The Popper Phenomenon

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Popper has been the most prolific, wide-ranging and well-known of recent philosophers of science, and the most influential among scientists. In philosophy he has also unfortunately been made into a cult figure, like Wittgenstein; disciples have vied for the master's mantle and doctrines have been scrutinized as much for heresy as for error. This is a pity. Philosophy of science is a hard enough job without complicating it with personalities, patents and package deals. So is the job of integrating it with the rest of philosophy, dominated as it still is in this two-cultures country by a pervasive ignorance of and distaste for the sciences. Both jobs have been made harder by Popperian polemics, and the disrepute they have brought philosophy of science into among philosophers at large.

This matters because of the importance of much Popperian work. Much of it is true, much of it is original. It would have been still better, and had more deserved influence, had it been better integrated with other work in modern philosophy, in fields ranging from the foundations of statistics to the philosophy of language. Even as it is, there is no denying how much poorer philosophy, and especially philosophy of science, would have been without Popper's work and that to which, one way and another, it has given rise.

The appearance of a Schilpp volume or two on Popper is therefore apt and welcome, in that it both signals Popper's achievements and is to some extent an occasion for our mutual enlightenment.[1] However, I should be failing to exhibit an appropriately critical rationality if I did not draw some attention to the defects as well as the merits of the piece, which in many ways embodies characteristic Popperian qualities. There are interesting contributions from distinguished scientists and other theorists whom Popper has stimulated in their work. In many ways the remarkable influence Popper's work has had on the practitioners of the subjects he has philosophized about is the best testimony to its value. There is also here a useful variety of philosophical comment and criticism. But there is also too much hagiography and too much sectarian warfare. And Popper's passion for explaining himself at length has really got rather out of hand. Less bibliography, these volumes run to 1200 pages, which is more than Einstein or Russell needed; and one would not have thought their work so much less noteworthy than Popper's. Not that in fact there is that much more in these volumes, it is just repeated more and at greater length. I suspect the

[1] Paul Arthur Schilpp (ed.), *The Philosophy of Karl Popper* (La Salle, Illinois: Open Court, 1974), 2 volumes, xiv + 1323 pp., $30.00.

editor's prefatory question 'Would anyone wish to eliminate a single sentence [of Popper's]?' was less rhetorical than he meant it to seem. The work could profitably have been pruned of at least 200 pages. Some very familiar doctrines are here laid out again at length, not once but several times: in the *Autobiography*, in the more descriptive essays, and in Popper's replies. The degree of redundancy and editorial laxness displayed in the finished product is worthy of Reidel. And on the whole the layouts are not new, the arguments neither more nor less compelling than they were before. The principle that what Popperians say three times must have increased verisimilitude is not persuasive.

Take Popper's attitude to induction, a central point of Popperian method and mythology. Popperians find us obtuse who do not see that Popper has solved the problem of induction. The feeling is mutual. We find Popper on induction like, as the *TLS* once put it, a runner on the starting line crying 'I've won! I've won!' Ayer here repeats some long-standing objections to Popper's solution, of which Popper again fails to see the force. For me to re-rehearse them now would be to flog a very dead horse, but I suppose a reviewer must at least gesture at the skeleton.

Popper's brand of Humean scepticism is incredible; no one does or could live down to it either in everyday or in scientific practice. We all rely, and believe we should rely, more on well-attested laws and theories than on new or refuted ones. A century of electromagnetic theory has transformed radio from the merest speculation to the firmest of facts. A modern Moore could as well have appealed to radio waves as to hands to show the existence of the external world. No contractor whose transmitter fails can get away in court with a Popperian defence of its failure as merely demonstrating the scientifically falsifiable character of the bold conjectures underlying its design. On the contrary, he could easily be found culpably ignorant of well-established electromagnetic knowledge which it was his business to possess and whose application would have guaranteed success. That is a typical result of scientific activity. But how is it that we can expect to rely so—for our lives, as often as not—on such theories? Popper indeed admits that we have no *more* rational recourse than to rely on them. But the admission is pretty worthless, not to say disingenuous, since he gives us no *less* rational recourse either, and the whole point is that we have *more* reason to expect Mr Pye's transmitters to work than Mr Heath Robinson's.

Popper indeed is hot for rationality; but by divorcing it from reasons for anticipating one future experience rather than another, he deprives it of much of its import. Obviously we have such reasons, and the progress of science has given us many more, as everyone's actions every day attest that they believe. Why will Popperians not admit to such beliefs, which they reveal every time they turn on the light or use the telephone? As Carnap would say, none are so inductively blind as those who pretend they cannot see, and the pretence is the more ludicrous in those who visibly navigate by

sight. What the reasons I allude to are I do not know; that is the still unsolved problem of induction. In the end I expect inductions have to be justified inductively. Those who still feel this begs the question might perhaps ponder Susan Haack's recent *Mind* exposition of the deductive parallel. If the prospect of having to ban deduction too will not make Popperians take induction off the Index, I do not know what will.

Inductive blindness has admittedly prompted some good points in Popperian methodology. Their attempts to dispense with induction have led them to draw out many other important aspects of science. Its practice is indeed largely a matter of conjecture and refutation, and with theories couched in new terms it obviously must be; one cannot generate such new theories by generalizing singular statements couched in an old vocabulary. Such points need making, though they can be misapplied, as in Popperian polemics against inductive logic. Not that that particular point is original to Popper, as Medawar and others here remark, and as Popper readily concedes.

Likewise, Popper's emphases on testing and criticism, on the objectivity of logic and Tarskian truth, are all welcome antidotes to subjectivist, relativist and irrationalist trends in recent philosophy of science, even among erstwhile Popperians. But these admirable emphases can hardly be sustained without a little help from induction. There is neither strength nor virtue in a common-sense belief in objective reality divorced from the essentially inductive justification of beliefs, common sense and scientific alike, as to the details of that reality. The route from inductive scepticism leads, via the indeterminacy of translation thesis, straight to *Against Method*, the *reductio* of Popperian epistemology.

Lakatos thinks he has shown a rival route, to his 'methodology of research programmes'. Popper does not think so, and he is quite right (personalities apart, as they could more profitably have been kept in Popper's comments). The methodology of research programmes directs one to opt for progressive as opposed to degenerating research programmes. One cannot, however, tell at the time whether a research programme is progressive; that only emerges when the results are out. So the prescription is not much use unless it is accompanied by the inductive principle that recently progressive reasearch programmes are more likely than others to stay so in the near future. Otherwise the methodology is on a par with telling gamblers to put their money on winners; sound advice, no doubt, but not the slightest help to the practising punter. As a description of the history of good science, moreover, Lakatos admits that what he says is false, but has the cheek to claim that rejecting it on these grounds would be to apply the falsification criterion which his own methodology is designed to supplant. It should only be rejected, seemingly, if it would be a degenerate research programme to rewrite the history of science falsely as if it had been a history of research programmes. I fancy most historians would indeed

take deliberate falsification of history as a sign of degeneracy. Still, no doubt even that verdict from historians is defeasible by a progressive research programme in the history of the way histories of science have got written, showing their increasing tendency towards such falsification. And so on.

Kuhn's attempt to change the subject from the logic of discovery to the psychology of research fares no better in compensating the Popperian programme for its lack of inductive backbone. Nor does Popper's proposal, along with Quine, to replace traditional questions of epistemology with evolutionary speculation on the origins of our cognitive habits and the role of theorising therein. One must, I suppose, admire the ingenuity of these Humean evasions, even while wishing it less misplaced.

The whole apparatus of Popper's world 3 is just another such evasion. It is designed to rescue objective knowledge from Hume's sceptical clutches. Belief in the generalities of science must obviously be justified inductively if at all; therefore, for Popper, not at all. So if there is to be scientific knowledge, it cannot call for justified belief in what is known, since there is none. Therefore, since knowledge certainly does not call for unjustified belief, it does not call for belief at all. But if knowledge does not need belief, it does not need a believer. Belief (and perhaps its justification) being the only plausible personal constituents of knowledge, knowledge is thus liberated from the knowing subject. But if knowledge is not in people's heads or minds, where is it? Not in world 1, of physical things and events; not in world 2, of the mental. So it must be in a third world, Fregean but not timeless, of propositions, of problems and theories, of the objective contents alike of books and thoughts. World 3's population is initially created by us of world 2, but its inmates remain willy-nilly to plague or delight us and, through our deliberated actions, to affect even world 1, the world of physics.

Now this Foucaultian farrago is not just a harmless myth. It looks like a sort of explanation, a mechanism by which knowledge can grow in Hume's despite, which shows how our success in cognitive dealings with the world and each other is possible. But it is nothing of the sort; it is nothing more than a picture, a model in Duhem's derogatory sense, quite void of explanatory detail. Naming a supposed non-mental location for collective objective knowledge no more explains its possibility than my saying that induction is how we get knowledge of the future would explain the possibility of that. A name, old or new, for a set of problems is not the same as a solution to them. Now these problems do need solution, and there are philosophers working on them. The Davidson programme is one such approach to them, which promises to explain our knowledge in terms of the undeniable contents of world 1. Grice and Schiffer on meaning provide another such approach to explaining how knowledge can be communicated. Their theories may be wrong, but at least they have some content. World 3

word magic is no substitute for either of them, nor is it likely to make good their deficiencies.

In at least one respect, moreover, world 3 talk has proved positively misleading. Among its recent immigrants, Popper here includes social institutions. The trouble is that social institutions are a very mixed bunch. Language I suppose is one, and that may very well be in world 3 if anything is. But governments, unions, cultures and the like are also social institutions, and they certainly do not belong in world 3. Some recent writers on social science have read them into it, thus gratuitously reinforcing unfounded distinctions of method between social and natural sciences, based on the former having to theorize *inter alia* about people's values, interests and intentions. Popper is not responsible for others' misapplications of his ideas; but trying to decide in which of his worlds to put bodies of sentient problem-solvers does not conduce to clarity of thought about the social sciences.

However, perhaps Popperian social objects (using Quinton's term to pick things like unions out from things like languages) do belong in the Popperian world 3. Popperian work on the social sciences has displayed, in its combination of situational logic and methodological individualism, a curious evasion of the consequences here of a generally admirable and robust realism about theoretical entities. Given the irreducibility of social to psychological discourse, it is really no more plausible to claim that social objects are nothing but people than to claim that electrons are nothing but meter readings. I cannot avoid the suspicion that the needs of Popperian anti-Marxist polemic are what deny social objects their natural place as independent entities in world 1, where they belong. And since situational logic and anti-positivism prevent them from being mere logical constructions out of other world 1 and 2 entities, they have perforce to be accommodated among the metaphysical monstrosities of world 3.

Talking of metaphysical monstrosities, it has long seemed to me, as here to Kraft among others, that Popper still exaggerates the difference between his criterion of demarcation and the Vienna Circle's various attempts at a criterion of empirical meaningfulness. To be meaningful, after all, was simply to be capable of truth or falsity. The point of the positivist criteria was that if there is no empirical way of telling whether a statement is true or false, it is idle to credit it with empirical truth or falsity, and in particular to assess its role in discourse in such terms. That is not far off the rationale for insisting *faute de mieux* on falsifiability as the basic criterion for scientific status, where science is taken to be the paradigm of cognitive activity and an essential point of its utterances is to be at least candidates for truth. If Popper were not here so uncharacteristically concerned with the relative verbal virtues of the terms 'demarcation' and 'meaningfulness', he could perhaps have dealt with this minor historical point more briefly and more equably.

Some minor historical points, this time about propensity theories of probability, accompany some rather vulgar anti-inductivist rhetoric in Settle's essay. I should perhaps deal with a question of which Settle makes unduly heavy weather, since I am concerned in it. The question is whether propensities and probabilities are absolute or relative. Formally, Popper is right enough to prefer relative probability, on the ground that from it absolute probability can be defined. But there still looks to be a substantial question whether objective chances and propensities are absolute or relative. Take Settle's example of a loaded die. It has various chances of landing six in various gravitational fields, to which Settle concludes its propensity so to do is relative. Well, the die has similarly various accelerations under various applied forces, but it does not follow that its inertial dispositions are relative to the forces applied. On the contrary, Newtonian mechanics tells us to conjoin them into one inertial mass. A similar statistical theory of the influence of gravity on loaded dice would similarly tell us how to conjoin all its various propensities, i.e. its dispositions to yield various chances of landing six when thrown in various circumstances. (As the theory of radioactivity in fact tells us how to conjoin a radium atom's various propensities to decay in various times into one half-life.) The plain fact is that the loaded die has all these propensities all the time, just as in having an inertial mass it has simultaneously an indefinitely large number of dispositions to accelerate under various applied forces. What the die cannot do is to display all these dispositions simultaneously, since it cannot simultaneously have all these forces applied to it or be thrown simultaneously in all these different gravitational surroundings. So the chance of the die landing six varies with its surroundings, just as its acceleration does. But that no more means its propensity, its bias, varies or is relative to those surroundings than its inertial mass does or is. Settle fails to see this because, despite his disclaimer, he does, like Popper and most other propensity theorists, systematically confuse propensities with the chances that display them. And the mistake matters, since it mixes up the various chances a biased die can yield in various surroundings with the possibilities of changing the die's bias, and these are causally quite distinct matters. Just as, in Newtonian mechanics, the die's inertial mass is an absolute property of the die which may nevertheless be caused to change by other external or internal changes, so is its bias. And there is all the causal difference in the world between different surroundings displaying different dispositions, whether they be statistical or inertial, and changes of surroundings causing those dispositions themselves to change. If Settle had not mixed these things up he would have seen that my own 'preference for ascribing propensities to individual systems' does not, as he thinks, 'conceal a confusion'.

The other essay on propensities, by Suppes, makes a similarly minor point that I should perhaps tackle, since Popper's reply does not. Suppes complains that propensities do not provide an acceptable interpretation of

the usual probability calculus, basically because the calculus cannot be derived from, and hence explained by, the propensity theory as it can from classical, frequency and personalist theories. Well, it is not absolutely necessary for a theory of probability to explain the calculus, though no doubt it is the better for doing so. Carnap's theory of logical probability is in this respect an improvement on Keynes', just because Carnap measures the logical relation involved by the degree of belief it warrants in the conclusion that it relates to completely believed premises. Carnap is thus able to take over personalist arguments for the probability calculus being the measure of degree of belief whether warranted or not, whereas Keynes simply had to impose the probability axioms on his logical relation (where measurable) without any independent justification. In that it explains why logical probability should satisfy the standard axioms, Carnap's is indeed the more satisfying theory. But if its failure to do that were the only thing wrong with Keynes' theory, it would not be in bad shape. Similarly, there are many more serious questions to be raised about propensity theories than whether they explain their use of the standard probability calculus. Still, for what it is worth, most propensity theories do explain that. Mine does because I characterize the chances which display propensities by the corresponding degrees of belief in the outcomes involved. Thus I, like Carnap, can simply take over personalist arguments for using the standard calculus to provide a measure of degrees of belief. Other propensity theorists characterize propensities in terms of hypothetical limiting frequencies which repeated displays of the propensity would tend to produce. These theorists can likewise help themselves to familiar frequentist arguments for the standard calculus. All this seems to me very obvious; indeed I felt that in *The Matter of Chance* I had laboured the point too much, to the exclusion of more serious matters. But Suppes' essay shows perhaps that I did not labour it enough.

There is a multitude of other topics taken up in these two volumes, on which I am not well qualified to comment. Collectively, the essays certainly do justice to the range of Popper's thought, if they do not all individually do justice to its content. Perhaps I should conclude by extracting from the *Autobiography* a positive confirming instance of Popper's thesis of the fallibility of observation. He gives his version of the famous story of his visit to the Cambridge Moral Sciences Club in 1946. It differs in most material respects from the recollections of an eyewitness as unimpeachable as Popper himself. On the rival account, Popper's remark about Braithwaite's poker was made apropos of rationality, not morality, and was not made to Wittgenstein at all but some twenty minutes after Wittgenstein had left the room. Russell, furthermore, far from being one of the main speakers in the ensuing discussion, was probably not even present. Well, perhaps Popper's account is right, perhaps that of my distinguished informant is. It can hardly matter to a Popperian, whose principles would anyway not allow

him to be justified in believing either story. What does seem beyond dispute to the rest of us is that, as the minutes declare, 'the meeting was charged to an unusual degree with a spirit of controversy'. That sounds right.

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