 333
Benjamin, Walter 550
Berlin, University of 386–7
bias 13, 173, 360, 394–5, 542–3, 546
Bloch, Marc 400
Bopp, Franz 386

Carr, David 514–16
Carr, E. H. 14, 109, 547
causation 53–61, 98–108, 124–5, 149, 220–9, 299, 304–5
chaos and criticality 116–18
cold war 551–2
colligation 288
Collingwood, R. G. 15, 24, 27, 94, 100, 101, 106, 111, 137–8, 144, 147, 184–5, 248–9, 454, 500–2, 503, 504–5

common cause 55–61, 91, 220–9, 231–9, 409–10
Comte, Auguste 404–7
conditionals 100–2
constructivism *see* realism vs. anti-realism
counterfactuals 102–3, 109–18, 183
Croce, Benedetto 10, 251, 454, 499–500, 502–4

Danto, Arthur 17, 213
Darwin, Charles 307–16, 336–7, 404–14
Davidson, Donald 124, 139, 148–9, 342, 348
Dilthey, Wilhelm 532–5
Dray, William 92, 100, 101, 113, 139, 144, 147, 166, 249, 348
Duhem, Pierre 289–91
Dummett, Michael 187–8

economic history 377
Eichhorn, Karl Friedrich 386
Elster, Jon 103, 114–15, 214, 215, 300, 346, 489
Elton, Geoffrey (Ehrenberg) 547
Engels, Friedrich 488–9
evidence 15–20, 46, 87–9, 95–6, 107, 163, 176, 178, 184, 231, 233, 299–305, 319–28, 330, 333–4, 378, 384, 396–402
experimental science 44–6
explanation 22–4, 51–61, 91–4, 110–11, 131–40, 152–60, 293, 302–3, 323–6, 342–51, 417–20, 464–5

INDEX

- facts 11–15
 fatalism 122–3, 165
 Febvre, Lucien 28
 feminism 542–3
 folk psychology 247–350
 Foucault, Michel 129–30, 372, 379, 394, 491
 Fukuyama, Francis 468, 552–6
- Gadamer, Hans-Georg 536–7
 Goldstein, Leon 11, 184, 190
 Goodman, Nelson 21
- Habermas, Jürgen 537–8
 Hacking, Ian 46
 Haeckel, Ernst 413
 Hegel, Georg Friedrich 386, 433, 468–76, 477, 490–1, 494, 553–4
 Heidegger, Martin 486, 511–13, 518–20, 524, 535–6
 Hempel, Carl 23, 51–3, 67, 111, 132–3, 143, 165–7, 298–9, 332, 343–4, 417–20, 486
 reception 420–1
 Herder, Johann Gottfried von 457, 462–5
 hermeneutics *see* interpretation, understanding
 Herodotus 354–5
 historical sociology 377
 historicism 67, 396, 422–5, 480, 499
 historicity 508, 511–12, 514–16
 historiography
 comparative 400–2
 conceptual distinctions 1–4, 12, 235, 366–7, 470–1
 critical *see* scientific
 scientific 44–61, 67, 162–70, 178, 301–5, 373, 375, 383, 393–402, 471
 holism *see* individualism vs. holism
 Hume, David 17, 47, 53, 99–100, 138, 145, 253–4, 347
 Huntington, Samuel 556–9
 Husserl, Edmund 243, 251, 508–11, 518
- Ibn Khaldun 35, 441–3
 Ibn Miskawayh 440–1
 individualism vs. holism 209–17, 246–7, 299, 346
 information 17, 61, 223–4, 228–9, 320–5, 368, 399, 413, 439
- interpretation 10, 18–19, 30, 175, 247, 280–1, 288, 301, 335, 364, 378, 529–39
- Jenkins, Keith 547–8
- Kant, Immanuel 254, 433, 457–62, 469–70, 474, 495, 523–4
 see also neo-Kantians
 Kosso, Peter 332, 334, 336
 Kuhn, Thomas 20, 46, 48, 68, 183–4, 291–2, 372, 408
- Lakatos, Imre 291–2, 294, 372
 Lewis, David 58, 102–3, 111–13
 Loewith, Carl 30, 35
 Lyotard, Jean François 29, 68, 491, 541
- Mach, Ernst 288–90
 Mandelbaum, Maurice 103, 166, 173, 175, 182, 213, 250
 Marx, Karl 185–6, 214, 376, 393, 395, 408, 433, 488–96, 500
 memory 362–9
 Merleau-Ponty, Maurice 513–14
 metaphysics 11, 24
Methodenstreit 66, 376
 methodological individualism *see* individualism vs. holism
 middle-range theory 332–3
 Mink, Louis 188
 monism 413
 myth 353–61, 366, 451–3
- narrative 21–2, 24, 53–5, 127–8, 155, 325, 361, 398, 514, 515, 541
 natural history and its philosophy 44–62
 natural vs. cultural sciences 481–6, 537
 Neo-Kantians 243–6, 294, 376, 477–86
 Neurath, Otto 416–17
 Niebuhr, Barthold Georg 384, 386, 388, 393
 Nietzsche, Friedrich 366–7, 524, 554
 nomothetic vs. ideographic sciences 479–82
 Nora, Pierre *see* memory
- objectivity 10–11, 18, 27, 72–4, 172–9, 233–4, 256–8, 299, 384, 394–5, 482, 542, 547

- observation 16, 18, 46, 50, 156, 174–5, 184
 orientalism vs. fundamentalism 443–4, 542
 parsimony 234–6
 Patočka, Jan 518–27
 philosophy of historiography 9–25
 philosophy of historiography–philosophy of history
 conceptual distinction 2–4, 26–30, 41–2
 philosophy of history 26–43, 519
 Plato 357, 372, 473
 Popper, Karl R. 46, 47, 59, 166, 179, 212, 235, 246–7, 269, 293, 323, 344–5, 372, 386, 421–5, 551
 post-colonialism 542
 postmodernism 68–71, 158–60, 401, 540–9
 progress *see* teleology

 Quine, Wilard van Orman 68, 107, 176, 372

 Ranke, Leopold 2–3, 66, 173–4, 256–8, 375–6, 383–91, 393, 395, 397–8
 realism/anti-realism 163, 181–9, 190–7, 402, 474–6
 reduction 342–51
 Reichenbach, Hans 55–6, 220–2
 relativism 21–2, 28, 72, 176–8, 246, 258, 335, 372, 483, 502, 543
 repeatability/repetitiveness *see* unique events
 Rickert, Heinrich *see* neo-Kantians
 Ricoeur, Paul 362–3, 366, 514, 538

 Salmon, Wesley 57, 103, 222, 346
 Sartre, Jean-Paul 513
 Savigny, Carl von 386
 Schleiermacher, Friedrich 529–31
 Schutz, Alfred 514
 Searle, John 214–15, 217

 skepticism 396–7, 543–4
 Skinner, Quentin 274, 293
 Sober, Elliott 57–8, 60, 92, 221–3, 225, 231, 234–5, 237, 239
 social history 377–8
 sociology of knowledge 372, 495–6
 Spengler, Oswald 550
 supervenience 213–14, 326

 teleology 30, 105, 125–6, 166, 255–7, 275–6, 293, 307–16, 390–1, 410–11, 428, 434, 460–1, 472
 testimony 94, 188, 229, 397, 399
 theodicy 428
 Thucydides 356, 384, 397
 truth 181–9, 190–7, 394, 397–8, 448–50, 522, 544–6
 Tucker, Aviezer 28, 57–8, 90–1, 178–9, 300–1, 526

 underdetermination 95–6, 162–5, 178, 290, 325
 understanding 111, 142–50, 248–9, 376, 396, 529–39
 unique events 20, 23, 44, 49, 99, 100, 142, 244–6, 318–28, 346–7, 385

 values 14–15, 482–3
 Van Fraassen, Bas 186–7, 321
Verstehen see understanding
 Vico, Giambattista 35–6, 433, 446–54, 495, 499

 Weber, Max 376, 400
 Whewell, William 287–9
 Whig historiography *see* teleology
 White, Hayden 70, 75–7, 95, 188–9, 402, 541, 544, 548
 Windelband, Wilhelm *see* neo-Kantians
 Wylie, Alison 334, 336