

SYMPOSIUM

Karl Popper, 1902–1994

Learning from negative instances

On 17 September 1994, Karl Popper died at the age of 92. He was described as the official opposition of the Vienna Circle, the philosophical club which in the inter-war period espoused the then popular doctrine of 'logical positivism'. His relations with that club were 'friendly-hostile', to use the term with which he liked to characterize the relations between scientific researchers. He is the last of that generation (unless it is Carl G. Hempel, who sees himself as too young to belong there). The public aspect of Popper's friendly-hostile relations with his Viennese peers was unfortunately more hostile than friendly. Somehow, philosophers have managed to keep the accounts open for too long. The end of the era is the time to close them so as to get on with the job at hand, since the intermingling of personal affairs with objective knowledge is unhealthy. Yet most of the leading heirs of the Vienna Circle still coast around the issues raised by Popper, and hence they can neither ignore him nor quote him correctly. If past evidence is reliable here, then this will alter just about now. Is it?

Popper is known as the philosopher who offered a solution to the problem of induction *without* denying the validity of David Hume's classical critique of all theories of induction. The problem of induction is often put these days as: is the past a reliable indicator for the future? Or, will the future be like the past? The wording is not very fortunate, as we all agree that the past cannot be revived, that I will never be young again. The traditional way of stating the question was: are generalizations from experience reliable? This is better but still not very happy, as we all know from our childhood experience that some generalizations are most unreliable. A much better restatement of the same idea is: can we rely on the generalizations that rest on many instances and have no instance to the contrary? This is better, of course, but still not satisfactory, since there are generalizations that are erroneous and yet it is not easy to find evidence which contradicts them, especially those generalizations that rest on stereotypes. Moreover, science regularly suggests theories, such as about atoms and their

The death of Karl Popper last autumn received considerable coverage in the media. Commentators were unanimous in heralding the significance of his work. Yet there was little discussion of what his heritage might be. Attention was focused on his alleged influence on the New Right in Britain (Sir Keith Joseph is said to have deposited a copy of The Open Society in the library at 10 Downing Street) at the expense of his work as a philosopher. BBC 2's Late Show broadcast a programme in which Paul Johnson declared Popper to be 'one of the reasons why the West decided to fight the Cold War', but which altogether omitted to mention that he was a philosopher of science. This was, as John Naughton put it in The Observer, 'a bit like doing an obituary of Pope John Paul II without mentioning his religious affiliation'. Yet Popper's views about science are closely connected to his politics via his conception of rationality.

Here we publish four views of Popper's writings from people who came into contact with him in one way or another during his heyday in the 1960s. Joseph Agassi was Popper's Research Assistant, then colleague, at the London School of Economics. He has applied his own interpretation of Popper's views to the history of physics since the beginning of the nineteenth century. He is currently Professor of Philosophy at the universities of Tel-Aviv and York, Toronto. Jerry Ravetz was formerly Professor of Philosophy at Leeds University. He now runs The Research Methods Consultancy. Bernard Burgoyne was Popper's Research Assistant during 1967/8. He is presently director of the Centre for Freudian Analysis and Research, Middlesex University, and a practising psychoanalyst. Robin Blackburn taught politics at the LSE in the 1960s. He is the editor of New Left Review.

properties, that are not generalizations at all. The best way to word the problem of induction, then, is to refer to the fact that science consists in learning from experience and developing theories: how can evidence help us learn about the world at large? How is theoretical learning from experience possible? How can evidence lend support to theories?

This wording makes Hume's critique of all theories of inductive support unanswerable, as it is the true observation that there is no valid inference leading from particular evidence to universal theories. All efforts to find such inferences, especially the theories of probable inference, were already refuted by Hume. Popper agreed that learning from experience is not inferences from evidence to theories; that is to say, it is not inference supporting any theory. But, he argued, evidence can and does lead to the negation of theories; it leads to their refutation – such inferences are classically a part of deductive logic. The novelty of Popper's idea lies not in the assertion that refutations are valid inferences, but in the assertion that they are instructive, that they are indeed what makes science progressive and exciting.

This is the heart of Popper's philosophy of science: learning from experience is not by positive but by negative instances. This is a view that is hard to take seriously, as evidenced by the fact that quite a few of his self-portrayed followers deny that this is his teaching despite the fact that he said it so many times and never took it back. It must be admitted, though, that in his discussions of the details of his view, and in his response to incredulous comments on it, he did make two minor additional assumptions: that some *ad hoc* hypotheses may be tolerated for a while and that science requires some corroboration of its theories. These minor assumptions do not amount to a reversal of his view that learning from experience is by refutation, though they reduce the power of his theory even if they make it more credible. At the very least, one ought to cite him correctly, and preferably discuss his theory separately once in its austere version and once with its ancillary additions.

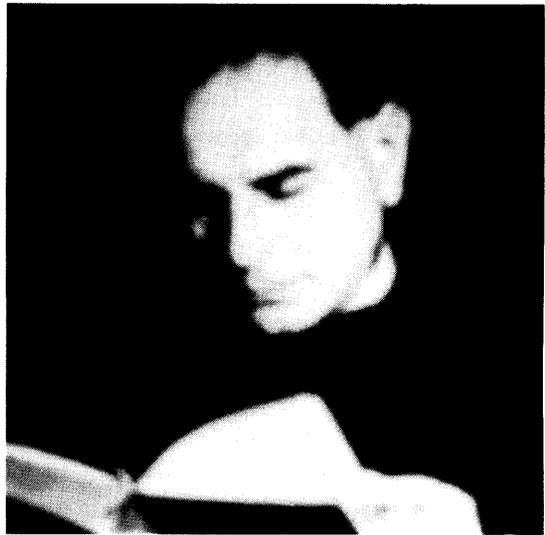
Demarcation: theories or languages?

The most prevalent misrepresentation of Popper's views sidesteps the whole issue just raised and centres upon his characterization of science. For he solved the problem of the demarcation of scientific theories in line with his solution of the problem of induction: a theory is scientific, he said, if and only if, on the supposition that it is false, there is a practicable way of trying to refute it. This theory was systematically misrepresented by his peers from the Vienna Circle, especially Rudolf Carnap: whereas Popper studied the demarcation of scientific theories, Carnap alleged that what he (Popper) studied was the demarcation of the language of science. The last time I heard Carl Hempel, the leading heir of the Circle, he repeated this misrepresentation, and he refused to accept my correction, even though he knows that Popper repeatedly complained about it. The difference between the two is very clear: the negation of a scientific theory is traditionally viewed as not scientific, whereas the language of science includes the negation of every statement that it includes. Thus the statement that is the negation of Newton's theory is not scientific, both by tradition and by Popper's view, but that very statement belongs to the same language to which Newton's theory belongs. Hempel refuted Carnap's variant of Popper's view and declared that Popper's view had been superseded. This kind of reasoning was never corrected by anyone except Popper and his close associates.

The views that Carnap and Hempel endorsed were those they had borrowed from Ludwig Wittgenstein. They presented their own variant of Popper's view as an improvement upon the original, one that Popper himself refused to endorse out of excess pride and/or a blind spot for the greatness of Wittgenstein's contribution. It is therefore impossible to assess Popper's contribution without reference to Wittgenstein's, even though, logically speaking, they are worlds apart. Indeed, the difficulty of admitting the significance of Popper's contribution is that this town is too small for both of them. The central view of Wittgenstein's that Popper spent so much effort refuting is now admitted to be erroneous: both Wittgenstein and Popper disliked metaphysics, but whereas Wittgenstein declared it ungrammatical, Popper always vehemently denied that. The latest classic on the Vienna Circle, Alberto Coffa's *To the Vienna*

Station, though it is on the philosophy of language, and though it praises Popper much more than is usual, still manages to ignore both the theory that all philosophy is meaningless and the unfair attribution of it to Popper. This will not do.

Popper's contribution to social and political philosophy is in line with his philosophy of science and is the more significant of the two – both politically and intellectually. According to him, democracy is the regime that allows the public peacefully to correct their mistakes, such as the election of an undesired ruler. His best-known idea is that the theory of historical destiny is not scientific. He never said it is false, only that it cannot be worded in a manner clear enough to be put to the test of experience. For to cohere with known facts, it must for ever remain vague, since, notoriously, future science is in principle unpredictable (or else it will be current science) and the impact



of science on society is tremendous. Of course, this is true of the doctrine in its generality. One may develop a doctrine concerning historical inevitability that will be testable; and then it will be easily refuted empirically by this very observation. For example, Karl Marx predicted that capital would become increasingly concentrated and that this would make the socialist revolution both easy and unavoidable. The prediction was based on the observation that industrial physical plants tend to be ever bigger, because of economies of scale. This, argues Popper, was overturned when the electric dynamo was invented. One has the choice, then, between considering Marx's theory as general or specific: the first option renders it metaphysical; the second empirically refuted. And so we can either take Marx's theory to be scientific and refuted, or non-scientific. It is not empirically confirmed, Popper adds, unless it is so worded that it can be only confirmed (if true), but not refuted (if false), so that it is then not scientific though its confirmations may give the impression that it is. Marx's followers either ignore Popper or distort his ideas. Those who ignore him, observes Ernest Gellner, do so while acknowledging that Marxism is not empirical, so that they have altered the status of the theory and now advocate it irrationally.

Every item mentioned here can be elaborated on. Much was added to it by Popper and his followers, and more can be added to that. Moreover, the most important issue has been left out of this discussion: the views of Popper may well be criticized, and some of his better followers did criticize them. But the point made here has to do with historical truth rather than with philosophical truth. Those who disagree with the historical description I have offered will do the public service by explaining why. Those who agree with it should not conceal their agreement. This will clear the air and allow the debate to continue fruitfully.

Joseph Agassi

Last of the great believers

Karl Popper's significance lies more in the sphere of educated popular culture than in academic philosophy. As a scholar, he was forever an amateur, with all the naivety and enthusiasm of that status. The defects of his works were well known to colleagues, and he cannot be said to have created a strong school in any field. But for his few permanent disciples, together with some influential converts and the broad audience who read something significant in his pronouncements, he was more in touch with real issues than all the orthodox professors in the various disciplines he ventured into.

His career demonstrated the benefits of starting young and living long. His basic ideas were formed when he was still a student during the First World War, and, although he waited

decades for recognition, when it finally came, after the end of the Second, it was real and gratifying. No other academic philosopher of science has been made a Fellow of the Royal Society. Such an imprimatur on his life's work reflects its contradictory character: a fierce commitment to ruthless criticism and self-criticism, which was somehow always focused on targets external to his milieu. Thus Marxist economics was criticized for being either unfalsifiable or falsified, but the same scrutiny was not applied to General Equilibrium Theory.

I have elsewhere (in *RP* 37, Summer 1984) analysed Popper's 'confessions', of how he was led to the realization that the positivist conception of science admitted all the sins of anti-scientific and anti-rational movements, and how the fantasized popular image of Einstein provided him with the symbol that he needed for scientific integrity. He was, of course, totally unclear on whether the search for refutations was a matter of logic, practice or motivation, and on whether it was characteristic of all actual science or only of the best. Some impressively counter-intuitive accounts of science have been produced by those who interpreted him as literally describing what is done by ordinary scientists.

Had he not produced the massive *Open Society* just when he did, it is possible that he would have remained an obscure Central European philosopher of science, effectively exiled at the Antipodes, and forever confused with that Vienna Circle whose teachings he did so much to criticize and correct. But in spite of displaying all the sins of amateur historical writing, where sources are merely quarries for quotes to buttress arguments advanced uncritically, the work had a great influence. For it provided a coherent answer to the question, what is really wrong with Marxism? In Britain after the war, socialist ideas had great popularity. There were, of course, many discordant and conflicting tendencies, but Marxism had been rehabilitated despite its notorious corruption under Stalin. The common struggle against the Nazis had discredited the political enemies of Marxism because of their previous tendencies towards encouragement of Hitler. Yet in the late 1940s, Stalin was again showing his tyrannical side in full force, intellectuals who had tried to co-operate with Communists were retreating, bruised, and the time was ripe for a new, untainted statement of an anti-collectivist philosophy. The concept of 'open society', and an infamous tradition of its philosophical enemies, were just what was needed. Popper became the guru for a generation of young British centrist political intellectuals, and his message of anti-dogmatic thinking reverberated widely for decades afterwards.

His political philosophy was, for Popper, a contribution to the war against Fascism; while his abiding love was the philosophy of science. In this endeavour, in spite of recognition at the highest levels, his career was less satisfying. It took quite a few years for him to begin to attract pupils of great promise at the LSE; and when they arrived at the end of the 1950s, they were so intense and committed that life in the Popper group was rarely peaceful. Splits and defections, in the best *Mittleeuropäisch* style, were the order of the day. And his own methodological mission had scarcely got underway when it was challenged by the representative of a new consciousness about science, Thomas Kuhn.

The Kuhnian challenge

With Kuhn's disenchanted vision of science alternating between dogmatic normality and non-logical revolutions, the edifice of Enlightenment assumptions about knowledge began to crumble. Popper's own personal reaction was peculiarly weak. In his comment on Kuhn (in the rational reconstruction of the famous 1965 encounter), he did make a good point that 'normal science' is an enemy of science and of civilization. But he then went on to inveigh against dogmatism in science teaching, using as evidence a remark made by a colleague some thirty years previously! It is as if, before this particular occasion, he had not really thought much about what ordinary researchers and teachers of science, either natural or economic, actually do. That consideration might have dulled the prophetic vision, which, however confused, was always intense.

Popper can be seen as the last deep philosopher who espoused science as the embodiment of

virtue. Indeed, his great philosophical insight, abandoning verification for falsification, can be seen as a heroic gesture of jettisoning Science as True in order to rescue Science as Good. The whole Einstein fable displays Popper as a moralist rather than as an epistemologist, and Popper himself never undertook a historical case study of a refutation in science. It is almost as if there was a faith that what a real scientist does is to surrender his pride and open himself to defeat. Then he is saved and proved correct.

Thus Popper's historical significance is that of the last of the great believers in Science. His significant publications in epistemology and methodology did not extend beyond the 1960s. His cause was taken up, with greater sophistication but less effect, by Lakatos. He could just about ward off the Kuhnian challenge, but the duel with Feyerabend proved too much for him. Soon afterwards came the deluge of deconstructionism, so that by the time of his death Popper was nearly as much ancient history as was Carnap.

Yet it could be that after Popper has been given a rest, he will have something to teach us after all. Current socio-philosophical analyses of science teeter on the edge of nihilism, and it has become recognized that a content-free conception of science can be used by nasty people as well as nice ones. Like any philosophical quest, the search for the essence of genuine knowledge is never-ending. Popper's vivid reminder that science is and must be a moral activity is a permanent contribution to our understanding.

Jerry Ravetz

Question and answer

How does anyone involved in the field of scientific research orientate their work? The answer generally assumed in a Britain innocent and unaware of Karl Popper's philosophy was by means of an inductive logic, or by means of inductive techniques. The first great change brought about by the translation of Popper's work from the 1930s, and the growing influence of his English-language writings, was that this solution, so doubtful and so deceitful in its recipes, and so feebly able to explain the development of creative science, was first weakened and then discredited as an account of the structure and practice of natural science. The elements of Popper's alternative solution were: that the field of science is structured and guided by a logic of question and answer – otherwise known as deductive logic, or Socratic dialectic – in concert with an account of the resolution of problems perceived as generated by hypotheses, guesses, presuppositions, and their consequence. This was the most fundamental realignment of the methodology of the sciences to be introduced this century.

Agassi addressed a second version of our question to Popper's work in the late 1950s. In this new form it introduced the problem of the relation of philosophy to the dialectic of science. Does the dialectic of question and answer in science produce its own direction, or is it given an orientation by something external, by philosophical presupposition? Popper, like Freud, had proposed that science has no need of any external philosophical world-view. But when Popper perceived the problems of philosophy as having their roots in science, Agassi sought a contrary thesis – that the direction given to science is provided by circumjacent metaphysics. This led Agassi to call such a direction-giving structure a 'metaphysical research programme', and his work tried to show such programmes as operative in the history of modern science. Lakatos tried to relocate priority within an 'enlarged' scientific terrain by putting forward the term 'research programme' irrespective of the scientific or extra-scientific nature of the guiding themes built into it.

Problems are raised by these questions that advance the problem-situations of logic and ethics. Popper had never suggested that mathematics works through a process of criticism of conjecture: Lakatos did. The function of a proof, according to Lakatos, is to give you a better idea of how to go about establishing the falsity of what has been proved. His work reorientates

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the question of the internal structure of a proof, just as Agassi's work had reformulated the investigation of the ethical status of questioning. This school of Popper–Agassi–Lakatos is of great importance: in terms of a modern conflict of schools, Popper's team has Socrates and Buber, Pappus, Proclus, and Polya among its antecedents; Kuhn, on the other hand, has some modern sociology. The Popperian tradition has the richness of centuries behind it, and it shows. The structures of modern science determine the access that people have to what is real, and so these questions are no mere bagatelle.

My objection to Agassi may now perhaps be clear. When he says that Popper's contributions to social philosophy, when weighed against his philosophy of science, are 'the more significant of the two', and not only 'politically' but also 'intellectually', he confuses a secondary literature, often verging upon the second-rate, with a tradition that introduces, or reintroduces, revolutionary elements into the theory of science. There are two discourses in Lakatos – a philosophy of mathematics and a philosophy of science – the second markedly inferior, apart from some inspirational terminology carried over from

his philosophy of mathematics. Similarly, there are two discourses in Popper – the second, a commentary on Marx, Freud and social science generally, is of an appalling poverty when compared to the discourse on Faraday, on Maxwell, on Einstein, on the major shifts and massive reorientations within the field of modern science.

The 'social sciences' are in a dire state compared with the mathematical sciences today, and the direr for having ignored Popper's philosophy – but Popper's philosophy of natural science, not his work on social theory. All these 'social' domains suffer from not having incorporated Popper's 'recommendations on technique' for practitioners of science. I have nothing to say about Marx; about Freud, however, I want to say a lot – the classical and Popperian tradition of problem-solving, of finding a way through, is not distinct from psychoanalysis.

Socratic transference

The Socratic programme is to set up conditions in which one of the partners is led to loosen their attachment to their opinions – in the case of psychoanalysis, to sexual opinions, sexual phantasies. In this respect, at least one aspect of analytical technique recapitulates the problems and procedures of Socratic discourse. Psychoanalysis, however, goes a little beyond Socrates: there are certain problems that cannot be attacked – their suppositions are hidden away in the unconscious, inaccessible (except via the procedures proposed by the hypothesis of free association). There also exists a pervasive field of false connection, of fictive, fabricated realities that cover over these gaps in consciousness. This is no slight problem in epistemology, and addressing it is why the research frontier in psychoanalysis is nowadays concerned with 'structures' and structuralisms.

Transference is a process of finding ways through – of resolving aporias, of being able to start the formulation of otherwise inaccessible problems. Love was born, according to the mythologies of Plato's Symposium, from a uniting of poverty with Poros – a finding of ways through what would otherwise be only a passionate suffering. Love in the transference thus becomes an aspect of problem-solving activity. Lacan regularly formulated his theoretical work as a series of problems and aporias; psychoanalytical technique is itself the attempted resolution of an aporia.

Freud's proposal was that psychoanalysis needed no orientation given to it – either in terms of its development of theory, or in terms of the direction of clinical practice – from outside the field of psychoanalysis. In particular, in response to a series of proposals put forward by Jim Putnam in the United States, Freud denied any such relation of dependency between psychoanalysis and philosophy – moral or otherwise. Instead, Freud claimed, psychoanalysis is guided by the research programme internal to natural science. I am here translating the Freud–

Putnam term 'Weltanschauung' by the Agassi–Lakatos term 'research programme', and in this context, it is no bad translation. With respect to ethics, Agassi has Putnam's position, finding guidance for science externally. Freud assumed that a psychological science which took into itself the problems of psychoanalysis would be a science radically changed. One aspect of this transformation is the move from the intersubjectivities of the I and the other to Freud's dialectic of Ego and It.

Bernard Burgoyne

Popper and the new left

Without being in the least aware of it, Popper should be reckoned a formative influence on the New Left in Britain and the emergence of Anglo-Marxism. Popper's work divides, fairly neatly, into two: on the one hand, there were crusading anti-historicist polemics; on the other, research into the philosophy of science. Since hostility to Marxism was the motive force of his life, how could Popper possibly be said to have influenced any section of the Left, let alone any current that impinged on Marxism?

The reason this was possible is that Popper focused his critique very exactly on the deterministic and positivist Marxism of the Second International. In the 1950s and some way into the 1960s it was thought that one of the defining features of Marxism was a belief in historical inevitability. It is this doctrine which Popper was concerned to refute. Those of us who studied social science and philosophy in the LSE of the 1960s were fascinated by Marxism, but not at all attracted to the pathos of predestinarianism.

Few 'sixties' revolutionaries could have reached university without reading *The Open Society and its Enemies* or *The Poverty of Historicism*. One soon discovered that the Hegel scholarship in these books was open to serious question. More generally, Popper's utterly partisan approach to the history of philosophy was deemed a damaging flaw. But despite all this, the central argument against deterministic historical analysis was quite acceptable, although if Popper had not written fundamental works on the philosophy of science then his influence, even subliminally, would have been negligible.

Popper's work on falsifiability is now often appraised in terms of its registering of the vital importance of the empirical test. But in an intellectual climate dominated by empiricism – that of the English-speaking world of the 1950s and '60s – Popper's classic arguments were seen as drawing attention to theory-construction and undermining a naive belief in the simple accumulation of data, from which theory would eventually arise, like steam out of a kettle. This anti-empiricist impetus in Popper's work was evident in the writings of those influenced by him, above all, Paul Feyerabend. The latter's brilliant and iconoclastic work was, of course, a development beyond Popper, but it continued to owe something to Popper's problematic – and a lot to dialogue with Imre Lakatos, another errant pupil.

While Popper appealed to the prestige of science in order to discredit other forms of thought, such as Marxism or psychoanalysis, Feyerabend helped prepare the ground for that distrust of the reasoning of science that became a hallmark of the New Left. So, while there is no reason to deny Popper's conservatism, the fact remains that his thought touched the concerns of Anglo-Marxists at a number of points. When he chose the title *The Poverty of Historicism* he was echoing Marx's *The Poverty of Philosophy*. When Edward Thompson chose the title of his polemic with Althusser, *The Poverty of Theory*, Marx furnished the model. But if we bear in mind the central themes of this work, then we may suspect that a Popperian echo was not entirely unwelcome.

Robin Blackburn