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TOWARDS A REFLEXIVE SOCIOLOGY A WORKSHOP WITH PIERRE BOURDIEU*

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SOME NOTES ON THE RECEPTION OF BOURDIEU'S WORK IN AMERICA

Over the last two decades, the work of French sociologist Pierre Bourdieu has emerged as one of the most innovative, wide-ranging, and influential bodies of theories and research in contemporary social science.¹ Cutting deeply across the disciplinary boundaries that delimit sociology, anthropology, education, cultural history, linguistics, and philosophy, as well as across a broad spectrum of areas of specialized sociological inquiry (from the study of peasants, art, unemployment, schools, fertility, and literature to the

analysis of classes, religion, kinship, sports, politics, law, and intellectuals), Bourdieu's voluminous *oeuvre*² presents a multi-faceted challenge to the present divisions and accepted modes of thinking of sociology. Chief among the cleavages it is striving to straddle are those which separate theory from research, sever the analysis of the symbolic from that of materiality, and oppose subjectivist and objectivist modes of knowledge (Bourdieu 1973c, 1977a, 1980a). Thus Bourdieu has for some time forsaken the two antinomies which have recently come to the forefront of theoretical discussions, those of structure and action on the one hand, and of micro- versus macro-analysis on the other.³

In circumventing or dissolving these and other dichotomies (see Bourdieu 1987e, 1988c, 1988e; also Brubaker 1985, pp. 749–753), Bourdieu has been insistently pointing to the possibility of a *unified political economy of practice*, and especially of symbolic power, that fuses structural and phenomenologically-inspired approaches into a coherent, epistemologically grounded, mode of social inquiry of universal applicability—an *Anthropologie* in the Kantian sense of the term, but one that is highly distinctive in that it explicitly encompasses the activity of the social analyst who sets out to offer theoretical

* The interview part of this text is based on a series of discussions with, and transcripts of talks by, Pierre Bourdieu, held alternately in French and in English over a period of several months in Chicago and Paris. The initial core of the article comes from remarks made by Professor Bourdieu in debate with the participants to the Graduate Workshop on Pierre Bourdieu, a group of doctoral students at the University of Chicago who studied his work intensively during the Winter Quarter of 1987. These conversations and "oral publications" were later complemented by written exchanges and subsequently edited (and in part rewritten) by Loïc J.D. Wacquant, who also added the notes and references. We are grateful to the Social Sciences Division of the University of Chicago for a small grant that made this Workshop possible and to Professor Bourdieu for kindly agreeing to submit himself to a full day of intense questioning. Finally, I would like to thank Daniel Breslau, W. Rogers Brubaker, and Craig J. Calhoun for their helpful suggestions on an earlier draft of the introductory notes (for which I alone bear responsibility) and Norbert Wiley for his friendly support of the whole project.

¹ See the bibliographical references *in fine* for a sample of recent discussions of Bourdieu's sociology. By far the best overview is Brubaker (1985). Several books in English devoted to Bourdieu's work are in the making. The Center for Psychosocial Studies in Chicago recently organized a conference on "The Social Theory of Pierre Bourdieu" which drew together anthropologists, philosophers, sociologists, and linguists from the United States, France, Great Britain, and Germany; a volume is planned under the editorship of Craig Calhoun, Edward LiPuma, and Moishe Postone.

² Bourdieu is the author of some 25 books and approximately 250 articles (not including translations) and it is impossible to even mention them all in this essay. The References include a selection of his major publications, with a special emphasis on those available in English.

³ For reasons that will become obvious below, it is fundamentally mistaken to include Bourdieu among the proponents of "structuration theory," as Münch (1989, p. 101) does, if only because his theory of practice predates Giddens' scheme (1979, 1984) by a decade and more. For a condensed statement of the dialectic of habitus and field, or position and dispositions, by which the French sociologist dissolves the micro/macro opposition, see Bourdieu (1980d and 1981c).

accounts of the practices of others (Bourdieu 1980b, 1982a, 1987a, 1988a). Bourdieu's writings are also unique in that they comprise and blend the full range of sociological styles, from painstaking ethnographic accounts to sophisticated mathematical modelling to highly abstract meta-theoretical and philosophical arguments.⁴

Yet, curiously, this work which is so catholic and systematic in both intent and scope has typically been apprehended in "bits and pieces" and incorporated piecemeal. Garnham and Williams's (1980, p. 209) warning that such "fragmentary and partial absorption of what is a rich and unified body of theory and related empirical work across a range of fields. . . can lead to a danger of seriously misreading the theory" has proved premonitory. If a selected number of his theories and concepts have been used extensively, and sometimes quite effectively, by American social scientists working in specific areas of research or theorizing,⁵ by and large, Bourdieu's work *in globo* remains widely misunderstood and misinterpreted, as the mutually exclusive critiques frequently addressed to it testify. The encyclopedic reach of his particular investigations has tended to hide the underlying unity of Bourdieu's overarching purpose and reasoning.

Perhaps more than in any other country, the reception of Bourdieu's work in America, and to a comparable degree in Great Britain,⁶ has been characterized by fragmentation and piecemeal appropriations that have obfuscated the systematic nature and novelty of his enterprise. Thus, to take but a few instances of such partial and splintered readings, specialists of education quote profusely *Reproduction in Education, Society and Culture* (Bourdieu and Passeron 1977),⁷ but seldom relate its

more structural argument to the conception of action expounded in *Outline of a Theory of Practice* (Bourdieu 1972, 1977a) that underlies it, or even to his prolific research on the genesis and social efficacy of systems of classification and meaning in educational institutions (e.g., Bourdieu, Passeron, and de Saint Martin 1965; Bourdieu and de Saint Martin 1974; Bourdieu 1967a, 1974b, 1981b). As a result, understanding of Bourdieu's so-called "reproduction theory," a staple in the sociology of education, has been substantially hampered. Jay MacLeod's (1987) otherwise excellent ethnographic study of leveled aspirations among working-class youth in an American public housing project provides us with an exemplary instance of such systematic misconstrual.

Because he relies nearly exclusively on the theoretical exposé sketched in the first part of *Reproduction* and, even more so, on *secondary* interpretations of Bourdieu by American commentators,⁸ MacLeod

Classic" by the International Scientific Institute which puts out the Social Science Citation Index. Bourdieu (1989c) reflects upon this. His piece on "Social Reproduction and Cultural Reproduction" (Bourdieu 1973b) is also frequently referred to as representative of his sociology of education, if not of his whole sociology.

⁸ For instance, MacLeod (1987, p. 11, my emphasis), refers to Bourdieu as "a prominent French reproduction theorist." Ignorance of Bourdieu's empirical research is so total that MacLeod (1987, p. 14) is able to quote approvingly Swartz's (1977, p. 553) statement that "many of [Bourdieu's] most interesting insights and theoretical formulations are presented without empirical backing." When discussing the substance of Bourdieu's concepts or propositions, MacLeod repeatedly quotes not from Bourdieu's own writings but from positions attributed to him by Giroux (on school legitimation, p. 12; on the definition of habitus, p. 138) and Swartz (on determinism in the circular relationship between structure and practice, p. 14). This leads MacLeod to present as assessment of Bourdieu that features as omissions and shortcomings what have been the very core and strengths of the latter's sociology: "Bourdieu underestimates the achievement ideology's capacity to mystify structural constraints and encourage high aspirations" (p. 126; compare with the critique of the meritocratic ideology set out in Bourdieu and Passeron's [1979] *The Inheritors*, a book considered by many to have been the Bible of the student movement in May 1968, or with Bourdieu's development of the concepts of misrecognition and symbolic power [e.g., Bourdieu 1979b]), and ignores "the cultural level of analysis" (p. 153)!

⁴ E.g., Bourdieu (1973d, 1979d); Bourdieu et al. (1966, pp. 115–128), Bourdieu and Darbel (1966), Bourdieu and de Saint Martin (1987); and Bourdieu (1979b, 1982a) and Bourdieu and Passeron (1977, Book I) respectively.

⁵ See Lamont and Larreau (1988) for a survey of the diverse uses of Bourdieu's concept of "cultural capital" in American research and the bibliography they cite.

⁶ See Robbins (1988) for a recapitulation of the early English reception of Bourdieu.

⁷ This book was recently pronounced a "Citation

assigns to Bourdieu exactly the kind of objectivist, structuralist position that the latter has discarded and self-consciously set himself the task of overcoming since the mid-sixties (e.g., Bourdieu, Boltanski et al. 1965, pp. 17–23; Bourdieu 1968b and 1973c; Bourdieu 1972, pp. 155–200). Unapprised of the extensive and varied empirical work in which the French sociologist has addressed the very issues he grapples with (namely, why and how agents who occupy similar objective positions in social space come to develop different, even opposite, systems of expectations and aspirations; under what conditions such aspirations turn out to be the internalization of objective chances; how misrecognition and ideological distortion induce the dominated to accept their exclusion as legitimate),⁹ MacLeod presents a truncated snapshot of Bourdieu that entrenches the deterministic misreading of his work.¹⁰ Having thoroughly misrendered it, the author of *Ain't Making It* then finds it necessary to “reinvent” Bourdieu’s theory of habitus in an attempt to overcome the duality of structure and agency and the dead-end of structural causation: the “theoretical deepening” of the concept he claims to effect (MacLeod 1987, pp. 139–48) retraces, in a very rudimentary fashion, some of the very steps taken before him by Bourdieu¹¹ and the new theoretical function he pretends to

assign to a revised theory of habitus—mediating between structure and practice—is that which has, from the outset, been one of the French sociologist’s foremost motives behind his reactivation of this old philosophical notion (Bourdieu 1967b, 1984a, 1985c, 1987a). The final irony, then, is that far from refuting Bourdieu’s “theory” as he maintains,¹² MacLeod’s ethnography strongly supports it and undercuts the very distortions popularized by critics like Swartz and Giroux on which this author bases his contentions.

If sociologists of education rarely extend themselves beyond surface interpretations of *Reproduction* to include Bourdieu’s empirical and anthropological undertakings, conversely, anthropologists refer liberally to *Outline of A Theory of Practice* (Bourdieu 1972, 1977a), which has acquired the status of a classic in their field, or to Bourdieu’s rich and penetrating ethnographies of Algerian peasants and urban workers (Bourdieu 1962a, 1964, 1965, 1973a, 1973d, 1979c; Bourdieu and Sayad 1964), but typically overlook his more sociological forays on school processes, intellectuals, class relations, and on the economy of cultural goods in advanced societies, forays that are directly germane to, buttress, and amplify his anthropological arguments. The effect in this case has been to truncate both the empirical underpinnings of Bourdieu’s rethinking of the nature and limits of anthropological knowledge and to obscure the rationale that underlies his importation of materialist

⁹ See, on French students, Bourdieu (1973b, 1974b), Bourdieu and Passeron (1979); on this same dialectic of objective chances and subjective hopes among Algerian proletarians, Bourdieu et al. (1963), Bourdieu (1973a, 1979c); on class strategies, Bourdieu (1978b), Bourdieu and Boltanski (1977), and the detailed discussion in “Class Future and the Causality of the Probable” (Bourdieu 1974a).

¹⁰ “His is a radical critique of a situation that is essentially immutable” (MacLeod 1987, p. 14). This interpretation resonates with those of Jenkins (1982) and Collins (1981), among others.

¹¹ MacLeod (1987, p. 138 and 128) argues, for instance, that the system of dispositions acquired by agents is shaped by gender, family, educational and occupational history as well as residence and that the limited social mobility allowed by liberal democracies serves to legitimate inequality. Both of these propositions are elaborated by Bourdieu at great length throughout his work (see in particular Bourdieu and Passeron 1977 and 1979; Bourdieu 1974a and 1984a, especially pp. 101–114, 167–175).

¹² “The circular relationship Bourdieu posits between objective opportunities and subjective hopes is incompatible with the findings of this book” (MacLeod 1987, p. 138). See Bourdieu (1974a, 1980d, 1988c) and Harker (1984) for an effective refutation of the “circularity” thesis. Thus the French sociologist (Bourdieu 1974a, p. 5) warns that we “must avoid unconsciously universalizing the model of the quasi-circular relationship of quasi-perfect reproduction which is adequate only in those [particular] cases where the conditions of production of habitus and the conditions of its functioning are identical or homothetical.” In fact, it is hard to think of anyone who would agree more with the chief conclusion of *Ain't No Makin' It* that “social reproduction is a complex process” than Bourdieu, who has devoted a quarter of a century of intense research to documenting and penetrating this complexity (e.g., Bourdieu 1987f and 1989a, Bourdieu and de Saint Martin 1987).

critique into the realm of culture (Bourdieu 1986a, 1988c). Even recent discussions of *Distinction* (Bourdieu 1984a), a *summa* of research-cum-theorizing where the French sociologist brings together many of the topics and themes that exercised him and his research team over the preceding fifteen years, rarely break out of this narrow vision: none of the major extended reviews of the book (Douglas 1981, Hoffman 1986, Berger 1986, Garnham 1986, Zolberg 1986) mentions either *Outline* or its companion volume *Le sens pratique* (Bourdieu 1980a),¹³ in which Bourdieu draws out the more general anthropological conclusions of his research on class, culture, and politics in contemporary France, and links them to his earlier investigations of Kabyle rituals and peasant social strategies.¹⁴

The reasons for such a limited and fractured understanding of a uniquely unified scientific corpus that so forthrightly questions premature specialization and empirical balkanization are several, as Bourdieu's own theory would lead us to predict. First, there are the divisions, at once objective (into disciplinary niches, institutional specialties, and academic networks and turfs) and subjective (in the corresponding categories of perception and appreciation), that structure the field of U.S. social science and in turn shape the reception of foreign intellectual products. Thus American scholars typically seek to force Bourdieu's sociology into the very dualistic alternatives (micro/macro, agency/structure, normative/rational, function/conflict, synchrony/diachrony, etc.) that it aims at transcending.¹⁵ In the same way,

as was hinted above, commentators often pigeon-hole him in some empirical sub-specialty and limit their exegesis to that portion of his research that falls within its purview, ignoring the extensions, revisions and corrections Bourdieu may have made when studying similar processes in a different social setting. By seeking thus to "retranslate" Bourdieu's work into home-grown, or at least more familiar, theoretical idioms (for instance, as a combination of Blau and Giddens, with a touch of Goffman and Collins)¹⁶ or to apportion or assimilate him into standard empirical subfields (as a sociologist of education, analyst of taste, class theorist, student of sports, critic of linguistics, etc.), rather than to try to understand his work in its own terms (as is the case with other major European social theorists),¹⁷ they have created a largely

strategy of theoretical *reductio* is Elster's (1984a) effort to fit Bourdieu's analysis of distinction into the Procrustean bed of functional, causal, and intentional explanations. This allows him to declare it irretrievably flawed on "methodological" grounds—but at the cost of so total an initial distortion of Bourdieu's thesis that its distinctive structure and content have by then entirely disappeared anyway. This is pointed out by a fellow "analytical Marxist" who recognizes that "even a quick look at [Bourdieu's] main theoretical essay, and at concrete sociological explanations he offers elsewhere, reveals a picture very different from the strawmen erected here and there in Boudon's and Elster's footnotes" (Van Parijs 1981, p. 309).

¹⁶ There are no doubt large areas of overlap and convergence between the concerns of Bourdieu and those of social theorists such as Giddens or Habermas. One immediate and critical difference between them, though, is that Bourdieu's theoretical advances are fully grounded in, and geared to return to, empirical research. See *infra* for Bourdieu's views on this.

¹⁷ It is interesting to speculate why the works of Habermas and Foucault, for instance, which, on face value, are just as alien as Bourdieu's to American categories of sociological understanding, have not suffered from the same urge to read them into national traditions and pre-constructions. Arguably, the fact that they advertise themselves as philosophers (or philosopher-cum-sociologist in one case and philosopher-cum-historian in the other), whereas Bourdieu forthrightly takes up the mantle of sociology, has given them a warrant for legitimate "otherness" and helped shield them from such extreme ethnocentric reduction (see Merquior [1985] for an analysis of the academic success of Foucault along those lines, i.e., as a product of his affiliation to the mixed genre of "litero-philosophy"). Another reason for such differences in treatment may also have to do with the fact that, in contradistinction to

¹³ Again, the critical exception is Brubaker's (1985) comprehensive discussion of Bourdieu's sociology that very explicitly and extensively links the two works.

¹⁴ In point of fact, these two volumes, *Distinction* and *Le sens pratique*, are so intimately interwoven in Bourdieu's mind that, shortly before they went to print almost simultaneously, he inverted their concluding chapters so that each cannot be read in full without tackling at least part of the other.

¹⁵ Brubaker (1985, p. 771) aptly notes that "the reception of Bourdieu's work has largely been determined by the same 'false frontiers' and 'artificial divisions' (Bourdieu 1980b, p. 30, 35) that his work has repeatedly challenged." Paradigmatic of this

fictitious Bourdieu made up of a congeries of seemingly unrelated and incomprehensibly dispersed inquiries with little apparent connection beyond that of the identity of their author.

This *intellectual ethnocentrism*—the inclination to refract Bourdieu through the prism of native sociological lenses¹⁸ has been strongly fortified by the erratic, incomplete, and lagged flow of translations, which has not only disrupted the sequence in which his investigations were conducted and articulated, but has also kept a number of key writings out of the reach of his American audience. The exigencies of translation have led to a confusing compression of the chronology of Bourdieu's work (reinforced by the author's own tendency to rework his materials endlessly and to publish with years of delay) or even to a reversal for English-speaking readers.¹⁹ The fact that the genuinely open and *collective* nature of Bourdieu's enterprise clashes with the deeply entrenched American stereotype of the French "patron" and

"circle" (popularized by Terry Clark [1973] and Lemert [1981, 1982, 1986]) constitutes yet another obstacle. To an extent, such quasi-concepts born from the uncontrolled projection, onto the French intellectual universe, of the foreign observer's relation to it, as in Lemert's hydra-like *tout Paris*, have obscured the real functioning of the French sociological field from view and, most notably, the striking parallels, both institutional and intellectual—some of them *crescive*, many others arrived at by design—between Bourdieu's research team and the Durkheimian school. Consequently, the sprawling mass of empirical studies published in the journal founded in 1975 by Bourdieu, *Actes de la recherche en sciences sociales*, by himself and others, is almost never consulted by American readers, just as the ongoing work by his colleagues and current or former associates at the Center for European Sociology in Paris are regularly overlooked.²⁰

The Anglophone reception of Bourdieu has also been considerably affected by the general unfamiliarity of American social scientists with the Continental traditions of social theory and philosophy which form the backdrop of his endeavor, most of which do not partake of the "horizon of expectations" (Jauss 1982) of mainstream American sociology. This, of course, is partly true of other major European strands of social-cultural theory, including Habermas, Foucault, phenomenology, and structuralism, as Wuthnow et al. (1984, p. 7) point out. However, a grasp of the nexus of antagonistic and competing positions within and against which the French sociologist developed his own stance²¹ is par-

Habermas's for instance, Bourdieu's work is rich and precise in empirical content and can thus fall prey to both theoretical *and* empirical retranslation. Finally, there is the content of their respective theories: Bourdieu's sociology contains a radically disenchanting questioning of the symbolic power of intellectuals that sits uneasily with Habermas' and Foucault's comparatively more prophetic stances.

¹⁸ All academic fields tend to be ethnocentric. In the case of the United States, however, this is aggravated by the "blindness of the dominant" due to the hegemonic status of American social science worldwide. American intellectual myopia functions as the opposite of that of smaller sociologies, such as Dutch sociology (cf. Heilbron 1988): while the latter cannot see themselves, the former does not see others and tends to see itself everywhere.

¹⁹ Only 7 of Bourdieu's books are presently available in English (compared to 11 in German). At least 5 more are currently being translated. Two examples: the English version of the 1964 monograph *The Inheritors* came out in English in 1979, two years after the 1970 book *Reproduction* which was based upon it. The pivotal volume *Le métier de sociologue* (Bourdieu, Chamboredon, and Passeron 1968) in which Bourdieu and his associates lay out the tenets of the revised "applied rationalism" that supplies the epistemological foundations of his entire project, remains untranslated to this day. As a result, readers who are not conversant with the work of Bachelard and of the French school of the history of science (notably Koyré and Canguilhem) are left in the dark about the critical-historicist theory of knowledge that underlays Bourdieu's sociology.

²⁰ Among those and other writings closely influenced by Bourdieu, one should site at minimum Boltanski (1987, 1984a), Boltanski and Thévenot (1983), Verdès-Leroux (1978, 1983), Grignon (1971), Maresca (1983), Viala (1985), Castel (1988), Muel-Dreyfus (1983), Charles (1987), de Saint Martin (1971), Suaud (1978), Moulin (1987), Boschetti (1988), Bozon (1984), Isambert (1984), Pinçon (1987), Pinto (1984), Viala (1985), Zarca (1987), Caro (1982), and Chamboredon et Prévot (1975). See also the bibliographic references for a selection of articles from *Actes de la recherche en sciences sociales* that draw upon, apply, or extend Bourdieu's scheme.

²¹ Among others, the opposition between Sartrean phenomenology and Lévi-Straussian structuralism,

ticularly crucial because Bourdieu is an unusually self-conscious writer who reflects incessantly and intensely upon the intellectual and social determinants that bear on his enterprise.²² Furthermore, much of his thinking was shaped by a definite reaction both *against* the positivist model of social science imported into France by the first generation of America-trained social scientists in the fifties and sixties (Stoetzel, Boudon, and Crozier among others),²³ and *against* the “literaro-philosophical” tradition (Merquior 1985) that reigned over the French intellectual universe of the 1950s. A good many aspects of his sociology remain largely unscrutable unless one has a definite idea of the streams of thought that influenced him, whether positively or *a contrario*, and of the images of the intellectual that formed the “regulative idea” of his *Beruf*—balancing uneasily between the ambivalent rejection of the “total intellectual,” as he put it in a tribute to Sartre who symbolized it (Bourdieu 1980e), and a deeply political opposition both to the “soft humanism” of Christian phenomenologists and to the epistemological haughtiness implied in the structuralist conception of practice and knowledge (a twin set of attitudes that was no doubt exacerbated by Bourdieu’s first-hand ex-

perience of the constraints and ambiguities of the role of the intellectual in the dramatic circumstances of the Algerian war).

This has been compounded by the fact that what recent French social theory American sociologists have paid attention to—Derrida’s “deconstruction,” Lyotard’s “post-modernism,” and Barthes’ or Baudrillard’s semiology—stands poles apart from Bourdieu, in spite of superficial similarities. The recent fad of “post-” or “super-structuralism” (Harland 1987)²⁴ has tended to divert attention from Bourdieu’s less glamorous and media-conscious claims or, worse, to enshroud him in the halo of theoretical currents he has unceasingly combatted since their emergence.²⁵ Last but not least, there is the extreme difficulty of Bourdieu’s style and prose. The idiolect he has created in order to break with the common-sense understandings embedded in common language, the nested and convoluted configuration of his sentences designed to convey the essentially relational and recursive character of social processes, the density of his argumentation have not facilitated his introduction into the discourse of Anglo-American social science.²⁶ All of these

which Bourdieu (1980a: Preface) regarded, very early on, as the embodiment of fundamental scientific options; the subtle influence of Merleau-Ponty, Husserl and Heidegger; the desire to undercut the claims of structural Marxism; the mediation of Mauss; or Bourdieu’s early appropriation of Cassirer, Saussure, Schutz, and Wittgenstein, etc. It is also important to note what germane traditions of thought Bourdieu drew relatively little upon (for example the Frankfurt school) or ignored almost entirely (most prominently Gramsci, whom he admits to having read very late, cf. Bourdieu 1987a, p. 39). For an account by Bourdieu of the transformation of the French intellectual field in the post-War era, and of his situation and trajectory within it, see Bourdieu and Passeron (1968), Bourdieu (1979b, 1986a, 1987a) and Honneth, Kocyba and Schwibs (1986).

²² Witness the mix of fiery passion and cold analytic persistence he puts into neutralizing a whole array of potential misreadings of *Homo Academicus* (1988a, chapter 1, “A ‘Book for Burning?’”). Also Bourdieu (1980a, 1980b, 1987a).

²³ Bourdieu was alone among the notable French sociologists of his generation conspicuously not to attend Lazarsfeld’s famed seminars at the Sorbonne in the sixties.

²⁴ A label, it should be noted in passing, which is used strictly by English-speaking exegetes and has no currency in France, even among those it presumably designates, cf. Descamps (1986), Montefiore (1983).

²⁵ In this respect, while it shares with all (post-) structuralisms a rejection of the Cartesian *cogito*, Bourdieu’s project differs from them in that it represents an attempt to make possible, through a reflexive application of social-scientific knowledge, the historical emergence of something like a rational (or a reasonable) subject. It is highly doubtful, therefore, that “Bourdieu would gladly participate in splashing the corrosive acid of deconstruction on the traditional subject” as Rabinow (1982, p. 175) claims. See Bourdieu (1984a, pp. 569, 494–500, 1987d) on Baudrillard and Derrida respectively. Bourdieu and Passeron’s (1963) critique of the “sociologists of mutations” and “massmediology” in the early sixties (mainly Edgar Morin and Pierre Fougeyrollas) would seem to apply *mutatis mutandis* to much of the Baudrillardian writings of today.

²⁶ Although it has not prevented it altogether. See Light et al. (1989) for an example of distillation of Bourdieu into introductory textbook material. The two volumes by Accardo (1983) and Accardo and Corcuff (1986) have attempted to do much the same thing in French in a more systematic fashion. Again,

factors have combined and reinforced one another to prevent American social scientists from fully grasping the originality, scope, and systemacity of Bourdieu's sociology.

The recent publication in English of *Homo Academicus* (Bourdieu 1988a) and of *Language and Symbolic Power* (Bourdieu 1989a), as well as a string of other papers in American journals (Bourdieu 1987b, 1987c, 1987d, 1987g, 1988c, 1988d, 1988e, 1988f, forthcoming),²⁷ offers an opportunity to begin to redress this situation. With these books, two nodes of issues that have preoccupied Bourdieu over a number of years become accessible to an English-speaking audience: the analysis of intellectuals and of the objectifying gaze of sociology; the study of language and linguistic practices as an instrument and an arena of social power. Both imply very directly, and in turn rest upon, a self-analysis of the sociologist as a cultural producer and a reflection on the social-historical conditions of possibility of a science of society. Both of these themes are also at the center of Bourdieu's meticulous study of *Heidegger's Political Ontology* (1988b) and of the recent collection of essays entitled *Choses dites* (1987a) in which the French thinker turns his method of analysis of symbolic producers upon himself. Exploring the intent and implications of these books provides a route for sketching out the larger contours of Bourdieu's intellectual landscape and for clarifying key features of his thought. Beyond illustrating the open-ended, diverse, and fluid nature of his scientific project better than would a long exegesis, the following dialogue, loosely organized around a series of *epistemic displacements* effected by Bourdieu, brings out the underlying

one must wonder whether incessant complaints over Bourdieu's style and syntax are not a symptom of a much deeper difficulty—or of a reluctance to embrace a style of thought that makes one squirm as it cuts through the mist of one's enchanted relationship to the social world and to one's condition as an intellectual—since other “difficult” writers (Habermas, Foucault or even Weber come to mind) do not elicit nowhere near the same level of protestation as the author of *Distinction* does.

²⁷ See the other recent English-language writings listed in the selected bibliography at the end of this article.

connections that unify his empirical and theoretical work. In so doing, it should help clear out some of the obdurate obstacles that stand in the way of a more adequate and more fruitful appropriation of his sociology in America.

FROM THE SOCIOLOGY OF ACADEMICS TO THE SOCIOLOGY OF THE SOCIOLOGICAL EYE

Loïc J.D. Wacquant: In *Homo Academicus* (Bourdieu 1988a), you offer a sociology of your own universe, that of French intellectuals. But clearly your aim is not simply to write a monograph on the French university and its faculty, but to make a much more fundamental point about the “sociological method.” Can one speak of a “surface object” and a “true object” in this investigation?

Pierre Bourdieu: My intention in doing this study—which I began in earnest in the mid-sixties, at a time when the crisis of the academic institution which was to climax with the student movement of '68 was rampant but not yet so acute that the contestation of academic “power” had become open—was to conduct a sort of sociological experiment about sociological practice itself. The idea was to demonstrate *in actu* that, contrary to the claims of those who pretend to undermine sociological knowledge or seek to disqualify sociology as a science on the grounds that (as Mannheim insisted, and before him Weber and Marx) the sociologist is socially situated, included in the very object he or she wishes to objectivize, sociology can escape to a degree from this historicist circle, by drawing on its knowledge of the social universe in which social science is produced to control the effects of the determinisms which operate in this universe and, at the same time, bear on social science itself.

So you are entirely right, throughout this study, I pursue a double goal and construct a double object: the naive, apparent object of the French university as an institution, which requires an analysis of its structure and functioning, of the various species of power that are efficient in this universe, of the trajectories and

agents who come to take up positions in it, of the “professorial” vision of the world, etc.; and the deeper object of the reflexive return entailed in objectifying one’s own universe: that which is involved in objectifying an institution socially recognized as founded to claim objectivity and universality for its own objectifications.

LW: This device—using the university, that is, the taken-for-granted setting of your own daily life, as a pretext for studying the sociological gaze—is one you had previously used when, in the early sixties, you conducted an investigation of marriage practices in your own village in Southwestern France (Bourdieu 1962b, 1962c, 1977b) after completing one of similar practices among Algerian peasants (Bourdieu 1972, 1980a).

PB: Yes. *Homo Academicus* represents the culmination, at least in a biographical sense, of a very self-conscious “epistemological experiment” I started in the early sixties when I set out to apply to my most familiar universe the methods of investigation I had previously used to uncover the logic of kinship relations in a foreign universe, that of Algerian peasants and subproletarians.

The “methodological” intent of this research, if we may call it that, was to overturn the natural relation of the observer to his universe of study, to make the mundane exotic and the exotic mundane, in order to render explicit what, in both cases, is taken for granted and to offer a very concrete, very pragmatic, vindication of the possibility of a full sociological objectivation of the object *and* of the subject’s relation to the object—what I call *participant objectivation* (Bourdieu 1978a). This required resisting a temptation that is no doubt inherent in the posture of the sociologist, that of taking up the absolute point of view upon the object of study—here to assume a sort of intellectual power over the intellectual world. So in order to bring this study to a successful issue and to publish it, I had to discover the deep truth of this world, namely, that everybody in it struggles to do what the sociologist is tempted to do. I had to objectivize this temptation and, more pre-

cisely, to objectivize the form that it took at a certain time in the sociologist Pierre Bourdieu.

LW: Throughout your work, you have emphasized this need for a reflexive return on the sociologist and on his/her universe of production, insisting that it is not merely a form of intellectualism but has real scientific consequences. What is the significance of this return from an epistemological or theoretical point of view? And what difference does it make, concretely, to do a reflexive sociology of the kind you advocate?

PB: Indeed, I believe that the sociology of sociology is a fundamental dimension of sociological epistemology. Far from being a specialty among others, it is the necessary prerequisite of any rigorous sociological practice. In my view, one of the chief sources of error in the social sciences resides in an uncontrolled relation to the object which results in the projection of this relation into the object. What distresses me when I read some works by sociologists is that people whose profession it is to objectivize the social world prove so rarely able to objectivize themselves and fail so often to realize that what their apparently scientific discourse talks about is not the object but their relation to the object—it expresses resentment, envy, social concupiscence, unconscious aspirations or fascinations, hatred, a whole range of unanalyzed experiences of and feelings about the social world.

Now, to objectivize the objectivizing point of view of the sociologist is something that is done quite frequently, but in a strikingly superficial, if apparently radical, manner. When we say “the sociologist is inscribed in a historical context,” we generally mean “the bourgeois sociologist” and leave it at that. But objectivation of any cultural producer involves more than pointing to—and bemoaning—his class background and location, his race or his gender. We must not forget to objectivize his position in the universe of cultural production, in this case the scientific or academic field. One of the contributions of *Homo Academicus* is to demonstrate that, when we carry out objectivations à la Lukács (and after him Lucien Goldmann,

to take one of the most sophisticated forms of this very commonplace sociologicistic reductionism), that is, brutally put in direct correspondence cultural objects and their producers (or their public, as when it is said that such a form of English theater expresses "the dilemma of a rising middle class"), we commit what I call the *short-circuit fallacy* (Bourdieu 1988d): by seeking to establish a direct link between very distant terms, we omit the crucial mediation provided by the relatively autonomous space of the field of cultural production.

But to stop at this stage would still leave unexamined the most essential bias, whose principle lies neither in the social positioning, nor in the specific position of the sociologist in the field of cultural production (i.e., his or her location in a space of possible theoretical, substantive, or methodological stances), but in the invisible determinations inherent in the intellectual posture itself, in the scholarly gaze, that he or she casts upon the social world. As soon as we observe (*theorein*) the social world, we introduce in our perception of it a bias due to the fact that, to study it, to describe it, to talk about it, we must retire from it more or less completely. This *theoreticist* or intellectualist bias consists in forgetting to inscribe, into the theory we build of the social world, the fact that it is the product of a theoretical gaze, a "contemplative eye." A genuinely reflexive sociology must avoid this "ethnocentrism of the scientist" which consists in ignoring everything that the analyst injects in his perception of the object by virtue of the fact that he is placed outside of the object, that he observes it from afar and from above. Just like the anthropologist who constructs a genealogy entertains a relation to "kinship" that is worlds apart from that of the Kabyle head of clan who must solve the very practical and urgent problem of finding an appropriate mate for his daughter, the sociologist who studies the American school system, for instance, is motivated by preoccupations and has a "use" of schools that have little in common with those of a father seeking to find a good school for his daughter.

The upshot of this is not that theoretic knowledge is worth nothing but that we

must know its limits and accompany all scientific accounts with an account of the limits and limitations of scientific accounts: theoretic knowledge owes a number of its most essential properties to the fact that the conditions under which it is produced are not that of practice.

LW: In other words, an adequate science of society must construct theories which comprise within themselves a theory of the gap between theory and practice.

PB: Precisely. An adequate model of reality must take into account the distance between the practical experience of agents (who ignore the model) and the model which enables the mechanisms it describes to function with the unknowing "complicity" of agents. And the case of the university is a litmus test for this requirement, since everything here inclines one to commit the theoreticist fallacy. Like any social universe, the academic world is the site of a struggle over the truth of the academic world and of the social world in general. (Very rapidly, we can say that the social world is the site of continual struggles to define what the social world is; but the academic world has this peculiarity today that its verdicts and pronouncements are among the most powerful socially.) In academe, people fight constantly over the question of who, in this universe, is socially mandated, authorized, to tell the truth of the social world (e.g., to define who and what is a delinquent, where the boundaries of the working class lie, whether such and such a group exists and is entitled to rights, etc.). To intervene in it as a sociologist naturally carried the temptation of claiming for oneself the role of neutral referee, of the judge, to distribute rights and wrongs.

In other words, the intellectualist and theoreticist fallacy (which, in anthropology takes the form of the epistemic claim that "I know better than my informant") was the temptation *par excellence* for someone who, being a sociologist, and thus party to the ongoing struggle over truth, set out to tell the truth of this world of which he is a part and of the opposed perspectives that are taken on it. The necessity of the reflexive return is not the

expression of a sort of epistemological “sense of honor” but a principle that leads to constructing scientific objects into which the relation of the analyst to the object is not unconsciously projected. The fact that I had explicitly assigned myself the purpose of scrutinizing the object (the University) but also the work of construction of the object allowed me, I believe, to sidestep the intellectualist trap. I was aware from the outset that my task involved not simply telling the truth of this world, as can be uncovered by objectivist methods of observation, but also showing that this world is the site of an ongoing struggle to tell the truth of this world.

This temptation to crush one’s rivals by objectifying them, which was ever-present in the objectivist phase of this research, is at the roots of serious *technical* mistakes. I emphasize “technical” here to stress the difference between scientific work and pure reflection. For everything that I have just said translates into very concrete research operations: variables added or taken out of correspondence analyses, sources of data reinterpreted or rejected, new criteria inserted into the analysis, etc. For instance, anticipating the hostile reactions that such questions would trigger among intellectuals, I knew that I could not resort to direct interviewing; I had to resign myself, in the manner of historians, to prosopography, and to using strictly public and published information. Every single indicator of intellectual notoriety I use required an enormous amount of work to construct because, in a universe where identity is made largely through symbolic strategies and by collective belief, the most minor piece of information (is so and so an *agrégé*?) had to be independently verified from different sources.

LW: This return upon the generic relation of the analyst to his object and upon the particular location he or she occupies in the space of scientific production would be what distinguishes the kind of reflexivity you advocate from that of Gouldner (1970) or Garfinkel (1967)?

PB: Yes. Garfinkel is content with explicating only things that are very general, universal, tied to the status of the agent as

a knowing subject; his reflexivity is strictly phenomenological in this sense. In Gouldner, reflexivity remains more a programmatic slogan than a veritable program of *work*. What must be objectivized is not the individual who does the research in his biographical idiosyncrasy but the position he occupies in academic space and the biases implicated in the stance he takes by virtue of being “out of the game” (*hors jeu*). What is lacking most in this American tradition, no doubt for very definite sociological reasons (among which the lesser role of philosophy in the training of researchers and the weaker presence of a critical political tradition can be singled out) is a truly reflexive and critical analysis of the academic institution and, more precisely, of the sociological institution, conceived not as an end in itself but as the condition of scientific progress.

This is to say, in passing, that the kind of “sociology of sociology” that I advocate has little in common with this kind of complacent and intimist return upon the private *person* of the sociologist²⁸ or with a search for the intellectual *Zeitgeist* that animates his or her work (as, for instance, in Gouldner’s [1970] analysis of Parsons in *The Coming Crisis of Sociology*), or yet with this self-fascinated, and a bit complacent, observation of the observer’s writings which has recently become something of a fad among some American anthropologists (e.g., Marcus and Fisher 1986, Geertz 1987) who, having become *blasé* with fieldwork, turn to talking about themselves rather than about their object of research. This kind of falsely radical denunciation of ethnographic writing as “poetics and politics” (Clifford and Marcus 1986) which becomes its own end opens the door to a form of thinly-veiled nihilistic relativism (of the kind that one finds also in some versions of the “strong programme” in the sociology of science, notably in Latour’s [1987] recent work) that stands as the polar opposite to a truly reflexive social science.

²⁸ Bourdieu’s (1988a) elaboration of the important distinction between “epistemic individual” and “empirical individual” is relevant here. Also Bourdieu (1987c).

LW: What is your response to the criticism that may be levied that *Homo Academicus* deals exclusively with a particular case, that of France, which poses problems for generalization, and that furthermore the data are twenty-years old?

PB: Inasmuch as the real object of the analysis goes well beyond the apparent one, the historical specificity of the French case in no way invalidates or limits the implications of the inquiry. But I would go further: one of the goals of the book is to show that the opposition between the universal and the unique, between nomothetic analysis and ideographic description, is a false antinomy. The relational and analogical mode of reasoning fostered by the concept of field enables us to grasp particularly within generality and generality within particularity, by making it possible to see the French case as a “particular case of the possible” as Bachelard says. Better, the specific historical properties of the French academic field—its much higher degree of centralization and institutional unification, its well-delimited barriers to entry, if we contrast it with the American higher education system for instance—make it a uniquely suited terrain for uncovering some of the universal laws that tendentially regulate the functioning of all fields.

Likewise, the criticism—which was already raised against *Distinction* by some of my American commentators—that the data are old entirely misses the mark inasmuch as one of the purposes of the analysis is to uncover *transhistorical invariants*, or sets of relations between structures that persist within a clearly circumscribed but relatively long historical period. In this case, whether the data are 5 or 15 years old matters little. Proof is that the main opposition that emerges, within the space of disciplines, between the college of arts and sciences on the one hand and the schools of law and medicine on the other, is nothing other than the old opposition, already described by Kant in *The Conflict of the Faculties*, between the faculties that directly depend upon temporal powers and owe their authority to a sort of social delegation and the faculties that may be labelled “pure,” self-founded, whose authority is premised

upon scientificity (the faculty of sciences being typical of this category).

And I recently carried out yet another experimental verification of this principle of the durability of fields as relational configurations by showing that the structure of the field of French *Grandes Ecoles*, conceived as a set of objective positional differences and distances among elite graduate schools, and between them and the social positions of power which lead to them and to which they in turn lead, has remained remarkably constant, nearly identical in fact, over the twenty-year period from 1968 to the present (Bourdieu and de Saint Martin 1987; Bourdieu 1987f and 1989a).

LW: Precisely, several commentators (e.g., Collins 1981, Jenkins 1982, Sulkunen 1982, Connell 1983, Wacquant 1987) have criticized your models for being static and “closed”, leaving little room for resistance, change, and the irruption of history. Doesn’t *Homo Academicus* answer this concern by putting forth an analysis of May ’68 which, in effect, dissolves the opposition between structure and history and between structural history and event history?

PB: I must say that I find many of these criticisms strikingly superficial; they reveal that those who make them may have paid more attention to the titles of my books (most blatantly in the case of *Reproduction*) than to the actual analyses they contain. I have repeatedly denounced both what I call the “functionalism of the worst case” and the dehistoricizing that follows from a strictly structuralist standpoint (e.g., Bourdieu 1968b and 1987a, pp. 56ff.). Likewise, I cannot begin to comprehend how relations of domination, whether material or symbolic, could possibly operate without implying, activating resistance. The dominated, in any social universe, can always exert a certain force, inasmuch as to belong to a field means by definition that one is capable of producing effects in it (if only to elicit reactions of exclusion on the part of those who occupy the dominant positions), thus of putting certain forces into motion.

In *Homo Academicus*, I try to account, as completely as possible, for the crisis of

May '68 and, at the same time, to put forth some of the elements of an invariant model of crises or revolutions. In the course of the analysis of this specific event, I discovered a number of properties which have me wondering if they are not very general. First I tried to show that the crisis internal to the university was the product of the meeting of two partial crises provoked by separate, autonomous evolutions. On the one hand we have a crisis among the faculty triggered by the effects of the rapid and massive swelling of the ranks of professors and by the resulting tensions between the dominant and subordinate categories of teachers. On the other hand, we find a crisis of the student body due to a whole range of factors, including the overproduction of graduates, the devaluation of credentials, etc. These partial, local crises converged, providing a base for conjunctual alliances. The crisis then spread along lines which were very determinate, toward instances of symbolic production in particular (radio, TV, the church, and so on), that is, all those universes in which there was a conflict of legitimacy between the established holders of the legitimacy of discourse and the new contenders who preached the ministry of the universal.

LW: More generally, could you clarify the place of history in your thinking?

PB: Obviously, this is an immensely complex question and I can only outline its resolution in the most general terms. Suffice it to say that the separation of sociology and history is a disastrous division and one totally devoid of epistemological justification: all sociology should be historical and all history sociological. In point of fact, one of the functions of the theory of fields that I propose is to make the opposition between reproduction and transformation, statics and dynamics, or structure and history, vanish. As I tried to demonstrate practically in my research on the French literary field in Flaubert's time and on the artistic field around Manet's time (Bourdieu 1983d, 1987i, 1988d), we cannot grasp the dynamics of a field if not by a synchronic analysis of its structure and, simultaneously, we cannot grasp this structure without a historical, or genetic,

analysis of its constitution, and of the tensions that exist between positions, as well as between this field and other fields, and especially what I call the field of power.

In the present state of the social sciences, however, I think that the history of the *longue durée*, the kind of "macro-history" most sociologists practice when they tackle processes of rationalization, bureaucratization, modernization, etc., continues to be one of the last refuges of a thinly-masked social philosophy. What we need to do, rather, is a form of structural history that is rarely practiced, which finds in each successive state of the structure under examination both the product of previous struggles to maintain or to transform this structure and the principle, via the contradictions, the tensions, and the relations of force which constitute it, of subsequent transformations.

The intrusion of pure historical events, such as May '68 or any other great historical break, becomes understandable only when we reconstruct the plurality of "independent causal series" of which Cournot spoke to characterize chance (*le hasard*), that is, the different and relatively autonomous historical concatenations that are put together in each universe and whose collision, through synchronization, determines the singularity of historical happenings. But here I will refer you to the analysis of May 68 that I developed in the last chapter of *Homo Academicus* and which contains the embryo of a theory of symbolic revolution that I am presently developing.

FROM STRUCTURE TO FIELD

LW: In the preface to the English edition of *Homo Academicus*, you write that this book "tacitly refutes the notion of profession." What is it in the notion of profession, or in the sociology of occupations as it is practiced in the U.S. in particular, that you find objectionable? What separates an analysis conducted in terms of field from one conducted in terms of profession?

PB: The notion of profession is dangerous because it has all appearances of false

neutrality in its favor. Profession is a folk concept which has been uncritically smuggled into scientific language and which imports in it a whole social unconscious. It is the product of a historical work of construction and representation of a group which has slipped into the very science of this group. This is why this “concept” works so well, or too well: the category of profession refers to realities that are, in a sense, “too real” to be true, since it grasps at once a *mental* category and a *social* category, socially produced only by superseding or obliterating all kinds of differences and contradictions.

All this social work of construction of the category must be undone and analyzed so that a rigorous sociological construct can be built that accounts for its success. Everything becomes different, and much more complicated if I take seriously the *work of agregation* and symbolic imposition that was necessary to produce the “academic profession” and if I treat it as a field, that is, a space of social forces and struggles.²⁹ The first question that arises is: How to draw up a representative sample in a field? If, following the canon dictated by orthodox methodology, you take a random sample, you mutilate the very object you have set out to construct. If, in a study of the field of lawyers, for instance, you do not draw the President of the Supreme Court, or if, in an inquiry into the French intellectual field of the 1950s, you leave out Jean-Paul Sartre, or Princeton University in a study of American academics, your field is destroyed, insofar as these personas or institutions alone mark a crucial position—there are positions in a field which command the whole structure.³⁰ Moreover, there is an ongoing struggle over the limits of the field of academics, over who belongs to it and who does not. This is a question that the most daring of positivists solve by what they call an “operational definition,” by arbitrarily

deciding who is included and who is not. Again, this empirist surrender has all appearances for itself, since it abandons to the social world *as it is*, to the established order of the moment, the most essential operations of research, thereby fulfilling a deeply conservative function of ratification of the *doxa*.

Naturally, if you adopt the notion of profession as an instrument—rather than as an object—of analysis, none of this creates any difficulty. As long as you take it as it presents itself (as in the hallowed *data* of positivist sociologists), no profession is difficult to apprehend. What group would turn down the sacralizing and naturalizing recording of the social scientist? What “profession” would take exception to a sociological report that gives objective, that is public, reality to their subjective representation of their collective being? As long as you remain within the realm of socially constituted and socially sanctioned appearances—and this is the order to which the notion of “profession” belongs—you will have all appearances in your favor, even the appearance of scientificity.

In other words, to accept the preconstructed notion of profession is to lock oneself up in the alternative of celebration (as do many American studies of “professions”) and partial objectivation. By reconceptualizing it as a field, as I do in *Homo Academicus*, it becomes possible to break with the notion of profession *and* to reintegrate it within a model of the full reality it pretends to capture.

LW: The notion of field is, together with those of habitus and capital, the central organizing concept of your work, particularly your more recent work, which includes studies in the fields of artists and intellectuals, classes, lifestyles, Grandes Ecoles, religion, the field of power, of law, of housing construction, etc.³¹ You use the

²⁹ See Boltanski (1987) for an in-depth examination of the organizational and symbolic invention of the category of “cadres” in French society.

³⁰ How Sartre both dominated, and was in turn dominated by his own domination in, the French intellectual field is shown in detail by Boschetti (1988) and Bourdieu (1980e, 1984b).

³¹ On the intellectual and artistic field, see *inter alia* Bourdieu (1971a, 1975b, 1975c, 1983a, 1983d, 1988a); on the field of classes and class lifestyles, Bourdieu (1978b, 1984a, 1987b); on cultural goods, Bourdieu (1980h, 1985d) and Bourdieu and Delsaut (1975); on the religious field, Bourdieu (1971b, 1987h), Bourdieu and de Saint Martin (1982); on the scientific field (1981d, 1987e, forthcoming); on the

notion of field in a highly technical and precise sense which is perhaps partly hidden behind its common-sense meaning. Could you explicate in a few words where the notion comes from (for Americans, it is likely to evoke the “field theory” of Kurt Lewin), what you put under it and what its theoretical purposes are?

PB: To think in terms of field is to *think relationally*. The relational (rather than more narrowly “structuralist”) mode of thinking is, as Cassirer demonstrated in *Substanzbegriff und Funktionsbegriff*, the hallmark of modern science and one could show that it lies behind scientific enterprises apparently as different as those of Marx, of the Russian formalist Tyrianov, of Kurt Lewin, of Norbert Elias, and of the pioneers of structuralism in anthropology, linguistics and history, from Lévi-Strauss to Jakobson to Dumézil. (If you check, you will find that both Lewin and Elias draw explicitly on Cassirer, as I do, to move beyond the Aristotelian essentialism that spontaneously impregnates social thinking.) I could twist Hegel’s famous word and say that the real is the relational: what exist in the social world are relations, not interactions between agents or inter-subjective ties between individuals, but objective relations which exist “independently of individual consciousness and will,” as Marx said.

I define a field as a network, or a configuration, of objective relations between positions objectively defined, in their existence and in the determinations they impose upon their occupants, agents or institutions, by their present and potential situation (*situs*) in the structure of the distribution of species of power (or capital) whose possession commands access to the specific profits that are at stake in the field, as well as by their objective relation to other positions (domination, subordination, homology, etc.). Each field presupposes, and generates by its very functioning, the belief in the value of the stakes it offers.

juridical field and the field of power, Bourdieu (1986c, 1987g, 1981a, 1989a), Bourdieu and de Saint Martin (1978, 1982, 1987), respectively; the field of private housing construction is explored in Bourdieu et al. (1987).

In highly differentiated societies, the social cosmos is made up of a number of such relatively autonomous social micro-cosms, i.e., spaces of objective relations which are the site of a logic and of a necessity that is specific and irreducible to those which regulate other fields. For instance, the artistic field, or the religious field, or the economic field all follow specific logics: while the artistic field has constituted itself by refusing or reversing the law of material profit (Bourdieu 1983d), the economic field has emerged, historically, through the creation of a universe within which, as we commonly say, “business is business,” where the enchanted relations of *phylia*, of which Aristotle spoke, of friendship and love, are excluded.

LW: How does one determine the existence of a field and its boundaries, and what is the motor cause of its functioning?

PB: The question of the limits of the field is always at stake in the field. Participants to a field, say, economic firms, high fashion designers, or novelists, constantly work to differentiate themselves from their closest rivals in order to reduce competition and to establish a monopoly over a particular sub-sector of the field. Thus the boundaries of the field can only be determined by an empirical investigation. Only rarely do they take the form of juridical frontiers, even though they are always marked by more or less institutionalized “barriers to entry.” The limits of the field are situated at the point where the effects of the field cease.

The principle of the dynamics of a field lies in the form of its structures and, in particular, in the distance, the gaps, between the various specific forces that confront one another. The forces that are active in the field—and thus selected by the analyst as pertinent because they produce the most relevant differences—are those which define the specific capital. A capital does not exist and function but in relation to a field: it confers a power over the field, over the materialized or embodied instruments of production or reproduction whose distribution constitutes the very structure of the field, and over the regularities and the rules which define the

ordinary functioning of the field, and thereby over the profits engendered in this field.

As a space of potential and active forces, the field is also a *field of struggles* aimed at preserving or transforming the configuration of these forces. Concretely, the field as a structure of objective relations of force between positions undergirds and guides the strategies whereby the occupants of these positions seek, individually or collectively to safeguard or improve their position, and to impose the principle of hierarchization most favorable to their own products. The strategies of agents depend on their position in the field, that is, in the distribution of the specific capital.

LW: What difference is there between a field and an apparatus?

PB: An essential difference: struggles and thus historicity! The notion of apparatus is the Trojan horse of “pessimistic functionalism:” it is an infernal machine, programmed to accomplish certain purposes no matter what, when, or where. The school system, the State, the church, political parties or unions are not apparatuses but fields. In a field, agents and institutions constantly struggle, according to the rules constitutive of this space of game, with various degrees of strength and therefore diverse probabilities of success, to appropriate the specific products at stake in the game. Those who dominate in a given field are in a position to make it function to their advantage, but they must always contend with the resistance, “political” or not, of the dominated.

Now, under certain historical conditions, which must be examined, a field may start to function as an apparatus. When the dominant manage to crush and annul the resistance and the reactions of the dominated, when all movements go exclusively from the top down, the effects of domination are such that the struggle and the dialectic which are constitutive of the field cease. There is history only as long as people revolt, resist, act. Total institutions—asylums, prisons, concentration camps—or totalitarian states are attempts to institute an end to history. Thus apparatuses represent a pathological state, what we

may consider to be a limiting case, of fields.

LW: Very briefly, how does one conduct the study of a field, what are the necessary steps in this type of analysis?

PB: An analysis in terms of field involves three necessary and internally connected moments. Firstly, one must analyse the position of the field vis-a-vis the field of power. In the case of the “society” of artists and writers (Bourdieu 1983d), we find that the literary field is contained within the field of power where it occupies a dominated position. (In common, and much less adequate, parlance: artists and writers, or intellectuals more generally, are a “dominated fraction of the dominant class”). Secondly, one must map out the objective structure of the relations between the positions occupied by the agents or institutions who compete for the legitimate form of specific authority of which this field in the site. And, thirdly, one must analyze the habitus of agents, the system of dispositions they have acquired by internalizing a determinate type of social and economic condition and which find in a definite trajectory within the field under consideration a more or less favorable opportunity to become actualized.

The field of positions is methodologically inseparable from the field of stances or position-takings (*prises de position*), i.e., the structured system of practices and expressions of agents. Both spaces, that of objective positions and that of stances, must be analyzed together, treated as “two translations of the same sentence” as Spinoza put it. It remains nevertheless that, in situation of equilibrium, the space of positions tends to command the space of position-takings. Artistic revolutions, for instance, are but the result of transformations of the relations of power constitutive of the space of artistic positions which are themselves made possible by the meeting of the subversive intentions of a fraction of producers with the expectations of a fraction of the audience, thus by a transformation of the relations between the intellectual field and the field of power. Needless to say, what is true of the artistic field, applies to other fields. One can observe

the same “fit” between positions within the academic field on the eve of May 1968 and the political stances taken by the various protagonist of these events, as I show in *Homo Academicus*.

What must be emphasized is, firstly, that the external determinations that bear on agents situated in a given field (intellectuals, artists, politicians, or construction companies), never apply on them directly, but only through the specific mediation of the specific forms and forces of the field, after having undergone a *re-structuring* that is all the more important the more autonomous the field, that is, the more it is capable of imposing its specific logic, the cumulative product of its specific history. (This is what Baudelaire expressed when he exclaimed: “If there is one thing more abominable and worst than the bourgeois, it is the bourgeois artist”).

Secondly, we can observe a whole range of structural and functional *homologies* between the field of class relations, the political field, the literary field, etc.: each has its dominant and its dominated, its struggles for usurpation or exclusion, its mechanisms of reproduction, and so on. But every one of these characteristics takes on a specific, irreducible, form in each field (a homology may be defined as a resemblance within a difference). Thus, being contained within the field of power, the struggles that go on in the philosophical field, for instance, are always overdetermined and tend to function in a double logic. They have political effects and fulfill political functions by virtue of the homology of position that obtains between such a such a philosophical contender and such and such political or social group in the field of class relations.

To sum up, the chief merit of the notion of field, in my eyes, is that it allows us to transcend a whole series of methodological and theoretical antinomies: between internal reading, or tautegoric analysis as Schelling called it, and external or allegoric analysis; between efficient and final causes; between the individual and the society; between the normative discourse of celebration and the positive, or positivist, discourse, often animated by an iconoclast intent, which overlooks the specificity of

local determinations; and between the analysis of essence as the universalization of a given case and historicist immersion into particularity.

INTEREST, HABITUS, AND RATIONALITY

LW: Your use of the notion of interest has often called forth the charge of “economism” (e.g., Caillé 1981, 1987, Joppke 1986). What theoretical role does interest play in your mode of analysis?

PB: Building upon Weber, who utilized the economic model to develop a materialist sociology of religion and to uncover the specific interests of the great protagonists of the religious game, priests, prophets and sorcerers (Bourdieu 1987h), I introduced the notion of interest—I prefer to use the term *illusio* since I always speak of specific interest, of interests that are both presupposed and produced by the functioning of historically delimited fields—in my analysis of cultural producers in reaction to the dominant vision of the intellectual universe, to call into question the ideology of the *freischwebende Intelligenz*. The notion of interest as I use it, which, paradoxically, as you indicate, has brought forth the accusation of economism against a work which, from the very outset (I could refer here to my first ethnographic pieces on the sense of honor among the Kabyles [Bourdieu 1965 and 1979d]) was conceived in opposition to economism, is the means of a deliberate and provisional reductionism which allows me to bring the materialist mode of questioning into the cultural sphere from where it was expelled, historically, when the modern notion of art was invented and the field of cultural production won its autonomy (Bourdieu 1980h, 1987d).

This is to say that the concept of interest as I construe it has nothing in common with the naturalistic, trans-historical, and universal interest of utilitarian theory. (It would be otiose to show that Adam Smith’s self-interest is nothing more than an unconscious universalization of the form of interest required and engendered by a capitalist economy.) Far from being an

anthropological invariant, interest is a *historical arbitrary*, a historical construction that can be known only through historical analysis, *ex post*, through empirical observation, and not deduced a priori from some fictitious—and so naively Eurocentric—conception of “Man.”

LW: This would imply that there are as many “interests” as there are fields, that each field simultaneously presupposes and generates a specific form of interest that is incommensurable with those that have currency elsewhere.

PB: Absolutely. There are as many practical understandings of the game, and thus interests, as there are games. Each field calls forth and gives life to a specific form of interest, a specific *illusio* as tacit recognition of the value of the stakes of the game and as practical mastery of its rules. Furthermore, this specific interest implied by one’s participation in the game specifies itself according to the position occupied in the game (dominant vs. dominated, or orthodox vs. heretic) and with the trajectory that leads each participant to this position. Anthropology and comparative history show that the properly social magic of institutions can constitute almost anything as an interest, and as a realistic interest, i.e., as an investment (in the double meaning the word has in economics and in psychoanalysis) that is objectively paid back by an “economy.”

LW: Beyond interest and investment, you have “imported” from economic language a number of other concepts, such as market and capital (e.g., Bourdieu 1985d, 1986b), all of which evoke the economic mode of reasoning. What sets your theoretical approach apart from the “economic approach” to social action?

PB: The only thing I share with neo-marginalist economists are the words. Take the notion of investment. By investment I mean the propensity to act which is born out of the relation between a field and a system of dispositions adjusted to the game it proposes, a sense of the game and of its stakes which implies both an inclination and an ability to play the game. The general theory of the economy of

fields which emerges progressively from generalization to generalization (I am presently working on a multi-volume book in which I try to isolate, at a more formal level, the general properties of fields) enables us to describe and to specify the *specific form* taken by the most general mechanisms and concepts such as capital, investment, interest, within each field, and thus to avoid all kinds of reductionisms, beginning with economism, which recognizes nothing but material interest and the search for the maximization of monetary profit.

Thus my theory owes nothing, despite appearances, to the transfer of the economic approach. And, as I hope to demonstrate fully one day, far from being the founding model, economic theory (and Rational Action Theory which is its sociological derivative) is probably best seen as a particular instance, historically dated and situated, of field theory.

LW: Would the notion of habitus be the conceptual lynchpin by which you rearticulate these apparently economic notions into a model of action that is radically different from that of economics?

PB: In double opposition to the objectivism of action “without an agent” of the Althusserians and to the subjectivism which portrays action as the deliberate pursuit of a conscious intention, the free project of a conscience positing its own ends and maximizing its utility through rational computation, I have put forth a theory of practice as the product of a *practical sense* (Bourdieu 1980a), of a socially constituted “sense of the game.” Against positivistic materialism, the theory of practice as practice posits that objects of knowledge are *constructed*, and not passively recorded. And against intellectualist idealism, it reminds us that the principle of this construction is habitus, the system of structured and structuring dispositions which is constituted by practice and constantly aimed at practical—as opposed to cognitive—functions.

In order to sidestep objectivism without relapsing into subjectivism and its demonstrated incapacity to account for the necessity immanent in the social world, it is necessary to return to practice as the locus

of the dialectic between *opus operatum* and *modus operandi*, between the objectified and the embodied products of historical action, structures and habitus.

I could show that the concept of habitus, like that of field, is relational in that it designates a mediation between objective structures and practices. First and foremost, habitus has the function of overcoming the alternative between consciousness and the unconscious and between finalism and mechanicalism. Following the programme suggested by Marx in the *Theses on Feuerbach*, it aims at making possible a materialist theory of knowledge which does not abandon to idealism the idea that all knowledge, be it mundane or scholarly, presupposes a work of construction, but a work which has nothing in common with intellectual work, a practical activity which sets into motion the practical *ars inveniendi* of habitus. (All those who used this old concept or similar ones before me—from Hegel's *ethos* to Husserl's *Habitualität* to Mauss's *hexis*—were inspired by a theoretical intention akin to mine, which was to escape from under the philosophy of the subject without doing away with the agent).

In order to capture the gist of social action, we must recognize the *ontological complicity*, as Heidegger and Merleau-Ponty suggested, between the agent (who is neither a subject or a consciousness, nor the mere executant of a role or the *Träger* of a function) and the social world (which is never a mere "thing" even if it must be constructed as such in the objectivist phase of research). Social reality exists, so to speak, twice, in things and in minds, in fields and in habitus, outside and inside of agents. And when habitus encounters a social world of which it is the product, it finds itself "as fish in water," it does not feel the weight of the water and takes the world about itself for granted.

LW: All of this puts you in a frontal opposition to this wide, if heterogenous, current that has recently been gaining strength across the social sciences under the label of Rational Action Theory or Rational Choice Theory.

PB: Without the shadow of a doubt.

Forgetting all the abstractions it has to effect in order to produce its theoretical artefact, Rational Action Theory (RAT) typically substitutes the scientist for the practical habitus. It slips from the model to the reality and does as if the action that its model accounts for had this model as its principle. The social actor of RAT is nothing but the imaginary projection of the *sujet savant* (knowing subject) into the *sujet agissant* (acting subject).³²

Note also that this "imaginary anthropology" has nothing to tell us about the social genesis of historically varying forms of interests since it postulates *ex nihilo* the existence of a universal, preconstituted interest. Just as it ignores the individual and collective *history* of agents through which structures are formed and reproduced and which "live" in them. In reality, far from being posited as such in an explicit, conscious project, the strategies suggested by habitus as a "feel for the game" aim, on the mode of "protension" so well characterized by Husserl in *Ideen*, towards the "objective potentialities" immediately given in the immediate present. Must we talk of "strategy," then? The word is strongly associated with the intellectualist and subjectivist tradition which, from Descartes to Sartre, has dominated Western philosophy and which is now again on the upswing with RAT, a theory so well-suited to satisfy the spiritualist *point d'honneur* of intellectuals. This is not a reason not to use it, however, with a totally different theoretical intention, to designate the objectively orientated lines of action which social agents continually construct.

Moreover, the theory of habitus explains why the finalism of Rational Choice Theory, although anthropologically false, may appear as empirically sound. Individualist finalism, which conceives action as determined by the conscious aiming at explicitly posed goals, is a well-founded illusion: the sense of the game which implies an anticipated adjustment of habitus to the necessities and to the probabilities inscribed in the field does present itself under the

³² See Bourdieu (1980a, pp. 71–86) for a thorough critique of Sartrean phenomenology and Elster's brand of Rational Choice Theory along these lines.

appearance of a successful “aiming at” a future. Likewise, the structural affinity of habituses belonging to the same class is capable of generating practices that are convergent and objectively orchestrated outside of any collective “conspiracy” or consciousness. In this fashion it explains many of those phenomena of quasi-teleology which can be observed in the social world, such as those forms of collective action or reaction which pose such insuperable dilemmas to RAT.

But the efforts of the proponents of some or other version of Rational Action Theory remind me of Tycho Brahé trying to salvage the Ptolemaic paradigm after Copernicus: it is the anthropological postulates of RAT concerning the nature of social action that are, in my view, irretrievably flawed. Both the kind of finalism represented by RAT, which wants to see nothing but choice (if under constraints: limited rationality, irrational rationality, “weakness of the will,” etc., the variations are endless—here again, anyone who recalls Sartre’s analysis of bad faith or of oaths will quickly recognize the intellectual contortions of an Elster [1984b] in *Ulysses and the Sirens* as the mediocre remake of a well-known show), and the mechanistic determinism taken to its extreme by structural Marxists equally mutilate the intrinsically double reality of human existence as a thing of the world for which there are things, a fundamental anthropological reality that Pascal captured brilliantly when he said: “*Le monde me comprend et m’anéantit comme un point mais je le comprends*” (in short, the world encompasses me but I understand it).

The proper object of social science, then, is neither individuals, this *ens realissimum* naively crowned as the paramount, rock-bottom reality by all “methodological individualists,” nor groups as sets of concrete individuals sharing a similar location in social space, but the *relation between two realizations of historical action*, in bodies (or biological individuals) and in things. It is the double and obscure relation between habitus, i.e., the durable and transposable system of schemes of perception, appreciation, and action that result from the institution of the social in the

body, and fields, i.e., systems of objective relations which are the product of the institution of the social in things, or in mechanisms that have the quasi-reality of physical objects; and, of course, of everything that is born out of this relation, that is, social practices and representations, or fields as they present themselves in the form of realities perceived and appreciated.

LW: What is the nature of this relationship of “ontological complicity” between habitus and field and how does it work itself out more precisely?

PB: The relation between habitus and field operates in two ways. On one side, it is a relation of *conditioning*: the field structures the habitus, which is the product of the embodiment of the immanent necessity of a field (or of a hierarchically intersecting set of fields). On the other side, it is a relation of knowledge or *cognitive construction*: habitus contributes to constituting the field as a meaningful world, a world endowed with sense and with value, in which it is worth investing one’s energy. Two things follow: firstly, the relation of knowledge depends on the relation of conditioning that precedes it and fashions the structures of habitus; secondly, social science is necessarily a “knowledge of a knowledge” and must make room for a sociologically grounded phenomenology of the primary experience of the field or, to be more precise, of the invariants and variations of the relation between different types of fields and different types of habitus.

In short, the specificity of social science lies in the fact that its object of knowledge is a reality which includes agents who have this very reality as an object of knowledge. The task becomes, then, to construct a theory of practice *as practice* and a theory of the practical mode of knowledge that is implied in it. Thus, if it is indispensable to break with the spontaneous knowledge of the social world, it is no less necessary to include in our theory the practical knowledge against which scientific knowledge is constructed and which continues to orient practices. The relation of practical knowledge is not that between a subject and an object constituted as such and perceived as

a problem. *Habitus* being the social incorporated, it is “at home” in the field it inhabits, it perceives it immediately as endowed with meaning and interest. Practical action may be described by analogy with the *orthé doxa* of Plato in *Meno*, as the “right opinion:” the coincidence between dispositive and position, between the “sense of the game” and the game, explains that the agent does “what he or she has to do” without posing it explicitly as a goal, below the level of calculation and even consciousness, beneath discourse and representation.

The theory of *habitus*, again, allows us to overcome a whole series of antinomies into which the theory of action routinely locks itself, those of consciousness and the “thingness” of social facts, of mechanicalism and finalism, of subjective teleology (as in all so-called theories of “rational choice”) and objective teleology (which personalizes collectives, “the State,” the “Bourgeoisie,” etc., and endows them with intentions and projects).

LW: Does the theory of *habitus* rule out strategic choice and conscious deliberation as one modality of action?

PB: Not at all. The immediate fit between *habitus* and field is only one modality of action, if the most prevalent one (“We are empirical,” said Leibniz, by which he meant practical, “in three quarters of our action”). The lines of action suggested by *habitus* may very well be accompanied by a strategic calculation of costs and benefits which tends to carry out at a conscious level the operations which *habitus* carries out in its own way. Rational choice may even become a *métier*, a profession, as in the trade of the historian, the economist, or the scientist. Times of crises, in which the routine adjustment of subjective and objective structures is brutally disrupted, constitute a class of circumstances when indeed “rational choice” often appears to take over. But, and this is a crucial proviso, it is *habitus* itself that commands this option. We can always say that individuals make choices, as long as we do not forget that they do not choose the principle of these choices.

LANGUAGE, PHILOSOPHY, AND SYMBOLIC DOMINATION

LW: In *Language and Symbolic Power* (Bourdieu 1982b, 1989b), you develop a sweeping critique of structural linguistics, or what one might call the “pure” study of language. You put forth an alternative model which, to simplify greatly, makes language an instrument or a medium of power relations, rather than simply a means of communication, that must be studied within the interactional and structural contexts of its production and actualization. Could you summarize the gist of this critique?

PB: What characterizes “pure” linguistics is the primacy it accords to the synchronic, internal, structural perspective over the historical, social, economic, or external, determinations of language. I have sought, especially in *Le sens pratique* (Bourdieu 1980a, pp. 51–70), to draw attention to the relation to the object and to the theory of practice implicit in this perspective. The Saussurian point of view is that of the “impartial spectator” who seeks understanding as an end in itself and thus leads to impute this “hermeneutic intention” to social agents, to construe it as the principle of their practices. It takes up the posture of the grammarian, whose purpose is to study and codify language, as opposed to that of the orator, who seeks to act in and upon the world through the performative power of the word. Thus by treating it as an *object* of analysis rather than using it to think and to speak with, it constitutes language as a *logos*, by opposition to a *praxis*, as a *telos* without practical purpose or no purpose other than that of being interpreted, in the manner of the work of art.

This typically scholastic opposition is a product of the scholarly apperception and situation—another case of the scholastic fallacy we talked about earlier. This scholarly *epoché* neutralizes the functions implied in the ordinary usage of language. Language according to Saussure, or in the hermeneutic tradition, is constituted into an instrument of intellection and into an object of analysis, a dead language (written and foreign as Bakhtine points out), a self-

contained system completely severed from its real uses and denuded from its practical—and political—functions (as in Fodor's and Katz's pure semantics). The illusion of autonomy of the "purely" linguistic order which is asserted by the privilege granted the internal logic of language at the expense of the social conditions of its timely usage opens the way to all subsequent theories which proceed as if the theoretical mastery of the code sufficed to confer practical mastery of socially appropriate usages.

LW: By that, do you mean to assert, contrary to the claims of structural linguistics, that the meaning of linguistic utterances cannot be derived, or deduced, from the analysis of their formal structure?

PB: Yes, and to put it more strongly, that grammaticality is not the necessary and sufficient condition of the production of meaning, as Chomsky (1967) would lead us to believe by overlooking the fact that language is made not for linguistic analysis, but to be spoken and to be spoken *à propos*. (The Sophists used to say that what is important in learning a language is to learn the appropriate moment, *kairos*, for saying the appropriate thing.) All the presumptions, and all the subsequent difficulties, of all structuralisms (and this is true both of anthropology and sociology) are contained in nutshell in this initial operation which reduces the speech act to a mere execution. It is this primeval distinction between language and its realization in speech, that is in practice and in history, which is at the root of the inability of structuralism to think the relation between two entities other than as the model and its execution, essence and existence, and which amounts to putting the scientists, keeper of the model, in the position of a Leibnizian God to whom the objective meaning of practices is given.

In challenging this posture, I am trying to recover the lost foundations of linguistic exchanges and, again, to overcome the shortcomings of both the economic and the purely linguistic analysis of language. What is it that they both forget? Essentially, to sum up a long and difficult demonstration in one sentence, that linguistic relations

are always relations of power (*rappports de force*) and, consequently, cannot be elucidated within the compass of linguistic analysis alone.³³ Even the simplest linguistic exchange brings into play a complex and ramifying web of historical power relations between the speaker, endowed with a specific social authority, and an audience, which recognizes this authority to varying degrees, as well as between the groups to which they respectively belong. What I have sought to show is that a very important part of what goes on in verbal communication, even the content of the message itself, remains unintelligible as long as one does not take into account the totality of the structure of power relations that underlay the exchange.

Let me take a simple example, that of communication between settlers and natives in a colonial or post-colonial context. The first question that arises, and one typically overlooked by linguists, is: what language will they use? Will the dominant embrace the language of the dominated as a token of his newly-found concern for equality? If he does, there is a good chance that this will be done through what I call a strategy of condescension (cf. Bourdieu 1984a, pp. 472–473): by temporarily but ostentatiously abdicating his dominant position in order to "reach down" to his interlocutor, the dominant profits from this relation of domination, which continues to exist, by denying it. Symbolic denegation (in the Freudian sense of *Verneinung*), i.e., the fictitious bracketting of the relation of power, exploits this relation of power in order to produce the recognition of the relation of power that abdication elicits. Let us turn now to the situation, which in fact is by far the most frequent one, where it is the dominated who is obliged to adopt the language of the dominant—and here the relation between standard, white English and black American provides a paradigm. In this case, the dominated speaks a *broken language*, as William Labov (1973) has shown, and his linguistic capital is more or less completely devalued,

³³ See Bourdieu and Boltanski (1975), Bourdieu (1975a, 1977c, 1983b) and Bourdieu (1980b, pp. 95–112, 121–142) for further developments.

be it in school, at work, or in social encounters. What conversation analysis leaves out too easily, in this case, is that every linguistic interaction between whites and blacks is constrained by the encompassing structural relation between their respective appropriations of English, and by the power imbalance which sustains it and gives the arbitrary imposition of “white” English its air of naturalness.

To push this analysis further, one would need to introduce all kinds of positional coordinates, such as gender, level of education, class origins, residence, etc. All these variables intervene at every moment in the determination of the objective structure of “communicative action,” and the form taken by linguistic interaction will hinge substantially upon this structure, which is unconscious and works almost wholly “behind the backs” of locutors. In short, if a French person talks with an Algerian, or a black American to a WASP, it is not two persons who speak to each other but, through them, the colonial history in its entirety, or the whole history of the economic, political, and cultural subjugation of blacks (or women, or workers, etc.) in the United States.

LW: You also denounce the “illusion of linguistic communism” (Bourdieu and Boltanski 1975) according to which the social competence to speak is equally given to all.

PB: Any discourse is the product of the encounter of a *linguistic habitus*, that is, a competence at once technical and social, and a *market*, i.e., a system of relations of force which determine the price of linguistic products and thus helps fashion linguistic production. The anticipation of the price that my discourse will fetch contributes to determining the shape and content of my discourse, which will be more or less “tense,” more or less censored, sometimes to the point of annulment—as in the silence of intimidation.

This means that not all linguistic utterances are equally acceptable and not all locutors equal. Saussure says that language is a “treasure” and he describes the relation of individuals to language as a sort of democratic participation to the common

treasure. The illusion of “linguistic communism” is the illusion that everyone participates in language as they enjoy the sun, the air or water—in a word, that language is not a rare good. In fact, access to language is quite unequal and the theoretically universal competence liberally granted to all by linguists is in reality monopolized by some. Certain categories of locutors are deprived of the capacity to speak in certain situations (and often acknowledge this deprivation in the manner of this agriculturalist who explained that he never thought of running for mayor of his small township by saying: “But I don’t know how to speak!”).

Inequalities of linguistic competence reveal themselves on the market of daily interactions, that is, in the chatter between two persons, in a public meeting, in a seminar, and on the radio or TV. Competence effectively functions differentially and there are monopolies on the market of linguistic goods just as on the market of economic goods. This is most visible in politics, where spokespersons, being granted a monopoly over the legitimate political expression of the will of a collective, speak not only in favor of those whom they represent but also in their place (Bourdieu 1985b, 1981a).

LW: Your analysis of language, then, is not an accidental “incursion” into the domain of linguistics but, rather, an extension, to a new empirical realm, language and speech, or discursive practices more generally (including those of linguists), of the method of analysis you have applied to other cultural products.

PB: Yes. I think that the division between linguistics and sociology is unfortunate and deleterious to both disciplines. I have spent my entire life fighting such arbitrary boundaries, which are pure products of academic reproduction and have no epistemological foundation whatsoever, between sociology and anthropology, sociology and history, sociology and linguistics, the sociology of art and the sociology of education, the sociology of sport and the sociology of culture, etc. Here again is a situation where “tres-

passing,” as Albert Hirschman would say, is a prerequisite for scientific advance.

LW: If I could try to summarize what you are saying: the meaning and social efficacy of a message is only determined within a given field (e.g., journalism or philosophy), itself nested in a network of hierarchical relations with other fields (such as the field of power, of law, of class relations, etc.). Without an understanding of the full structure of objective relationships that define positions in this field, of the specific forms of censorship they imply, and without knowledge of the trajectories and linguistic dispositions of those who occupy them, it is impossible to fully explicate processes of communication, why something is said or not said, by whom, what is meant, what is understood, and with what effects, i.e., what can be “done with words,” to borrow Austin’s (1962) formula.

PB: This is exactly what I tried to demonstrate in my study on *The Political Ontology of Martin Heidegger* (Bourdieu 1975c and 1988b). Indeed, it is the logic of my research on language and on the notion of field which led me to concern myself with Heidegger. The work of Heidegger (with which I became intimately familiar very early on, at a time of my youth when I was preparing a book on the phenomenology of affective life and of temporal experience) appeared to me as a “strategic research site,” to use Merton’s expression, to verify my hypotheses on the effect of censorship exerted by fields of cultural production: Heidegger is a master—I am inclined to say, *the* master—of double talk or, if you wish, of polyphonic discourse. He manages to speak simultaneously in two keys, that of scholarly philosophical language and that of ordinary language. This is particularly visible in the case of the apparently “pure” concept of *Fürsorge* which plays a central role in the Heideggerian theory of time and which, in the expression *soziale Fürsorge*, social security, refers to the political context and to the condemnation of the welfare state, of paid vacations, of health insurance, etc. But Heidegger interested me also as the exemplary incarnation of the “pure philosopher” and I wanted to show, in what was

apparently the most unfavorable case for the sociology of cultural works as I conceive it, that the method of analysis I propose could not only account for the sociopolitical conditions of production of the work but also lead to a better understanding of the work itself, that is, in this case, of the central thrust of Heideggerian philosophy, namely, the ontologization of historicism.

This being said, I used the controversy which recently erupted around the work of Heidegger,³⁴ and in which certain philosophers (Lacoue-Labarthe and Lyotard notably) displayed more clearly than ever before their profound political irresponsibility, to highlight the politically ambiguous implications of a certain way of conceiving philosophy which has spread in France since the 1960s: a vision of philosophy, especially through the exaltation of the works of Nietzsche or Heidegger, that leads to an *aestheticism* of transgression, to a “radical chic,” as some of my American friends put it, that is extremely ambiguous intellectually and politically. Under this angle, my work—I think in particular of *L’amour de l’art* (Bourdieu et al. 1966)³⁵ or *Distinction*—stands as the very antithesis of the supreme philosophical role which, since Sartre, has always entailed an aesthetic dimension: the critique, not of culture, but of the *social uses of culture as a capital and an instrument of symbolic domination*, is incompatible with the aestheticist entertainment often concealed behind a scientific front, as in Barthes or *Tel Quel* (not to mention even more trivial manifestations such as Baudrillard’s *Cool Memories*), of those French philosophers who have taken the degree of aestheticization of philosophy to a degree hitherto unequalled. Derrida is, on this point, no

³⁴ The publication of Farias’ (1987) study documenting Heidegger’s support of and involvement in Nazi politics triggered a heated and politically charged intellectual controversy into which all the “heavyweights” of the French intellectual field were drawn. It was the occasion of a vigorous exchange between Derrida and Bourdieu in the pages of the daily *Libération*. For a sample of this debate in France and in Germany, see Davidson (1989).

³⁵ Bourdieu’s work on the social production and uses of art also includes Bourdieu, Boltanski, Castel and Chamboredon (1965), and Bourdieu (1968a, 1971c, 1980h, 1985d, 1987d).

doubt the most skilled and the most ambiguous insofar as he manages to give the appearance of a radical break to those semi-ruptures which extend the game of iconoclast destruction into the realm of culture. His analyses always stop short of the point where they would fall into "vulgarity," as I showed in the post-scriptum of *Distinction* (1984a, pp. 485–500); situating himself both inside and outside the game, on the field and on the sideline, he plays with fire by brushing against a genuine critique of philosophical imposture without ever completing it, and for good reason.

Thus the "Heidegger affair" was for me an opportunity to show that philosophical aestheticism is rooted in a social aristocratism which is itself at the base of a contempt for the social sciences that is highly unlikely to facilitate a realistic vision of the social world and which, without necessarily determining political "mistakes" as monstrous as Heidegger's *grosse Dummheit*, have very serious implications for intellectual life and, indirectly, for political life. It is no happenstance if the French philosophers of the sixties, and in particular Derrida and Foucault, whose philosophical project was formed in a fundamentally ambivalent relation with the "human sciences" and who never fully repudiated the privileges of caste associated with the status of philosopher, have given a new life, throughout the world but especially in the United States, to the old philosophical critique of the social sciences and fueled, under the cover of "deconstruction" and the critique of "texts," a thinly-veiled form of irrationalist nihilism.

LW: Your analysis of Heidegger, and of the social production and functioning of philosophical discourse more generally,³⁶ thus presupposes, and calls forth, an analysis of the objective position of sociology in relation to philosophy.

PB: Since the second half of the 19th century, European philosophy has constantly defined itself in opposition to the

social sciences, against psychology and against sociology in particular, and through them, against any form of thought that is explicitly and immediately directed at the "vulgar" realities of the social world. The refusal to derogate by studying objects deemed inferior or by applying "impure" methods, be it statistical survey or even the simple historiographic analysis of documents, castigated at all times by philosophers as "reductionist," "positivist," etc., goes hand in hand with the refusal to plunge into the fleeting contingency of historical things that prompts those philosophers most concerned by their statutory dignity always to return (often through the most unexpected routes, as Habermass testifies today), to the most "universal" and "eternal" thought.

A good number of the specific characteristics of French philosophy since the 60s can be explained by the fact that, as I demonstrate in *Homo Academicus*, the university and intellectual field came, for the first time, to be dominated by specialists in the human sciences (led by Lévi-Strauss, Dumézil, Braudel, etc.). The central focus of all discussions at the time shifted to linguistics, which was constituted into the paradigm of all the human sciences, and even of such philosophical enterprises as Foucault's. This is the origin of what I have called the "-logy effect" to designate the desperate efforts of philosophers to borrow the methods, and to mimic the scientificity, of the social sciences without giving up the privileged status of the "free thinker:" thus the literary semiology of Barthes (not to mention Kristeva and Sollers), the archeology of Foucault, the grammatology of Derrida, or the attempt of the Althusserians to pass the "pure" reading of Marx off as a self-sufficient and self-contained science (cf. Bourdieu 1975b).

THE REFUSAL OF "THEORETICAL THEORY"

LW: Since we are talking "theory," let me bring up a puzzle. You are frequently billed, and certainly read, as a "social theorist" (and, as you well know, this is a very definite type in the gallery of possible

³⁶ Further analyses of philosophy as an institution and as a discourse are found in Bourdieu (1975a, 1975b, 1975c, 1980f, 1982b, 1983a, and 1983c).

sociological personas in the United States). Yet I keep being struck by how seldom, in your work, you make purely “theoretical” statements or remarks. Instead, you keep referring to particular research problems and mundane dilemmas you encountered while gathering, coding, or analyzing data, or thinking through a substantive issue. Even in your research seminar at the Ecole des Hautes Etudes en Sciences Sociales in Paris, you warn your audience upfront that they shall not get from this course “neat presentations on habitus and field.” You are also extremely reluctant to discuss the concepts that you have coined and use in your work in isolation from their empirical supports. Could you explicate the place that theory occupies in your work?

PB: Let me say outright and very forcefully that I never “theorize,” if by that we mean engage in the kind of conceptual gobbledygook (*laius*) that is good for textbooks and which, through an extraordinary misconstrual of the logic of science, passes for Theory in much of Anglo-American social science. I never set out to “do theory” or to “construct a theory” *per se*, as the American expression goes. And it is a complete misapprehension of my project to believe that I am attempting some kind of “synthesis of classical theory” à la Parsons. There is no doubt a theory in my work, or, better, a set of *thinking tools* visible through the results they yield, but it is not built as such.

The ground for these tools—the notion of cultural capital,³⁷ for instance, that I invented in the early 60s to account for the fact that, after controlling for class origins, students from more cultured families have not only higher rates of academic success but exhibit different modes and patterns of cultural consumption and expression in a wide gamut of domains—lies in research, in the practical problems and puzzles encountered and generated in the effort to construct a phenomenally diverse set of objects in such a way that they can be

treated, thought of, comparatively or, more precisely, analogically. The thread which leads from one of my works to the next is the *logic of research*, which is in my eyes *inseparably* empirical and theoretical. I readily confess that I feel very little in common with the kind of rhetorical exercises in “theoretical theory” that are so common on your side of the Atlantic.

LW: What is the difference between “theoretical theory” and scientific theory as you conceive it?

PB: For me, theory is not a sort of prophetic or programmatic discourse which originates by dissection or by amalgamation of other theories for the sole purpose of confronting other such pure “theoretical theories.” (I need not give examples of these endless and unassailable “conceptual melting pots” of neologisms, refurbished categories, and pseudo-theorems, generally closed by a call for future research or empirical application, preferably by others—Glaser and Strauss [1967] speak somewhere of “theoretical capitalists,” perhaps rentiers would be a better image—whose paradigm remains, a decade after his death, Parsons’ AGIL scheme that some today are trying to resurrect.) Rather, scientific theory as I conceive it emerges as a program of perception and of action—a scientific habitus, if you wish—which is disclosed only in the empirical work which actualizes it. It is a *temporary construct which takes shape for and by empirical work*.³⁸ Consequently, it has more to gain by confronting new objects than by engaging in theoretical polemics that do little more than fuel a perpetual, self-sustaining, and too often vacuous meta-discourse around concepts treated as intellectual totems. There is nothing more sterile than epistemology or theory when it becomes a topic for society conversation and a substitute for research.

To treat theory as a *modus operandi* which practically guides and structures scientific practice obviously implies giving up the somewhat fetishistic accommodativeness that “theoreticians” usually establish

³⁷ See Bourdieu (1979a) on the “three forms” (embodied, objectified and institutionalized) of cultural capital, and Bourdieu (1986b) on the relations between cultural, social, economic, and symbolic capital.

³⁸ See Bourdieu and Hahn (1970) and Bourdieu et al. (1968, part I) for elaborations.

with it. It is for this reason that I never felt the urge to retrace the genealogy of the concepts I have coined or reactivated, like those *habitus*, *field*, or *symbolic capital*. Not having been born of theoretical parthogenesis, these concepts do not gain much by being resituated *vis-à-vis* previous usages. Their construction and use emerged in the practicalities of the research enterprise and it is in this context that they must be evaluated. The function of the concepts I employ is first and foremost to designate, in stenographic manner, within the research procedure, a theoretical stance, a principle of methodological choice, negative as well as positive. Systematization necessarily comes *ex post*, as fruitful analogies emerge little by little, as the useful properties of the concept are successfully tried and tested.³⁹

Unfortunately, the socially dominant model of sociology today is still predicated on a clear-cut distinction, and a practical divorce, between research (I think here in particular of this “science without a scientist” epitomized by public opinion research and of this scientific monster called “methodology”) and the “theory without object” of pure theoreticians, presently exemplified by the trendy, and mostly empty, discussion raging around the so-called “micro-macro link” (e.g., Alexander et al. 1987). This opposition between the pure theory of the *lector* devoted to the hermeneutic cult of the scriptures of the founding fathers (if not of his own writings), on the one hand, and survey research and methodology on the other is an entirely *social* opposition. It is inscribed in the institutional and mental structures of the sociological profession, rooted in the academic distribution of resources, positions, and competencies, as when whole schools (e.g., conversation analysis or

status attainment research) are based almost entirely on one particular method, and reinforced by the political demand for instruments of rationalization of social domination—and it must be rejected. I could paraphrase Kant and say that research without theory is blind and theory without research is empty.

The trick, if I may call it that, is to manage to combine immense theoretical ambition with extreme empirical modesty. The *summum* of the art, in social science, is, in my eyes, to be capable of engaging very high “theoretical” stakes by means of very precise and often very mundane empirical objects. We tend too easily to assume that the social or political importance of an object suffices in itself to grant importance to the discourse that deals with it. What counts, in reality, is the rigor of the *construction* of the object. I think that the power of a mode of thinking never manifests itself more clearly than in its capacity to constitute socially insignificant objects into scientific objects (as Goffman did of the minutiae of interaction rituals)⁴⁰ or, what amounts to the same thing, to approach a major socially significant object in an unexpected manner—something I am presently attempting by studying the effects of the monopoly of the state over the means of legitimate symbolic violence by way of a very down-to-earth analysis of what a certificate (of illness, invalidity, schooling, etc.) is and does. For this, one must learn how to translate very abstract problems into very concrete scientific operations.

PROGRESS AND PROSPECTS FOR SOCIOLOGY

LW: In a paper published in 1968 in *Social Research* (Bourdieu and Passeron 1968, p. 212), you expressed the hope that, “just as American sociology was able, for a time, by its empirical rigor, to act as the scientific bad conscience of French sociology,” French sociology might, “by its theoretical stringency, become the philosophical bad con-

³⁹ For instance, it is only after utilizing the notion of “social capital” for a good number of years and in a wide variety of empirical settings, from the matrimonial relations of peasants to the symbolic strategies of research foundations to designers of high fashion to alumni associations of elite schools (see, respectively, Bourdieu 1977b, 1980a, 1980b, 1981b; Bourdieu and Delsaut 1975), that Bourdieu wrote a paper outlining some of its generic characteristics (Bourdieu 1980c).

⁴⁰ See the eulogy written by Bourdieu (1983) for *Le Monde* upon Goffman’s sudden death.

science of American sociology.” Twenty years later, where does this wish stand?

PB: I think that it is the very distinction between theory and research implied by this statement that must be challenged. If French sociology is to become the scientific bad conscience of American sociology, then it must succeed in overcoming this separation by putting forth a new form of scientific practice founded at once upon a greater theoretical exigency and upon greater empirical rigor. The programme of work that I recently completed on French elites schools in the field of power attempts, in its own partial way, to contribute to the maturing of such a form of research. In the book entitled *The State Nobility* (Bourdieu 1989a) which grew out of it, I try to bring together the results of nearly 20 years of in-depth investigations, not of one but of some twenty Grandes Ecoles and of some 200 corporations and their CEOs, based on surveys, direct observation, interviews of students, archival documents, etc.; a reflection on methods, including the problem of theoretical sampling; a phenomenology of the experience of being selected in or out of the elite; and a structural theory of modes of reproduction. Of course, I have no illusions that this work reaches all the lofty goals I just set but I believe that it does represent a genuine attempt at truly marrying theoretical and empirical rigor.

LW: In what sense can we speak of progress then? Can we say that sociology has moved forward, or are we still battling with the same evils of Grand Theory and Abstracted Empiricism as C.-Wright Mills (1959) expressed it in the late 1950s?

PB: Instead of progress, I would rather speak of obstacles to progress, and of means of overturning these obstacles. There is undoubtedly progress, and sociology is a considerably more advanced science than observers, even its practitioners, are willing to grant. The reasons for this distrust of the scientific status of sociology are more social than epistemological: a truly scientific sociology, that is, a science of society that rejects the social demand for legitimation or manipulation, is a practice that is highly improbable

sociologically speaking—and perhaps more so in the United States than in many other countries. Sociology is an especially difficult science because it uncovers things that are hidden and sometimes even repressed, and because its objects are the stakes of struggles in social reality itself.⁴¹ Sociology denaturalizes, and thereby de-fatalizes the world, and the knowledge it produces is liable to exert a political efficacy every time it reveals the laws of functioning of mechanisms that owe part of their own efficacy to being misrecognized, i.e., every time it reaches into the foundations of symbolic violence.

I have repeated often that one of the necessary conditions for progress is the autonomy of the scientific field (Bourdieu, 1981d, forthcoming). But this does not mean that each national sociology must remain aloof, on the contrary. We need to engage in a collective reflection on the *institutional conditions of rational communication* in the social sciences. (It is an opportunity for such a reflection that I sought to promote in accepting to organize, along with James Coleman, the conference on “Social Theory and Emerging Issues in a Changing Society” to be held at the University of Chicago in April of 1989). What social scientists on both sides of the Atlantic must do is work to build and strengthen institutional mechanisms against isolationism, against all forms of scientific intolerance, mechanisms capable of promoting fair communication and a more open confrontation of ideas, theories, and paradigms. More than the positive and negative developments which have taken place in each national sociology in the last twenty years, what matters is the establishment of relations between American and Continental social scientists that make possible a greater unification of the field of world sociology and, most importantly, a *unification respectful of diversity*.

If there exist, *pace* Habermas, no trans-historical universals of communication, there certainly exists forms of social organization of communication that are liable to foster the production of the universal. We

⁴¹ See especially “Une science qui dérange” and “Le sociologue en question” in Bourdieu (1980b, pp. 19–60) for an elaboration of this point.

cannot rely on moral exhortation to abolish “systematically distorted” communication from sociology. Only a true *Realpolitik* of scientific reason can contribute to transforming structures of communication by helping to change both the modes of functioning of those universes where science is produced and the dispositions of the agents who compete in these universes, and thus the institution that contributes most to fashion them, the University.

LW: Isn't one of the conditions of scientific progress, then, to be capable of liberating oneself from the constraints of traditions of thought (and especially national traditions), which in turn presupposes a kind of “anti-nomic attitude” towards one's discipline: on the one hand you need concepts and theories to construct objects, thus you need to absorb and trust its heritage. But, on the other hand, these intellectual tools themselves are already (pre)constructions that carry over the accepted wisdom of our predecessors and create blinders which may hide as much as they reveal.

PB: Indeed, the sociologist is inescapably and endlessly faced with a sort of *double bind*, strapped in a Catch-22 situation of this sort. Without the intellectual instruments he owes his scholarly tradition, he or she is nothing more than an amateur, a self-taught, spontaneous sociologist—and certainly not the best equipped of all lay sociologists, given the generally limited span of the social experiences of academics. But, at the same time, there is the ever-present danger that he will simply substitute to the naive *doxa* of lay common sense the no less naive *doxa* of scientific common sense which parrots, in the technical jargon and under the official trappings of scientific discourse, the discourse of common sense, which retranslates it in this terrible, half-concrete, half-abstract *linguo* that his training and the censorship of the sociological establishment impose on him.

It is not easy to escape the horns of this dilemma, this alternative between the disarmed ignorance of the autodidact devoid of instruments of rigorous scientific construction and this *half-science* which unknowingly accepts categories of perception directly borrowed from the social

world. It is the task of research pedagogy to make students acutely aware of this double bind and to train them to resist its negative effects. (In this respect, I rest convinced that one of the chief obstacles to progress in the social sciences today lies in the ordinary teaching of sociology, and graduate students are no doubt its number one victim.) And it is the role of the reflexive return, of the social history of scientific practices, in a word, the objectivation of tools of objectivation, to remind us of it.

This being said, the social dispositions one brings into academia evidently play a crucial role here. Those best armed to avoid this dilemma are people who bring together an advanced mastery of scientific culture with a certain revolt against, or distance from, this culture (often rooted in an estranged experience of the academic universe which pushes one ‘not to “buy it” at face value), or, quite simply, a *political* sense which intuitively leads one to reject or to resist the aseptized and derealized vision of the social world offered by the socially dominant discourse in sociology.⁴² Needless to say, the more you consciously command the principles that lead you to challenge the accepted preconceptions of an intellectual tradition, the greater your chances of fully mastering your own thought and scientific products—in sum, to be the true *subject* of the problems that can be posed about the social world.

LW: Since you evoked the process of becoming a sociologist, perhaps I could bring this conversation to a close by asking you a more practical question: what advice would you give to young, aspiring sociologists, say, graduate students who are learning their trade and wish to escape this

⁴² For instance, Skocpol (1988) shows that the recent rebirth of macrohistorical sociology in the U.S. and its unique sensitivity to issues of conflict, power, and social transformation, are in part an effect of the academic maturing of an “uppy generation” of students trained during the rebellious sixties who came to academia with an experience of social and political activism that made it difficult, if not impossible, for them to believe in the consensual and falsely neutral vision of society promoted by structural functionalism and modernization theory.

sterile opposition between “empty theory” and “blind research”?

PB: First and foremost: have fun! The craft of the sociologist is one of the most pleasant and enriching activities one can indulge in, spanning the whole gamut of intellectual practices and skills, from those of the novelist laboring to create emotions and character to those of the mathematician striving to capture the world in abstract models and equations. We must repel any unilateral, undimensional and mono-maniacal definition of sociological practice, and resist all attempts to impose one.

Consequently, and this would be my second point, apprentice sociologists need to question and constantly challenge methodological prescriptions and interdicts. Social research is something much too serious and much too difficult that we can allow ourselves to mistake scientific *rigidity*, which is the nemesis of intelligence and invention, for scientific *rigor*, and thus to deprive ourselves of this or that resource available in the full panoply of traditions of our discipline—and of the sister disciplines of anthropology, economics, history, etc. In such matters, I would dare say that one rule only applies: “it is forbidden to forbid.” So watch out for methodological watchdogs! Of course, the extreme liberty I advocate here (and which, let me hasten to add, has nothing in common with the kind of relativistic epistemological *laissez faire* which seems to be much in vogue in some quarters) has its counterpart in the extreme vigilance that we must accord to the conditions of use of analytical techniques and to ensuring their fit with the question at hand. Instead of arbitrarily imposing this or that technology of measurement or analysis as the penultimate badge of scientificity, we must, whenever possible, mobilize and put to work all of the techniques which are relevant and practically usable given the definition of the problem under investigation. As the most rudimentary sociology of sociology reveals, methodological indictments are often no more than a disguised way of making a virtue out of necessity, of feigning to dismiss, to ignore in an active way what one is ignorant of in fact.

Thirdly, get your hands dirty in the

kitchen sink: do not settle for the cozy and derealized experience of the social world fostered by those bureaucratic machineries of survey research that create a huge buffer between the social analyst and the universe he or she claims to dissect. Direct contact with the object not only has the virtue of helping preserve you from the fetishization of concepts and theories; it will also make you more attentive to the details of research procedures, to the built-in assumptions and consequences of apparently innocuous technical choices that are generally made unthinkingly. Most of all, you must adopt an active and systematic posture vis-à-vis “facts.” To break with empiricist passivity, which rests content with ratifying the preconstructions of common sense, without relapsing into the vacuous discourse of grand “theorizing,” you must tackle a very concrete empirical case with the goal of *building a model* (which need not be mathematical to be rigorous), by linking the relevant data in such a manner that they function as a self-propelling program of research capable of generating systematic questions liable to be given systematic answers, in short, to yield a coherent system of relations which can be tested as such. To be intelligent in the scientific sense is to put oneself in a situation that automatically generates true problems and true, productive, difficulties.

Fourthly, beware of words. Language poses a particularly acute problem for the sociologist because it carries along a “spontaneous” social philosophy which constitutes one of the most formidable “epistemological obstacles” to a rigorous science of society, to speak like Bachelard (1938). Common language is the repository of the accumulated common sense of past generations, both lay and scientific, as crystallized in occupational taxonomies, names of groups, concepts (think of all the ideological baggage bore by the apparently innocuous couple of “achievement” and “ascription,” or consensus and conflict, or even individual and society), and so on. The most routine categories that sociologists borrow from it (e.g., young and old, “middle class” and “upper-middle class”) are naturalized preconstructions which, when they are ignored as such, function as unconscious

and uncontrolled instruments of scholarly construction. One of the most powerful instruments of rupture with the *doxa* embedded in words lies in the social history of problems, concepts, and objects of inquiry. By retracing the collective work that was necessary to constitute such and such issue (the feminization of the work force, the growth of the welfare state, teenage pregnancy, or religious fundamentalism) into a visible, scientifically legitimate problem, the researcher can shelter him or herself from the social imposition of problematics. For a sociologist more than any other thinker, to leave one's own thought in a state of unthought (*impensé*) is to condemn oneself to be nothing more than the instrument of what one claims to think.

This is why, in my view, the history of sociology, understood as an exploration of the scientific unconscious of the sociologist through the explication of the genesis of problems, categories of thought, and instruments of analysis, constitutes an absolute prerequisite for scientific practice. And the same is true of the sociology of sociology: I believe that if the sociology I propose differs in any significant way from the other sociologies of the past and of the present, it is above all in that it *continually turns back onto itself the scientific weapons it produces*. It is fundamentally reflexive in that it uses the knowledge it gains of the social determinations that may bear upon it, and particularly the scientific analysis of all the constraints and all the limitations associated with the fact of occupying a definite position in a definite field at a particular moment and with a certain trajectory, in an attempt to master and neutralize their effects.

Far from undermining the foundations of social science, the sociology of the social determinants of sociological practice is the only possible ground for a possible freedom from these determinations. And it is only on condition that he avails himself the full usage of this freedom by continually subjecting himself to this analysis that the sociologist can produce a rigorous science of the social world which, far from sentencing agents to the iron cage of a strict determinism, offers them the means of a

potentially liberating awakening of consciousness.⁴³

APPENDIX: SOME BIBLIOGRAPHICAL TIPS ON HOW TO READ BOURDIEU

For the novice, finding an entry into Bourdieu's work poses the thorny problem of where to start. The following strategy reflects my personal preferences and what some of the participants to the Workshop on Pierre Bourdieu I organized found practical (only English-language writings are included and short pieces are given preference over longer ones). The order of listing, from the more (meta-)theoretical and conceptual to the more empirical, is somewhat arbitrary since Bourdieu rarely separates epistemology, theory, and empirical work, but it is useful as a practical indication of the emphases of the papers. In general, it is recommended to withhold judgment until you have read a great deal; particularly, one must read across empirical domains and alternate more theoretical and more empirically-oriented pieces. Most of all, the style and the substance of his arguments being intimately linked, seek to understand Bourdieu in his own terms before "translating" him into more friendly lexicons.

Begin with Bourdieu's "Social Space and Symbolic Power" (this issue) and with Brubaker's (1985) excellent overview, then move on to the article "On symbolic power" (Bourdieu 1979b) for a dense statement of Bourdieu's work in relation to various strands of classical sociology and philosophy (Hegel, Kant, Cassirer, Saussure, Lévi-Strauss, Durkheim, Marx, Weber, etc.), and to the 1986 interviews (Honneth, Kocyba and Schwibs 1986; Bourdieu 1986a) which help situate it more fully on the French and international intellectual scene. Although somewhat dated, "The Three Forms of Theoretical Knowledge" (Bourdieu 1973c) is a useful summary of what the French sociologist sees as the respective strengths and weaknesses of three fundamental forms of theorizing: subjectivist, objectivist, and praxeological (the transcendence of these

⁴³ The empirical demonstration of this argument is, of course, *Homo Academicus* (Bourdieu 1988a).

two). This piece also serves as a useful introduction to *Outline of a Theory of Practice* (Bourdieu 1972, 1977a).

Next, read “Men and Machines,” a terse piece where Bourdieu (1981c) outlines his conceptualization of the dialectic, or “ontological complicity,” between social action incarnate in bodies (habitus, dispositions) and in institutions (fields, positions), and by which he proposes to overcome the dichotomies of action and structure and micro- and macro-analysis. “The Forms of Capital” (Bourdieu 1986b) presents Bourdieu’s conception of the main species of capital or power: economic, cultural, social, and symbolic, and the specific effects and properties of each, as well as typical strategies and dilemmas of conversion. “Social Space and the Genesis of Groups” (Bourdieu 1985a) is a major statement of Bourdieu’s concept of social space and of his theory of group formation, including the role of symbolic power and politics in the constitution of social collectives. “The Economy of Linguistic Exchanges” (Bourdieu 1977c) extends this model to the analysis of language and leads into *Language and Symbolic Power* (1982b, 1989b).

Bourdieu’s view on the classification struggles through which correspondences between cultural and economic power are established, and which constitutes the link between *Reproduction* and *Distinction*, is expressed succinctly in Bourdieu and Boltanski (1981). “Changes in Social Structure and Changes in the Demand for Education” (Bourdieu and Boltanski 1977) analyzes the structure and functioning of the system of class strategies of reproduction. “Marriage strategies as Strategies of Reproduction” (Bourdieu 1977b) takes this analysis into the realm of kinship. Bourdieu and de Saint Martin’s (Appendix, in Bourdieu 1988a, pp. 194–225) exploration of the “Categories of Professorial Judgment” provides an extraordinarily vivid empirical illustration of the operation and mutual reinforcement of social and academic classifications.

An early empirical specification of the central concept of field is found in “The Specificity of the Scientific Field” (Bourdieu 1981d), where Bourdieu also provides the

basis for a sociological theory of scientific progress and develops a sociological epistemology. “The Field of Cultural Production” (1983d) exemplifies Bourdieu’s approach to culture and power and his uses of the concept of field, habitus, interest, structural homology, etc., in the context of a detailed study of the French literary scene of the late 19th century. “The Force of Law: Toward a Sociology of the Juridical Field” (Bourdieu 1987g) is an application of Bourdieu’s framework to the legal domain and outlines a sociological theory of law and its specific bearing upon society. “The Philosophical Establishment” (Bourdieu 1983a) does the same for the institution of philosophy.

Readers of a more empirical bent might want to begin with “The Categories of Professorial Judgment” and work their way backwards to the more conceptual pieces, then read Bourdieu’s studies of fields. Once all of this is digested, one must read *together Distinction* (Bourdieu 1984a) and *Outline of a Theory of Practice* (Bourdieu 1977a), before tackling *Homo Academicus* (1988a). Bourdieu’s best and, arguably, most important book *Le sens pratique* (1980a) is forthcoming in English under the title *The Logic of Practice* (by Polity Press and Stanford University Press).

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⁴⁴ These references were tracked with the help of Yvette Delsaut’s *Bibliographie des travaux de Pierre Bourdieu, 1958–1988* (Paris, Centre de Sociologie Européenne du Collège de France, 1989, mimeo, 39 p.), to whom I am thankful.

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