Popper's Views on Natural and Social Science
by Colin Simkin

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inductionists suppose, aim for the most probable theory in the sense of one which has the highest probable content. If we did, we would have to be satisfied with with a theory which had almost no content, and so hardly any worth for purposes of explication or application. A tautology would exactly fit the bill.[12]

[12] *The Logic of Scientific Discovery*, Ch. 10.
Norbert Weiner makes much the same point. In *The Human Use of Human Beings*, he wrote; 'the more probable the message, the less information it gives. Clichés, for example, are less illuminating than great poems.' (p. 31).

7. THE CONJECTURAL METHOD

To use one's eyes as far as they can be used, and then to guess the way forward - the art of science is no more than that; but it is quite enough, nevertheless, because the point is to guess right, that is to say, so that the result may be tested, and confirmed by experience.

Knut Wicksell

We know that science cannot grow out of empiricism alone, that in the constructions of science we need to use free invention which only a posteriori can be confronted with experience as to its usefulness. This fact could elude previous generations, to whom theoretical creation seemed to grow inductively out of empiricism without the creative influence of a free construction of concepts. The more primitive the status of science is the more readily can the scientists live under the illusion that he is a pure empiricist. In the nineteenth century many still believed that Newton's fundamental rule 'hypotheses non fingo' should underlie all healthy natural science.

Albert Einstein

Popper's rejection of all forms of induction does not lead him, like instrumentalists, conventionalists or pragmatists, into scepticism about our ability to reach towards objective truth through critical reasoning. To repeat what has been said above, he believes that all forms of life have some ability to improve understanding of their environments, and to deal with some objective problems which it presents to them, by exploration and by learning through trial and error. Humans are greatly helped in this process by the development of language and writing. Through these basic inventions people have become able to formulate problems, to examine them critically, and to discuss them with others, both directly through conversations, letters or seminars, and indirectly through journals or books, thereby developing their own critical faculties and reason.

This method of exploration by trial and error, and of learning from mistakes is, of course, most highly developed in science. It has thus made possible great advances in our understanding of the natural phenomena in World 1 and, to a lesser extent, of the psychological and social phenomena in World 2. Popper calls this development, 'the method of bold conjectures and ingenious and severe attempts to refute them'[1], or 'guesswork

[1] *Objective Knowledge*, p. 81.

controlled by rational criticism'. The conjectures are hypotheses or theories, tentatively advanced as solutions to scientific problems. From the standpoint of logic, it does not matter how the conjectures arise; what matters is whether they can be discussed and tested, logically and empirically, by other people.

The key logical problem here is the relation between singular statements of fact and the universal statement of a theory. Hume had emphasized that there is no logical path from the truth of any number of singular statements to the truth of any one universal statement. Popper has shown that neither is there a probability path. (see § 6 below.) But he also points out that there is an old logical path from the falsity of any singular statement to the falsity of any theory from which it can be deduced. In logic this result has been known as *modus tollens*, the mood which denies, and it has the form: if p then q, but not-q, therefore not-p. Here p could denote a scientific theory (e.g. planets follow circular orbits) and q a factual statement that could be deduced from it (e.g. the planet Mars has a circular orbit). If experience, or testing, shows that this factual statement—it could be a prediction—is wrong (e.g. Mars has an elliptical orbit), then it would follow that there is something wrong with the theory from which q has been deduced.

What this means is that, although we can never prove that a scientific theory is true or probable, it could be disproved by a contradiction between anything validly deduced from it and an accepted statement of a relevant empirical test and, in the case of a probabilistic theory, subject to a standard of significance for test results. To repeat what has been said in § 1, scientific theories are *falsifiable*, always open to refutation by empirical tests. It is this, Popper insists, that distinguishes them from other types of statement, and more particularly from metaphysical theories. Although some theories appear to be, or claim to be, scientific, they are not open to disproof by testing and so are basically metaphysical. We have seen that Popper puts Freud's psychoanalysis in this category of pseudo-scientific theory because any test of it could be interpreted as a confirmation, and also Marxism after its adherents evaded factual contradictions by re-interpreting both theory and evidence[2].

He makes, however, as we also saw in § 1, a basic distinction,

[2] *Conjectures and Refutations*, Ch. 1. Arthur Koestler, a lapsed Marxist, concluded that 'the mentality of a person who lives inside a closed system of thought, Communist or other, can be summed up in a single formula: He can prove everything that he believes, and he believes everything that he can prove. The

not always noted by critics, between the *falsifiability* of a theory, which is a logical matter, and its *falsification*, which is more doubtful and complicated[3]. In order to understand the distinction we have first to consider what a theoretical deduction or prediction involves. The first point to note is that often such a deduction is made, not from a single generalization, but from a theoretical system of interconnected generalizations, together with initial conditions. A prediction of the time and height of a future tide, for example, uses Newton's laws of motion and gravitation, and also some laws of hydrodynamics. There will, too, be other generalizations and facts which are presupposed in what Popper calls 'background knowledge', and more especially those which are connected with measurements and construction of instruments for making observations. In this example, they include measures of time and space—clocks, calendars, theodolites, barometers, etc. Finally there are initial conditions which pin down the theoretical system to a specific type of situation; e.g. the shape of a coastline, the depth of adjacent ocean and the strength of prevailing winds.

Because more or less background knowledge is needed for our observations, they are all 'theory impregnated'; there is no such thing as a bare fact[4]. All empirical statements are theoretical although some, to be sure, are more theoretical than others. This means that it is impossible to prove the truth of any test statement about an observation. Its accuracy can always be challenged, or that of the background knowledge upon which it depends. A theory can never, therefore, be decisively falsified; acceptance of its refutation requires informed judgement that tests have shown the contradicting statement can be accepted as true[5].

That is one difficulty. Another is that accepting the truth of a disproving test statement does not enable us to assert that it disproves any particular generalization within the theoretical system unless the other generalizations, and also the initial conditions and background knowledge, can all be taken for granted. As Popper says: 'we can, indeed, falsify only systems of theories and . . . any attribution of falsity to any particular state-

closed system sharpens the faculties of the mind . . . it produces a scholastic, Talmudic, hair-spliting brand of cleverness which affords no protection against committing the crudest imbecilities.' *Arrow in the Blue* (1952), p. 340.

[3] *Realism and the Aim of Science*, pp. xxix–xxv.

[4] *Objective Knowledge*, Ch. 2.18.

[5] *The Logic of Scientific Discovery*, pp. 109–11.

ment within such a system is highly uncertain'[6]. Duhem and Quine, we saw, had made the same point. (Objections, on this ground, to Popper's demarcation criterion between science and metaphysics are discussed in § 23 below.)

An example would be Kepler's third law of planetary motion which gives the square of the time taken by any planet to orbit the sun as proportional to the cube of its distance from the sun along the major semi-axis of this orbit, the ratio being the same for all planets. This formula can be deduced from all three of Newton's laws of motion, his law of gravitation, and the initial condition that the mass of any planet is negligible compared with that of the sun. Contradicting observations could strike at any of these laws or at the initial condition.

For all these reasons the advance of scientific knowledge has not been, and cannot be, free from controversy. That is all to the good if it leads to keener criticism and more severe testing of theories; errors could then be more effectively detected or exposed and better conjectures could be stimulated. The spectacular growth of modern scientific knowledge shows that controversy over its theories and tests has been mostly to the good for scientific progress if not, indeed, essential for it.

There are a number of steps in the critical examination of a theory. The first is to check its logical consistency and its amenability to empirical testing. A theory becomes more exposed to testing if we can minimize metaphysical elements, and ensure that auxiliary hypotheses do not reduce its empirical content and hence exposure to testing. The second step is to compare the theory with other theories in order to judge whether it would provide a better explanation if it were to survive empirical testing. Better here means explaining more precisely the same phenomena, or explaining, because of greater generality, not only the same but additional phenomena, especially newly discovered phenomena. In this connection, Popper has emphasized that a good new theory should depend on some 'simple, new, and powerful unifying idea'[7] which connects hitherto unconnected phenomena or relations between phenomena. The third step is to subject the theory to as many severe empirical tests as possible, tests which could include both predictions and practical applications.[8]

It has to be stressed, because of persistent misunderstandings

[6] *Conjectures and Refutations*, p. 187.
[7] idem, p. 241.
[8] *The Logic of Scientific Discovery*, pp. 32–33; *Conjectures and Refutations*, Ch. 10.v.

on the part of some of his critics, that Popper does *not* say that a theory is to be necessarily rejected because of a single adverse test. At the very least, the test statement would itself have to be thoroughly checked by other scientists before it could be accepted as contradicting the theory. Although from a logical standpoint a theory is falsified by a single counterinstance, in practice it would not necessarily be abandoned for that reason alone. If it had given useful results and if no better theory had yet been proposed, it could be provisionally retained, using caution in areas where the theory's application had become doubtful. Nor would it be hastily abandoned if it promised to have great explanatory power, and if there was hope that it might be so improved as to avoid apparent falsification in ways which did not reduce its explanatory power. Scientists need judgement and tenacity, as well as inspiration, in deciding, not which theory is most probable, but which is most worthy of further development and criticism[9]. A scientist who gives up his theory too easily in the face of difficulties may never discover its inherent possibilities. It is important to realize that theories can be overthrown only by better theories, not by tests alone.

Popper's account of the conditions for scientific progress has, as he says, some resemblance to the Darwinian theory of evolutionary progress through the survival of the fittest. Fitter theories, in terms of their explanatory power and robustness to testing, replace weaker ones and, in their turn, give way to stronger ones through a process of elimination by criticism and testing. Theories are thus seen as competing for acceptance and, just as the genetic changes which lead to new species are still mysterious, so are the inspirations which lead to new conjectures. These parallels are, of course, connected with Popper's 'evolutionary epistemology' to which references have been made in previous sections[10], and which is more fully considered in § 12.

It has been objected that scientists do not characteristically follow Popper's idea of searching for falsifications of existing theories but are rather content to go on applying theories which they have come to trust. This criticism received powerful support from Thomas Kuhn's book, The *Structure of Scientific Revolutions*, in which he introduced the notion of *paradigms*, which were defined as 'universally recognized achievements that for a long

[9] There are many scattered references to the points of this paragraph; see, for example, *The Logic of Scientific Discovery*, Ch. I.5; *Realism and the Aim of Science*, pp. xxiii, xxvi, 70, 187–89.
[10] *Objective Knowledge*, p. 261.

time provide model problems and solutions to a community of practitioners'. He used this notion to distinguish 'normal science',which relies on paradigms, from 'revolutionary science, which involves 'paradigm shifts'. A narrower definition was 'problem solutions', meaning basic experiments, such as Galileo's supposed demonstration that bodies of varying weight dropped from the Tower of Pisa all reached the ground at the same time, or the famous imaginary two-slit experiment in quantum mechanics to show the wave-particle nature of light. Such an experiment alters scientists' views about the phenomena which they study, and leads to a theoretical system which rests ultimately on their subjective consensus about the significance of the paradigmatic experiment. Once reached, this consensus is conservatively held, as 'normal' scientific work seeks to strengthen rather than to challenge it.

Popper admits that he had overlooked such 'normal science' so that he found Kuhn's description of it 'an eye-opener', although doubting whether it was significant before the mass production of scientists began after the First World War. He questions, too, whether it is right to label routine science as normal science because 'the very idea of routine is uncharacteristic of science'. There is, however, a danger that routine may take over —that 'science becomes a community of workers held together by routine'. For Popper, 'this will be the end of science as I see it'[11].

But he questions Kuhn's distinction between 'normal' and 'extraordinary' periods in science – between periods when there is an established routine of puzzle solving and those in which a crisis is followed by a revolution. Even in a quiet period there can be minor discoveries through critical rejection of established routines or theories by applied scientists. And in more revolutionary periods routines are not overthrown and replaced; no departure from standard astronomical procedures was needed to confirm Einstein's prediction of Mercury bending sunlight nor was it for years afterwards.

Kuhn held, too, that although 'paradigm shifts' occurred paradigms are 'incommensurate' because there is no standard for comparing them. They reflect different views of an unknowable world which has different aspects; Aristotle's physics is bad Newtonian physics but should not be compared with it because Aristotle gave a different meaning from Newton's to such basic

[11] *The Philosophy of Karl Popper*, pp. 1145–46.

concepts as motion, particle, time and space.[12] Truth and falsity, accordingly, hold only within a paradigm, not between paradigms. That, of course, is not Popper's view which is more fully explained in the next section, and is further contrasted with Kuhn's claim in § 11.

[12] See John Horgan, 'Profile: Reluctant Revolutionary', in *Scientific American*, May 1991, pp. 14–15.