Chapter 6

The 'Corroboration' of Theories

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Sir Karl Popper is a philosopher whose work has influenced and stimulated that of virtually every student in the philosophy of science. In part this influence is explainable on the basis of the healthy-mindedness of some of Sir Karl's fundamental attitudes: 'There is no method peculiar to philosophy'. 'The growth of knowledge can be studied best by studying the growth of scientific knowledge.'

> Philosophers should not be specialists. For myself, I am interested in science and in philosophy only because I want to learn something about the riddle of the world in which we live, and the riddle of man's knowledge of that world. And I believe that only a revival of interest in these riddles can save the sciences and philosophy from an obscurantist faith in the expert's special skill and in his personal knowledge and authority.

These attitudes are perhaps a little narrow (can the growth of knowledge be studied without also studying nonscientific knowledge? Are the problems Popper mentions of merely theoretical interest—just 'riddles'?), but much less narrow than those of many philosophers, and the 'obscurantist faith' Popper warns against is a real danger. In part this influence stems from Popper's realism, his refusal to accept the peculiar meaning-theories of the positivists, and his separation of the problems of scientific methodology from the various problems about the 'interpretation of scientific theories' which are internal to the meaning-theories of the positivists and which positivistic philosophers of science have continued to wrangle about.[1]

In this paper I want to examine his views about scientific methodology—about what is generally called 'induction', although Popper rejects the concept—and, in particular, to criticize assumptions that Popper has in common with received philosophy of science, rather than assumptions that are peculiar to Popper. For I think that there are a number of such common assumptions, and that they represent a mistaken way of looking at science.

1. Popper's View of 'Induction'

Popper himself uses the term 'induction' to refer to any method for verifying or showing to be true (or even probable) general laws on the basis of observational or experimental data (what he calls 'basic statements'). His views are radically Humean: no such method exists or can exist. A principle of induction would have to be either

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synthetic a *priori* (a possibility that Popper rejects) or justified by a higher-level principle. But the latter course necessarily leads to an infinite regress.

What is novel is that Popper concludes neither that empirical science is impossible nor that empirical science rests upon principles that are themselves incapable of justification. Rather, his position is that empirical science does not really rely upon a principle of induction!

Popper does not deny that scientists state general laws, nor that they test these general laws against observational data. What he say is that when a scientist 'corroborates' *a general* law, that scientist does not thereby assert that law to be true or even probable. 'I have corroborated this law to a high degree' only means 'I have subjected this law to severe tests and it has withstood them'. Scientific laws are *falsifiable*, not verifiable. Since scientists are not even trying to *verify* laws, but only to falsify them, Hume's problem does not arise for empirical scientists.

2. *A Brief Criticism of Popper's View*

It is a remarkable fact about Popper's book, *The Logic of Scientific Discovery* that it contains but a half-dozen brief references to the *application* of scientific theories and laws; and then all that is said is that application is yet another *test* of the laws. 'My view is that ... the theorist is interested in explanations as such, that is to say, in testable explanatory theories: applications and predictions interest him only for theoretical reasons—because they may be used as *tests* of theories' (*Logic of Scientific Discovery*, p. 59).

When a scientist accepts a law, he is recommending to other men that they rely on it—rely on it, often, in practical contexts. Only by wrenching science altogether out of the context in which it really arises—the context of men trying to change and control the world—can Popper even put forward his peculiar view on induction. Ideas are not *just* ideas; they are guides to action. Our notions of 'knowledge', 'probability', 'certainty', etc., are all linked to and frequently used in contexts in which action is at issue: may I confidently rely upon a certain idea? Shall I rely upon it tentatively, with a certain caution? Is it neccssary to check on it?

If 'this law is highly corroborated', 'this law is scientifically accepted', and like locutions merely meant 'this law has withstood severe tests'—and there were no suggestion at all that a law which has to withstood severe tests is likely to withstand further tests, such as the tests involved in an application or attempted application, then Popper would be right; but then science would be a wholly unimportant activity. It would be practically unimportant, because scientists would never tell us that any law or theory is safe to rely upon for practical purposes; and it would be unimportant for the purpose of understanding, since on Popper's view, scientists never tell us that any law or theory is true or even probable. Knowing that certain 'conjectures' (according to Popper all scientific laws are 'provisional conjectures') have not yet been refuted is *not understanding anything*.

Since the application of scientific laws does involve the anticipation of future successes, Popper is not right in maintaining that induction is unnccessary. Even if scientists do not inductively anticipate the future (and, of course, they do), men who apply scientific laws and theories do so. And 'don't make inductions' is hardly reasonable advice to give these men.

The advice to regard all knowledge as 'provisional conjectures' is also not reasonable. Consider men striking against sweat-shop conditions. Should they say 'it is only a provisional conjecture that the boss is a bastard. Let us call off our strike and try appealing to his better nature'. The distinction between *knowledge* and *conjecture* does real work in our lives; Popper can maintain his extreme skepticism only because of his extreme tendency to regard theory as an end for itself.

3. *Popper's View of Corroboration*

Although scientists, on Popper's view, do not make inductions, they do 'corroborate' scientific theories. And although the statement that a theory is highly corroborated does not mean, according to Popper, that the theory may be accepted as true, or even as approximately true,[2] or even as probably approximately true, still, there is no doubt that most readers of Popper read his account of corroboration as an account of something like the verification of theories, in spite of his protests. In this sense, Popper has, *contre lui* a theory of induction. And it is this theory, or certain presuppositions of this theory, that I shall criticize in the body of this paper.

Popper's reaction to this way of reading him is as follows:

> My reaction to this reply would be regret at my continued failure to explain my main point with sufficient clarity. For the sole purpose of the elimination advocated by all these inductivists was to *establish as firmly as possible the surviving theory* which, they thought, must be the *true* one (or, perhaps, only a *highly probable* one, in so far as we may not have fully succeeded in eliminating every theory except the true one).
>
> As against this, I do not think that we can ever seriously reduce by elimination, the number of the competing theories, since this number remains always infinite. What we do—or should do—is to *hold on, for the time being, to the most improbable of the surviving theories* or, more precisely, to the one that can be most severely tested. We tentatively *'accept'* this theory—but only in the sense that we select it as worthy to be subjected to further criticism, and to the severest tests we can design.
>
> On the positive side, we may be entitled to add that the surviving theory is the best theory—and the best tested theory—of which we know. (*Logic of Scientific Discovery*, p. 419)

If we leave out the last sentence, we have the doctrine we have been criticizing in pure form: when a scientist 'accepts' a theory, he does not assert that it is probable. In fact, he 'selects' it as most improbable! In the last sentence, however, am I mistaken, or do I detect an inductivist quaver? What does 'best theory' mean? Surely Popper cannot mean 'most likely'?

4. *The Scientific Method—The Received Schema*

Standard 'inductivist' accounts of the confirmation[3] of scientific theories go somewhat like this: Theory implies prediction (basic sentence, or observation sentence); if prediction is false, theory is falsified; if sufficiently many predictions are true, theory is confirmed. For all his attack on inductivism, Popper's schema is not *so* different: Theory implies prediction (basic sentence); if prediction is false, theory is falsified; if sufficiently many predictions are true, and certain further conditions are fulfilled, theory is highly corroborated.

Moreover, this reading of Popper does have certain support. Popper does say that the 'surviving theory' is *accepted*—his account is, therefore, an account of the logic of accepting theories. We must separate two questions: is Popper right about what the scientist means—or should mean—when he speaks of a theory as 'acccpted'; and is Popper right about the methodology involved in according a theory that status? What I am urging is that his account of that methodology fits the received schema, even it his interpretation of the status is very different.

To be sure there are some important conditions that Popper adds. Predictions that one could have made on the basis of background knowledge do not test a theory; it is only predictions that are *improbable* relative to background knowledge that test a theory. And a theory is not corroborated, according, to Popper, unless we make sincere attempts to derive false predictions from it. Popper regards these further conditions as anti-Bayesian;[4] but this seems to me to be a confusion, at least in part. A theory which implies an improbable prediction is improbable, that is true, but it may be the most probable of all theories which imply that prediction. If so, and the prediction turns out true, then Bayes's theorem itself explains why the theory receives a high probability. Popper says that we select the most improbable of the *surviving* theories—i.e., the accepted theory is most improbable even *after* the prediction has turned out true; but, of course, this depends on using 'probable' in a way no other philosopher of science would accept. And a Bayesian is not committed to the view that *any* true prediction significantly confirms a theory. I share Popper's view that quantitative measures of the probability of theories are not a hopeful venture in the philosophy of science;[5] but that does not mean that Bayes's theorem does not have a certain *qualitative* rightness, at least in many situations.

Be all this as it may, the heart of Popper's schema is the theory-prediction link. It is because theories imply basic sentences in the sense of 'imply' associated with deductive logic—because basic sentences are *deducible* from theories—that, according to Popper, theories and general laws can be falsifiable by basic sentences. And this same link is the heart of the 'inductivist' schema. Both schemes say: *look at the predictions that a theory implies; see if those predictions are true.*

My criticism is going to be a criticism of this link, of this one point on which Popper and the 'inductivists' agree. I claim: in a great many important cases, scientific theories do not imply predictions at all. In the remainder of this paper I want to elaborate this point, and show its significance for the philosophy of science.

5. *The Theory of Universal Gravitation*

The theory that I will use to illustrate my points is one that the reader will be familiar with: it is Newton's theory of universal gravitation. The theory consists of the law that every body a exerts on every other body b a force F_{ab} whose direction is towards a and whose magnitude is a universal constant g times M_aM_b/d^2, together with Newton's three laws. The choice of this particular theory is not essential to my case: Maxwell's theory, or Mendel's, or Darwin's would have done just as well. But this one has the advantage of familiarity.

Note that this theory does not imply a single basic sentence! Indeed, any motions whatsoever are compatible with this theory, since the theory says nothing about what forces other than gravitations may be present. The forces F_{ab} are not themselves directly measurable; consequently not a single *prediction* can be deduced from the theory.

What do we do, then, when we apply this theory to an astronomical situation? Typically we make certain simplifying assumptions. For example, if we are deducing the orbit of the earth we might assume as a first approximation:

(I) No bodies exist except the sun and the earth.

(II) The sun and the earth exist in a hard vacuum.

(III) The sun and the earth are subject to no forces except mutually induced gravitational forces.

From the conjunction of the theory of universal gravitation (U.G.) and these auxiliary statements (A.S.) we can, indeed, deduce certain predictions—e.g., Kepler's laws. By making (I), (II), (III) more 'realistic'—i.e., incorporating further bodies in our model solar system—we can obtain better predictions. But it is important to note that these predictions do not come from the theory alone, but from the conjunction of the theory with A.S. As scientists actually use the term 'theory', the statements A.S. are hardly part of the 'theory' of gravitation.

6. *Is the Point Terminological?*

I am not interested in making a merely *terminological* point, however. The point is not just that scientists don't use the term 'theory' to refer to the conjunction of U.G. with A.S., but that such a usage would obscure profound methodological issues. A *theory*, as the term is actually used, is a set of *laws*. Laws are statements that we hope to be *true* they are supposed to be true by the nature of things, and not just by accident. None of the statements (I), (II), (III) has this character. We do not really believe that *no* bodies except the sun and the earth exist, for example, but only that all other bodies exert forces small enough to be neglected. This statement is not supposed to be a law, of nature: it is a statement about the 'boundary conditions' which obtain as a matter of fact in a particular system. To blur the difference between A.S. and U.G. is to blur the difference between *laws* and *accidental statements*, between statements the scientist wishes to establish as *true* (the laws), and statements he already knows to be false (the oversimplifications (I), (II), (III)).

7. *Uranus, Mercury, 'Dark Companions'*

Although the statements A.S. *could* be more carefully worded to avoid the objection that they are known to be false, it is striking that they are not in practice. In fact, they are not 'worded' at all. Newton's calculation of Kepler's laws makes the assumptions (I), (II), (III) without more than a casual indication that this is what is done. One of the most striking indications of the difference between a theory, (such as U.G.) and a set of A.S. is the great care which scientists use in stating the theory, as contrasted with the careless way in which they introduce the various assumptions which make up A.S.

The A.S. are also far more subject to revision than the theory. For over two hundred years the law of universal gravitation was accepted as unquestionably true, and used as a premise in countless scientific arguments. If the standard kind of A.S. had not had to successful prediction in that period, they would have been modified, not the theory. In fact, we have an example of this. When the predictions about the orbit of Uranus that were made on the basis of the theory of universal gravitation and the assumption that the known planets were all there were turned out to be wrong, Leverrier in France and Adams in England simultaneously predicted that there must be another planet. In fact, this planet was discovered—it was Neptune. Had this modification of the A.S. not been successful, still others might have been tried—e.g., postulating a medium through which the planets are moving, instead of a hard vacuum or postulating significant nongravitational forces.

It may be argued that it was crucial that the new planet should itself be observable. But this is not so. Certain stars, for example, exhibit irregular behavior. This has been explained by postulating companions. When those companions are not visible through a telescope, this is handled by suggesting, that the stars have *dark companions*—companions which cannot be seen through a telesope. The fact is that many of the assumptions made in the sciences cannot be directly tested—there are many 'dark companions' in scientific theory.

Lastly, of course, there is the case of Mercury. The orbit of this planet can almost but not quite be successfully explained by Newton's theory. Does this show that Newton's theory is wrong? *In the light of an alternative theory,* say the General Theory of Relativity, one answers 'yes' But, in the absence of such a theory, the orbit of Mercury is just a slight anomaly, cause: unknown.

What I am urging is that all this is perfectly good scientific practice. The fact that any of the statements A.S. may be false—indeed, they are false, as stated, and even more careful and guarded statements might well be false—is important. We do not know for sure all the bodies in the solar system; we do not know for sure that the medium through which they move is (to a sufficiently high degree of approximation in all cases) a hard vacuum; we do not know that nongravitational forces can be neglected in all cases. Given the over-whelming success of the Law of Universal Gravitation in almost all cases, one or two anomalies are not reason to reject it. It is more *likely* that the A.S. are false than that the theory is false, at least when no alternative theory has seriously been put forward.

8. *The Effect on Popper's Doctrine*

The effect of this fact on Popper's doctrine is immediate. The Law of Universal Gravitation is *not* strongly falsifiable at all; yet it is surely a paradigm of a scientific theory. Scientists for over two hundred years did not derive predictions from U.G. in order to falsify U.G.; they derived predictions from U.G. in order to explain various astronomical facts. If a fact proved recalcitrant to this sort of explanation it was put aside as an anomaly (the case of Mercury). Popper's doctrine gives a correct account of neither the nature of the scientific theory nor of the practice of the scientific community in this case.

Popper might reply that he is not describing what scientists do, but what they *should* do. Should scientists then not have put forward U.G.? Was Newton a bad scientist? Scientists did not try to falsify U.G. because they could not try to falsify it; laboratory tests were excluded by the technology of the time and the weakness of the gravitational interactions. Scientists were thus limited to astronomical data for a long time. And, even in the astronomical cases, the problem arises that one cannot be absolutely sure that no nongravitational force is relevant in a given situation (or that one has summed *all* the gravitational forces). It is for this reason that astronomical data can *support* U.G., but they can hardly *falsify* it. It would have been incorrect to reject U.G. because of the deviancy of the orbit of Mercury; given that U.G. predicted the other orbits, to the limits of measurement error, the possibility could not be excluded that the deviancy in this one case was due to an unknown force, gravitational or nongravitational, and in putting the case aside as they could neither explain nor attach systematic significance to, scientists *were* acting as they 'should'.[6]

So far we have said that (1) theories do not imply predictions; it is only the conjunction of a theory with certain 'auxiliary statements' (A.S.) that, in general, implies a prediction. (2) The A.S. are frequently suppositions about boundary conditions (including initial conditions as a special case of 'boundary conditions'), and highly risky suppositions at that. (3) Since we are very unsure of the A.S., we cannot regard a false prediction as definitively falsifying a theory; theories are *not* strongly falsifiable.

All this is not to deny that scientists do sometimes derive predictions from theories and A.S. in order to test the theories. If Newton had not been able to derive Kepler's laws, for example, he would not have even put forward U.G. But even if the predictions Newton had obtained from U.G. had been wildly wrong, U.G. might still have been true: the A.S. might have been wrong. Thus, even if a theory is 'knocked out' by an experimental test, the theory may still be right, and the theory may come back in at a later stage when it is discovered the A.S. were not useful approximations to the true situation. As has previously been pointed out,[7] falsification in science is no more conclusive than verification.

All this refutes Popper's view that what the scientist does is to put forward 'highly falsifiable' theories, derive predictions from them, and then attempt to falsify the theories by falsifying the predictions. But it does not refute the standard view (what Popper calls the 'inductivist' view) that scientists try to *confirm* theories *and* A.S. by deriving predictions from them and verifying the predictions. There is the objection that (in the case of U.G.) the A.S. were known to be false, so scientists could hardly have been trying to confirm them; but this could be met by saying that the A.S. could, in principle, have been formulated in a more guarded way, and would not have been false if sufficiently guarded[8] I think that, in fact, there is some truth in the 'inductivist' view: scientific theories are shown to be correct be their successes, just as all human ideas are shown to be correct, to the extent that they are, by their successes in practice. But the inductivist schema is still inadequate, except as a picture of one aspect of scientific I procedure. In the next sections, I shall try to show that scientific activity cannot, in general, be thought of as a matter of deriving predictions from the conjunction of theories and A.S., whether for the purpose of confirmation or for the purpose of falsification.

9. *Kuhn's View of Science*

Recently a number of philosophers have begun to put forward a rather new view of scientific activity. I believe that I anticipated this view about ten years ago when I urged that some scientific theories cannot be overthrown by experiments and obseratiors *alone*, but only by alternative theories.[9] The view is also anticipated by Hanson,[10] but it reaches its sharpest expression in the writings of Thomas Kuhn[11] and Louis Althusser.[12] I believe that both of these philosophers commit errors; but I also believe that the tendency they represent (and that I also represent, for that matter) is a needed corrective to the deductivism we have been examining. In this section. I shall present some of Kuhn's views, and then try to advance on them in the direction of a sharper formulation.

The heart of Kuhn's account is the notion of a *paradigm*. Kuhn has been legitimately criticized for some inconsistencies and unclarities in the use of this notion; but at least one of his explanations of the notion seems to me to be quite clear and suitable for his purposes. On this explanation, a paradigm is simply a scientific theory together with an example of a successful and striking application. It is important that the application—say, a successful explanation of some fact, or a successful and novel prediction—be *striking*; what this means is that the success is sufficiently impressive that scientists—especially young scientists choosing a career—are led to try to emulate that success by seeking further explanations, predictions, or whatever on the same model. For example, once U.G. had been put forward and one had the example of Newton's derivation of Kepler's laws together with the example of the derivation of, say, a planetary orbit or two, then one had a paradigm. The most important paradigms are the ones that generate scientific fields; the field generated by the Newtonian paradigm was, in the first instance, the entire field of celestial mechanics. (Of course, this field was only a part of the larger field of Newtonian mechanics, and the paradigm on which celestial mechanics is based is only one of a number of paradigms which collectively structure Newtonian mechanics.)

Kuln maintains that the paradigm that structures a field is highly immune to falsification—in particular, it can only be overthrown by a new paradigm. In one sense, this is an exaggeration: Newtonian physics would probably have been abandoned, even in the absence of a new paradigm, if the world had started to act in a markedly non-Newtonian way. (Although even then—would we have concluded that Newtonian physics was false, or just that we didn't know what the devil was going, on?) But then even the old successes, the successes which were paradigmatic for Newtonian physics, would have ceased to be available. What is true, I believe, is that in the absence of such a drastic and unprecedented change in the world, and in the absence of its turning out that the paradigmatic successes had something 'phony' about them (e.g., the data were faked, or there was a mistake in the deductions), a theory which is paradigmatic is not given up because of observational and experimental results by themselves, but because and when a better theory is available.

Once a paradigm has been set up, and a scientific field has grown around that paradigm, we get an interval of what Kuhn calls 'normal science'. The activity of scientists during such an interval is described by Kuhn as 'puzzle solving'—a notion I shall return to.

In general, the interval of normal science continues even though not all the puzzles of the field can be successfully solved (after all, it is only human experience that some problems are too hard to solve), and even though some of the solutions may look *ad hoc*. What finally terminates the interval is the introduction of a new paradigm which manages to supersede the old.

Kuhn's most controversial assertions have to do with the process whereby a new paradigm supplants an older paradigm. Here he tends to be radically subjectivistic (overly so, in my opinion): data, in the usual sense, cannot establish the superiority of one paradigm over another because data themselves are perceived through the spectacles of one paradigm or another. Changing from one paradigm to another requires a 'Gestalt switch'. The history and methodology of science get rewritten when there are major paradigm changes; so there are no 'neutral' historical and methodological canons to which to appeal. Kuhn also holds views on meaning and truth which are relativistic and, on my view, incorrect; but I do not wish to discuss these here.

What I want to explore is the interval which Kuhn calls 'normal science'. The term 'puzzle solving' is unfortunately trivializing; searching for explanations of phenomena and for ways to harness nature is too important a part of human life to be demeaned (here Kuhn shows the same tendency that leads Popper to call the problem of the nature of knowledge a 'riddle'). But the term is also striking: clearly, Kuln sees normal science as neither an activity of trying to falsify one's paradigm nor as an activity of trying to confirm it, but as something else. I want to try to advance on Kuhn by presenting a schema for normal science, or rather for one aspect of normal science; a schema which may indicate why a major philosopher and historian of science would use the metaphor of solving puzzles in the way Kuhn does.

10. Schemata for Scientific Problems

Consider the following two schemata:

SCHEMA I

THEORY
AUXILIARY STATEMENTS

PREDICTION—TRUE OR FALSE?

SCHEMA II

THEORY
?????????

FACT TO BE EXPLAINED

These are both schemata for scientific problems. In the first type of problem we have a theory, we have some A.S., we have derived a prediction, and our problem is to see if the prediction is true or false: the situation emphasized by standard philosophy of science. The second type of problem is quite different. In this type of problem we have a theory, we have a fact to be explained, but the A.S. are missing: the problem is to find A.S., if we can, which are true, or approximately true (i.e., useful oversimplifications of the truth), and which have to be conjoined to the theory to get an explanation of the fact.

We might, in passing, mention also a third schema which is neglected by standard philosophy of science:

SCHEMA III

THEORY
AUXILIARY STATEMENTS

??????????

This represents the type of problem in which we have a theory, we have some A.S., and we want to know what consequences we can derive. This type of problem is neglected because the problem is 'purely mathematical'. But knowing whether a set of statements has testable consequences at all depends upon the solution to this type of problem, and the problem is frequently of great difficulty—e.g., little is known to this day concerning just what the physical consequences of Einstein's 'unified field theory' are, precisely because the mathematical problem of deriving those consequences is too difficult. Philosophers of science frequently write as if it is *clear*, given a set of statements, just what consequences those statements do and do not have.

Let us, however, return to Schema II. Given the known facts concerning the orbit of Uranus, and given the known facts (prior to 1846) concening what bodies make up the solar system, and the standard A.S. that those bodies are moving in a hard vacuum, subject only to mutual gravitational forces, etc., it was clear that there was a problem: the orbit of Uranus could not be successfully calculated if we assumed that Mercury, Venus, Earth, Mars, Saturn, Jupiter, and Uranus were all the planets there are, and that these planets together with the sun make up the whole solar system. Let S_1 be the conjunction of the various A.S. we just mentioned, including the statement that the solar system consists of at least, but not necessarily of only, the bodies mentioned. Then we have the following problem:

Theory: U.G.
A.S.: S_1
Further A.S.: ??????

Explanandum: The orbit of Uranus

Note that the problem is not to find further explanatory laws (although sometimes it may be, in a problem of the form of Schema II); it is to find further assumptions about the initial and boundary conditions governing the solar system which, together with the Law of Universal Gravitation and the other laws which make up U.G. (i.e., the laws of Newtonian mechanics) will enable one to explain the orbit of Uranus. If one does not require that the missing statements be true, or approximately true, then there are an infinite number of solutions, mathematically speaking. Even if one includes in S_1 that no nongravitational forces are acting on the planets or the sun, there are still an infinite number of solutions. But one tries first the simplest assumption, namely:

(S_2) There is one and only one planet in the solar system in addition to the planets mentioned in S_1.

Now one considers the following problem:

Theory: U.G.
A.S.: S_1, S_2

Consequence ???—turns out to be that the unknown planet must have a certain orbit O.

This problem is a mathematical problem—the one Leverrier and Adams both solved (an instance of Schema III). Now one considers the following empirical problem:

Theory: U.G.
A.S.: S_1, S_2

Prediction: A planet exists moving in orbit O—TRUE OR FALSE?

This problem is an instance of Schema I—an instance one would not normally consider, because one of the A.S., namely the statement S_2, is not at all known to be true S_2 is, in fact, functioning as a low-level hypothesis which we wish to test. But the test is not an inductive one in the usual sense, because a verification of the prediction is also a verification of S_2—or rather, of the approximate truth of S_2, (which is all that is of interest in this context). Neptune was not the only planet unknown in 1846; there was also Pluto to be later discovered. The fact is that we are interested in the above problem in 1846, because we know that if the prediction turns out to be true, then that prediction is precisely the statement S_3 that we need for the following deduction:

Theory: U.G.
A.S.: S_1, S_2, S_3

Explanandum: the orbit of Uranus

—i.e., the statement S_3 (that the planet mentioned in S_2 has precisely the orbit O)[13] is the solution to the problem with which we started. In this case we started with a problem of the Schema II type: we introduced the assumption S_2 as a simplifying assumption in the hope of solving the original problem thereby more easily; and we had the good luck to be able to deduce S_3—the solution to the original problem—from U.G. together with S_1, S_2, and the more important good luck that S_3 turned out to be true when the Berlin Observatory looked. Problems of the Schema II-type are sometimes mentioned by philosophers of science when the missing A.S. are laws; but the case just examined, in which the missing A.S. was just a further contingent fact about the particular system, is almost never discussed. I want to suggest that Schema II exhibits the logical form of what Kuhn calls a 'puzzle'.

If we exmine Schema II, we can see why the term 'puzzle' is so appropriate. When one has a problem of this sort, one is looking for something to fill a 'hole'—often a thing of rather underspecified sort—and that *is* a sort of *puzzle*. Moreover, this sort of problem is extremely widespread in science. Suppose one wants to explain the fact that water is a liquid (under the standard conditions), and one is given the laws of physics; the fact is that the problem is extremely hard. In fact, quantum mechanical laws are needed. But that does not mean that from classical physics one can deduce that water is *not* a liquid; rather the classical physicist would give up this problem at a certain point as 'too hard'—i.e., he would conclude that he could not find the right A.S.

The fact that Schema II is the logical form of the 'puzzles' of ' normal science explains a number of facts. When one is tackling, a Schema II-type problem, there is no question of deriving a prediction from U.G. plus given A.S., the whole problem is to find the A.S. The theory—U.G., or whichever—is *unfalsifiable in the context*. It is also not up for 'confirmation' any more than for 'falsification'; *it is not function in a hypothetical role*. Failures do not falsify a theory, because the failure is not a false prediction from a theory together with known and trusted facts, but a failure to *find* something—in fact, a failure to find an A.S. Theories, during their tenure of office, are highly immune to falsification; that tenure of office is ended by the appearance on the scene of a better theory (or a whole new explanatory technique), not by a basic sentence. And successes do not 'confirm' a theory, once it has become paradigmatic, because the theory is not a 'hypothesis' in need of confirmation, but the basis of a whole explanatory and predictive technique, and possibly of a technology as well.

To sum up: I have suggested that standard philosophy of science, both 'Popperian' and non-Popperian, has fixated on the situation in which we derive predictions from a theory and test those predictions in order to falsify or confirm the theory —i.e., on the situation represented by Schema I. I have suggested that, by way of contrast, we see the 'puzzles' of 'normal science' as exhibiting the pattern represented by Schema II, the pattern in which we take a theory as fixed, take the fact to be explained as fixed, and seek further facts—frequently contingent[14] facts about the particular system—which will enable us to fill out the explanation of the particular fact on the basis of the theory. I suggest that adopting this point of view will enable us better to appreciate both the relative unfalsifiability of theories which have attained paradigm status, and the fact that the 'predictions' of physical theory are frequently facts which were known beforehand and not things which are surprising relative to background knowledge.

To take Schema II as describing everything that goes on between the introduction of a paradigm and its eventual replacement by a better paradigm would be a gross error in the opposite direction, however. The fact is that normal science exhibits a dialectic between two conflicting (at any rate, potentially conflicting) but interdependent tendencies, and that it is the conflict of these tendencies that drives normal science forward. The desire to solve a Schema II-type problem—explain the orbit of Uranus—led to a new hypothesis (albeit a very low-level one) namely, S_2. Testing S_2

involved deriving S_3 from it, and testing S_3 a Schema I-type situation. S_3 in turn served as the solution to the original problem. This illustrates the two tendencies, and also the way in which they are interdependent and the way in which their interaction drives science forward.

The tendency represented by Schema I is the *critical* tendency. Popper is right to emphasize the importance of this tendency, and doing this is certainly a contribution on his part—one that has influenced many philosophers. Scientists do want to know if their ideas are wrong, and they try to find out if their ideas are wrong by deriving predictions from them and testing those predictions—that is, they do this *when they can*. The tendency represented by Schema II is the *explanatory* tendency. The element of conflict arises because in a Schema II-type situation one tends to regard the given theory as something *known*, whereas in a Schema I-type situation one tends to regard it as *problematic*. The interdependence is obvious: the theory which serves as the major premise in Schema II *may* itself have been the survivor of a Popperian test (although it need not have been—U.G. was accepted on the basis of its explanatory successes, not on the basis of its surviving attempted falsifications). And the solution to a Schema II-type problem must itself be confirmed, frequently by a Schema I-type test. If the solution is a general law, rather than a singular statement, that law may itself become a paradigm, leading to new Schema II-type problems. In short, attempted falsifications do 'corroborate' theories—not just in Popper's sense, in which this is a tautology, but in the sense he denies, of showing that they are true, or partly true—and explanations on the basis of laws which are regarded as *known* frequently require the introduction of *hypotheses*. In this way, the tension between the attitudes of explanation and criticism drives science to progress.

11. Kuhn versus Popper

As might be expected, there are substantial differences between Kuhn and Popper on the issue of the falsifiability of scientific theories. Kuhn stresses the way in which a scientific theory may be immune from falsification, whereas Popper stresses falsifiability as the *sine qua non* of a scientific theory. Popper's answers to Kuhn depend upon two notions which must now be examined: the notion of an auxiliary hypothesis and the notion of a *conventionalist stratagem*.

Popper recognizes that the derivation of a prediction from a theory may require the use of auxiliary hypotheses (though the term 'hypothesis' is perhaps misleading, in suggesting something like putative laws rather than assumptions about, say, boundary conditions). But he regards these as part of the total 'system' under test. A 'conventionalist stratagem' is to save a theory from a contrary experimental result by making an *ad hoc* change in the auxiliary hypotheses. And Popper takes it as a fundamental methodological rule of the empirical method to avoid conventionalist stratagems.

Does this do as a reply to Kuhn's objections? Does it contravene our own objections, in the first part of this paper? It does not. In the first place, the 'auxiliary hypotheses' A.S. are not fixed, in the case of U.G., but depend upon the context. One simply cannot think of U.G. as part of a fixed 'system' whose other part is a fixed set of auxiliary hypotheses whose function is to render U.G. 'highly testable'.

In the second place, an alteration in one's beliefs, may be *ad hoc* without being unreasonable. '*Ad hoc*' merely means 'to this specific purpose'. Of course, '*ad hoc*' has acquired the connotation of 'unreasonable'—but that is a different thing. The assumption that certain stars have dark companions is *ad hoc* in the literal sense: the assumption is made for the specific purpose of accounting for the fact that no companion is visible. It is also highly reasonable.

It has already been pointed out that the A.S. are not only context-dependent but highly uncertain, in the case of U.G. and in many other cases. So, changing the A.S., or even saying in a particular context 'we don't know what the right A.S. are' may be *ad hoc* in the literal sense just noted, but is not '*ad hoc*' in the extended sense of 'unreasonable'.

12. *Paradigm Change*

How does a paradigm come to be accepted in the first place? Popper's view is that a theory becomes corroborated by passing severe tests: a prediction (whose truth value is not antecedently known) must be derived from the theory and the truth or falsity of that prediction must be ascertained. The severity of the test depends upon the set of basic sentences excluded by the theory, and also upon the improbability of the prediction relative to background knowledge. The ideal case is one in which a theory which rules out a great many basic sentences implies a prediction which is very improbable relative to background knowledge.

Popper points out that the notion of the number of basic sentences ruled out by a theory cannot be understood in the sense of cardinality; he proposes rather to measure it by means of concepts of *improbability* or *content*. It does not appear true to me that improbability (in the sense of logical [im]probability)[15] measures fasifiability, in Popper's sense: U.G. excludes *no* basic sentences, for example, but has logical probability *zero*, on any standard metric. And it certainly is not true that the scientist always selects 'the most improbable of the surviving hypotheses' on *any* measure of probability, except in the trivial sense that all strictly universal laws have probability zero. But my concern here is not with the technical details of Popper's scheme, but with the leading idea.

To appraise this idea, let us see how U.G. came to be accepted. Newton first derived Kepler's laws from U.G. and the A.S. we mentioned at the outset: this was not a 'test' in Popper's sense, because Kepler's laws were already known to be true. Then he showed that U.G. would account for the tides on the basis of the gravitational pull of the moon: this also was not a 'test', in Popper's sense, because the tides were already known. Then he spent many years showing that small perturbations (which were already known) in the orbits of the planets could be accounted for by U.G. By this time the whole civilized world had accepted—and, indeed, acclaimed—U.G.; but it had not been 'corroborated' at all in Popper's sense!

If we look for a Popperian 'test' of U.G.—a derivation of a new prediction, one risky relative to background knowledge—we do not get one until the Cavendish experiment of 1787, roughly a hundred years after the theory had been introduced! The prediction of S_3 (the orbit of Neptune) from U.G. and the auxiliary statements S_1 and S_2 can also be regarded as a confirmation of U.G. (in 1846!); although it is difficult to regard it as a severe test of U.G. in view of the fact that the assumption S_2 had a more tentative status than U.G.

It is easy to see what has gone wrong. A theory is not accepted unless it has real explanatory successes. Although a theory may legitimately be preserved by changes in the A.S. which are, in a sense, '*ad hoc*' (although not *unreasonable*), its *successes* must not be *ad hoc*. Popper requires that the predictions of a theory must not be antecedently known to be true in order to rule out *ad hoc* 'successes'; but the condition is too strong.

Popper is right in thinking that a theory runs a risk during the period of its establishment. In the case of U.G., the risk was not a risk of definite falsification; it was the risk that Newton would not find reasonable A.S. with the aid of which he could obtain real (non-*ad hoc*) explanatory successes for U.G. A failure to explain the tides by the gravitational pull of the moon alone would not, for example, have falsified U.G.; but the success did strongly support U.G.

In sum, a theory is only accepted if the theory has substantial, non-*ad hoc*, explanatory successes. This is in accordance with Popper; unfortunately, it is in even better accordance with the 'inductivist' accounts that Popper rejects, since these stress *support* rather than *falsification*.

13. *On Practice*

Popper's mistake here is no small isolated failing. What Popper consistently fails to see is that *practice is primary*: ideas are not just an end in themselves (although they are *partly* an end in themselves), nor is the selection of ideas to 'criticize' just an end in itself. The primary importance of ideas is that they guide practice, that they structure whole forms of life. Scientific ideas guide practice in science, in technology, and sometimes in public and private life. We are concerned in science with trying to discover correct ideas: Popper to the contrary, this is not *obscurantism* but *responsibility*. We obtain our ideas—our correct ones, and many of our incorrect ones—by close study of the world. Popper denies that the accumulation of perceptual experience leads to theories: he is right that it does not lead to theories in a mechanical or algorithmic sense; but it does lead to theories in the sense that it is a regularity of methodological significance that (1) lack of experience with phenomena and with previous knowledge about phenomena decreases the probability of correct ideas in a marked fashion; and (2) extensive experience increases the probability of correct, or partially correct, ideas in a marked fashion. 'There is no logic of discovery'—in that sense, there is no logic of *testing*, either; all the formal algorithms proposed for testing, by Carnap, by Popper, by Chomsky, etc., are, to speak impolitely, *ridiculous*; if you don't believe this, program a computer to employ one of these algorithms and see how well it does at testing theories! There are *maxims* for discovery and maxims for testing: the idea that correct ideas just come from the sky, while the methods for testing them are highly rigid and predetermined, is one of the worst legacies of the Vienna Circle.

But the correctness of an idea is not certified by the fact that it came from close and concrete study of the relevant aspects of the world; in this sense, Popper is right. We judge the correctness of our ideas by applying them and seeing if they succeed; in general, and in the long run, correct ideas lead to success, and ideas lead to failures where and insofar as they are incorrect. Failure to see the importance of practice leads directly to failure to see the importance of success.

Failure to see the primacy of practice also leads Popper to the idea of a sharp 'demarcation' between science, on the one hand, and political, philosophical, and ethical ideas, on the other. This 'demarcation' is pernicious, in my view; fundamentally, it corresponds to Popper's separation of theory from practice, and his related separation of the critical tendency in science from the explanatory tendency in science. Finally, the failure to see the primacy of practice leads Popper to some rather reactionary political conclusions. Marxists believe that there are laws of society; that these laws can be known; and that men can and should act on this knowledge. It is not my purpose here to argue that this Marxist view is correct; but surely any view that rules this out *a priori* is reactionary. Yet this is precisely what Popper does—and in the name of an anti-*a priori* philosophy of knowledge!

In general, and in the long run, true ideas are the ones that succeed—how do we know this? This statement too is a statement about the world; a statement we have come to from experience of the world; and we believe in the practice to which this idea corresponds, and in the idea as informing that kind of practice, on the basis that we believe in any good idea—it has proved successful! In this sense 'induction is circular'. But of course it is! Induction has no deductive justification; induction is not deduction. Circular justifications need not be totally self-protecting nor need they be totally uninformative:[16] the past success of 'induction' increases our confidence in it, and its past failure tempers that confidence. The fact that a justification is circular only means that that justification has no power to serve as a *reason*, unless the person to whom it is given as a reason already has some propensity to accept the conclusion. We do have a propensity—an *a priori* propensity, if you like—to reason 'inductively', and the past success of 'induction' increases that propensity.

The method of testing ideas in practice and relying on the ones that prove successful (for that is what 'induction' is) is not unjustified. That is an *empirical* statement. The method does not have a 'justification'—if by a justification is meant a proof from eternal and formal principles that justifies reliance on the method. But then, nothing does—not even, in my opinion, pure mathematics and formal logic. Practice is primary.

Notes

1. I have discussed positivistic meaning theory in "What Theories Are Not," published in *Logic, Methodology, and Philosophy of Science*, ed. by A. Tarski, E. Nagel, and P. Suppes (Stanford: Stanford University Press, 1962), pp. 240–51, and also in "How Not to Talk about Meaning," published in *Boston Studies in the Philosophy of Science*, Vol. II, ed. by R. S. Coehn and M. W. Wartofsky (New York: Humanities Press, 1965), pp. 205–22.
2. For a discussion of 'approximate truth', see the second of the papers mentioned in the preceding note.
3. 'Confirmation' is the term in standard use for *support* a positive experimental or observational result gives to a hypothesis; Popper uses the term 'corroboration' instead, as a rule, because he objects to the connotations of 'showing to be true' (or at least probable) which he sees as attaching to the former term.
4. *Bayes's theorem* asserts, roughly, that the probability of a hypothesis H on given evidence E is directly proportional to the probability of E on the hypothesis H, and also directly proportional to the antecedent probability of H—i.e., the probability of H if one doesn't know that E. The theorem also asserts that the probability of H on the evidence E is less, other things being equal, if the probability of E on the assumption—(*not*-H) is greater. Today probability theorists are divided between those who accept the notion of "antecedent probability of a hypothesis," which is crucial to the theorem, and those who reject this notion, and therefore the notion of the probability of a hypothesis on given evidence. The former school are called 'Bayeseans'; the latter 'anti-Bayeseans'.
5. Cf. my paper "'Degree of Confirmation' and Inductive Logic," in *The Philosophy of Rudolf Carnap* (The Library of Living Philosophers, Vol. II), ed. by Paul A. Schilpp (La Salle, Ill.: Open Court Publishing Co., 1963), pp. 761–84.
6. Popper's reply to this sort of criticism is discussed below in the section titled "Kuhn versus Popper."
7. This point is made by many authors. The point that is often missed is that, in case such as the one discussed, the auxiliary statements are much less certain than the theory under test; without this remark, the criticism that one *might* preserve a theory by revising the A.S. looks like a bit of formal logic, without real relation to scientific practice. (See below, "Kuhn versus Popper.")
8. I have in mind saying 'the planets exert forces on each other which are more than .999 (or whatever) gravitational', rather than 'the planets exert *no* nongravitational forces on each other'. Similar changes in the other A.S. could presumably turn them into true statements—though it is not methodologically unimportant that no scientist, to my knowledge, has bothered to calculate exactly what changes in the A.S. would render them true while preserving their usefulness.

9. Hilary Putnam, "The Analytic and the Synthetic," in *Minnesota Studies in the Philosophy of Science*, Vol. III, ed. by H. Feigl and G. Maxwell (Minneapolis: University of Minnesota Press, 1962), pp. 358–97.
10. N. R. Nanson, in *Patterns of Discovery* (Cambridge: Cambridge University Press, 1958).
11. Thomas S. Kuhn, *The Structure of Scientific Revolutions*, Vol. II, No. 2 of *International Encyclopedia of Unified Science* (Chicago: University of Chicago Press, 1962).
12. Louis Althusser, *Pour Marx* and *Lire le Capital* (Paris: Maspero, 1965).
13. I use 'orbit' in the sense of space-time trajectory, not just spatial path.
14. By 'contingent' I mean *not physically necessary*.
15. 'Logical probability' is probability assigning equal weight (in some sense) to logically possible worlds.
16. This has been emphasized by Professor Max Black in a number of papers: e.g., "Self-supporting Inductive Arguments," *Journal of Philosophy* 55 (1958), pp. 718–25; reprinted in Richard Swinburne (ed.), *The Justification of Induction* (Oxford Readings in Philosophy, Oxford University Press, 1974).

Retrospective Note (1978): A Critic Replies to his Philosopher

Popper's reply[1] to my criticism consists of two main charges: (1) that I misrepresent him to such an extent that I must not have read his main book, *The Logic of Scientific Discovery*; and (2) that I commit an outright logical blunder. Both of these charges are false and unfounded.

The Charge of Textual Misrepresentation

Thc charge that I misrepresented Popper's doctrine itself rests on two claims: that I say of Popper that he neglects the need for *auxiliary statements*; and that Popper in fact talked about auxiliary statements at length (under the name 'initial conditions') in *The Logic of Scientific Discovery*.

The first claim is false: nowhere in my essay does there appear one sentence which says Popper denies the existence of, ignores, or neglects auxiliary statements. In fact, my section on the Kuhn-Popper debate talks explicitly about Popper's treatment of auxiliary hypotheses (which is (1) that they are part of 'the total system' under test; and (2) that one must not preserve the total system by *adjusting* this part—to do so is a 'conventionalist stratagem' and *bad*). What I *did* say is that Popper *blurs the distinction* between auxiliary statements and theory, and this, I still maintain, is true.

The second claim is also false. The auxiliary statements I was talking about, the ones I was taking as examples and on which my argument turned, were *not* 'initial conditions'.

'Initial conditions', in Popper's sense, are *singular* statements (as he stresses again and again in *The Logic of Scientific Discovery*). Moreover whenever he treats the question of their testability; he treats them as *verifiable* ('true basic statements', in his terminology).

The auxiliary statements I gave as examples were:

(1) The solar system consists of only the following bodies (list).

(2) *No* nongravitational forces (or gravitational forces from outside the solar system) are acting on the solar system (to a certain small ε of accuracy).

Both of these statements are *universal* statements, not singular statements. Neither can be verified as a basic statement can—indeed, to verify the second one would *already* have to know the true theory of gravitation! Popper's charge of textual misrepresentation is unfounded, and, in fact, I was very careful to present his doctrine *accurately*.[2]

The Charge of Logical Blunder

What I contended in my criticism is that as scientists actually use the term 'theory' (and, as argued, they *should* use it), Newton's theory of universal gravitation is *not* falsifiable: only its conjunction with the two auxiliary statements just listed is falsifiable.

Popper claims that this is a logical blunder. His proof that this is a logical blunder—that U.G. is falsifiable *without* A.S. (auxiliary statements)—*is a quotation from me,* a quotation in which I say we would give up U.G. if the world started acting in a 'markedly non-Newtonian manner'.

Now the logical situation is precisely this: *any trajectories whatsoever of all the observed bodies are compatible with U.G. without A.S.* Moreover, this is so for a number of reasons:

(1) U.G. without A.S. says nothing at all about what *non*gravitational forces there might be! By assuming nongravitational forces perturbing the system, we can account for any trajectories at all, even if U.G. is true.

(2) Even if we assume the system is acted on only by *gravitational* forces, we can still account for any trajectories at all, to any finite degree of accuracy, by assuming gravitational fields *in addition* to the ones caused by the observed bodies (e.g. there might be bodies too small and too rapid to be observed which are so massive that they give rise to significant fields).

Of course, such *ad hoc* assumptions as would be required to preserve U.G. if the trajectories did 'crazy' things (e.g., if we got *square* orbits), would be enormously *inductively implausible*— which is why we would give up U.G. in such a case. But I was not conceding that square orbits (or whatever) would *deductively* falsify U.G.—which is what Popper takes me to be conceding. The logical blunder is his, not mine.

Main Point

Since the reader has my article available, I do not have to expand on what I actually said there. The main points are two: that Popper's prohibition on saving a successful theory by modifying the A.S. is *bad methodological advice* (as Imre Lakatos and others also pointed out); and that successful predictions *can* confirm a theory plus A.S. even when they are not potential falsifiers in Popper's sense. My distinction between a theory in the canonical sense and A.S. is not the same as Popper's distinction between theory and initial conditions, as has already been pointed out, but it is closely related to, if not quite the same as, Lakatos's distinction between a 'theory core' and a 'protective belt'. The importance of such a distinction has become widely recognized in recent years.

Notes

1. See "Initial Conditions," in P. Schilpp, ed., *The Philosophy of Karl Popper* (La Salle, Ill.: Open Court, 1974).
2. For example, I say that Popper says that a 'theory' implies predictions. 'Predictions' is a nontechnical word which covers what Popper himself calls *instantial sentences* (conditionals whose antecedent and consequent are both basic sentences) as well as basic sentences, and the claim that a theory by itself implies instantial sentences occurs in many places in *The Logic of Scientific Discovery*. I did, unfortunately, write 'basic sentence' instead of 'instantial sentence' in the article in a number of places.