

MICHAEL POLANYI AND THE PHILOSOPHY OF SCIENCE

The Viewpoint of a Practising Scientist

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3 THE VIEWS OF SOME SELECTED PHILOSOPHERS IN RELATION TO THOSE OF POLANYI

3.1. *Karl R. Popper*

3.1.1 *The personal relationship of Popper with Polanyi*

Polanyi's personal relationship to Popper was an interesting one. They had many things in common; both came from the same intellectual background in the Austro-Hungarian Empire at the beginning of this century, the one from Budapest and the other from Vienna; both opposed the positivist philosophers; both were passionately opposed to totalitarian societies and considered that personal freedom of choice is essential for the growth of science. However, they rarely referred to each other's work. Polanyi subtitled PK 'Towards a Post-Critical Philosophy'. He considered that overemphasis on doubt as a pathway to truth can lead to nihilism and totalitarianism, and that the retention of the humanist metaphysical concepts such as truth and justice is essential for the continuation of human freedom. In particular he had been horrified by the fact that such human considerations were ignored in the then Soviet Union, on the pretext that it was more important to further the predictions of the (falsely) scientific and (falsely) objective doctrine of Marxism. It has been said that Popper and Polanyi fell out over this. Popper considered that on the contrary metaphysics is the source of totalitarianism and that this must be opposed by doubt i.e. by maintaining a spirit of criticism within an open society. [9]

Popper's *magnum opus*, *The Logic of Scientific Discovery* [10], was published in English at about the same time as PK, but had some years earlier been published in German so that its content is independent of Polanyi's views. His general approach to science has been strongly commended by distinguished scientists such as Medawar, Eccles and Bondi.[7, 11]

3.1.2 *Induction and deduction; the hypothetico-deductive method; verification and falsification.*

One of Popper's staffing points concerned Francis Bacon's advice to scientists (natural philosophers as they were then called) to study nature by collecting related observations with a view to discerning patterns of occurrence that call for explanation. By this means theories could be generated by the process of induction. It was the contention of David Hume[12] that it was not logical to assume (as scientists do in the absence of evidence to the contrary) that a regular pattern of behaviour would continue without change. Popper strongly endorsed this view and further claimed that the manner in which one obtains ideas, from experimental data or otherwise, is not a solely logical process; it is often inscrutable. He proposed that the best procedure was to use one's imagination in order to select a hypothesis and then logically to

deduce the consequences, for scrutiny in comparison with existing and future experimental data, using critical analysis. This procedure is known as the hypothetico-deductive method. Hume's analysis also had the consequence that observations of further regularities could not in themselves constitute reliable verifications of a hypothesis. Popper, however pointed out that new observations which *falsified* the theory would in principle lead to a logical step forward by requiring a new theory to be devised. His advice therefore was that scientists should strive to falsify their theories in order to make progress.

Of course scientists do use attempted falsifications to distinguish between alternative hypotheses that could account for the observations in question. However, once the stage has been reached that the preferred hypothesis is accepted as a tentative theory, Polanyi's views appear to differ from Popper's in two ways. First he claims that an apparent falsification can never be totally decisive; there is always the possibility of an erroneous experiment or observation or of a wrong expectation derived from the theory. Therefore he says that in practice scientists do not and should not, immediately reject a theory because of an apparent anomalous observation, provided that the theory is strongly supported on other grounds. They should simply proceed with further relevant experimentation. Once again it is seen as a question of judgement on the part of the individual scientist. Second he claims that scientists do not in practice explicitly attempt to falsify their theories for to have found one of promise is an achievement in itself which should be built upon.

Gregory[13] has distinguished between two aspects of induction, both said to have been appreciated by Bacon. The first looks for repeats of the same phenomenon in essentially the same circumstances; this seems to be the aspect of induction that Hume had in mind. The second looks for the same type of phenomenon in different but related circumstances. Scientists make much useful progress by attempting to confirm the tentative theory by the latter means. This is a valuable activity in itself as it explores new areas, increases useful knowledge, and brings to light the further understandings latent in theory. Success in these respects also enhances the value of the theory itself. I would add the comment that most theories have limited ranges of applicability and in such cases a continued exploration of a wide range of circumstances finally reveals their limitations. Popper's preferred goal of falsification is thereby achieved but at the end of a profitable sequence of explorations carried out with a very different attitude and for a different reason. After all, a theory which has not been frequently confirmed within its area of competence is hardly worth the effort of falsifying.

Popper asserts that even experimental observations are theory-laden in the sense that they attract our attention because they either fit in with what we already consider to be the case, or because they are unexpected and uncomfortable, with the potentiality to cause us to modify our views. This is true, but the relationship between theory and experiment in sciences is of the chicken and egg type. I consider that a better formulation would be to say that there is a component of theory in all observations. Popper's attitude to induction, while formally correct, should not be taken as downgrading the importance of experimental or observational investigations. Polanyi's expressed view in PK is to agree that induction is indeed a far from reliable process, but that it nevertheless undeniably remains a valuable and principal source of information that has led to the formulation of many successful theories.

3.1.3 *The hypothetico-deductive method and the provisional nature of scientific theories*

There would, I believe, be wide agreement within the scientific community that Popper's hypothetico-deductive method is the correct formal method for assuring scientific progress.

One formulates (guesses) an idea that could explain interesting experimental or observational data and then explores its scope and consequences by deductive processes. However, the idea (the equivalent of a premise in a mathematical exploration) is itself provisional and, although this procedure can lead to progress, it cannot be claimed to lead to certain knowledge. Popper emphasises that in this sense all scientific theories are provisional. This is so even although many individual items of scientific knowledge, such as the contention that a benzene molecule has six carbon and six hydrogen atoms, or that Newtonian mechanics provide an adequate theoretical underpinning of the game of billiards, seem likely to be secure from refutation.

3.1.4 *Bold testable hypotheses – the way ahead?*

Popper found particular inspiration from the way Einstein developed the theory of relativity and thereby showed that Newton's classical mechanics – at that time considered to be an indisputably true scientific theory – was not valid in the more general case. The theory was derived, as Polanyi also discusses in PK, partly as a result of a paradox to do with electromagnetic theory that Einstein found on imagining himself travelling at the speed of light, and partly as a result of a perceived asymmetry between the classical theoretical treatments of two branches of electricity and magnetism. The theory was arrived at through thought-experiments rather than through the more normal procedure of considering the results of laboratory experiments, a fact that possibly accounts for Popper's seemingly less regard for the experimental approach. Einstein's theory was based upon a bold hypothesis with testable implications and Popper strongly advocated that such an approach to science should be generally adopted. This certainly becomes a necessity when there is a major problem, possibly involving the overthrow of a very strongly entrenched theory, as in Einstein's case. But it would seem to be unnecessary and inappropriate in the context of the application of a well-established theory to unexplored research areas. There the appropriate approach would seem to be to put forward hypotheses that constitute appropriate incremental changes to existing wider-standings. Popper's advocacy of the bold-hypothesis approach implies that he principally thought in terms of a heroic type of science, of which Einstein's development of the theory of relativity is a fine but atypical example. i.e. that he was really thinking in terms of what Kuhn later categorized as 'revolutionary' rather than 'normal' science. This distinction will be discussed below.

3.1.5 *The differing views of Popper and Polanyi*

There is no denying that the hypothetico-deductive approach advocated by Popper provides a formal philosophical framework for science that is strongly supported by a number of distinguished scientists. Polanyi's views differ principally in stressing additionally the importance of informal procedures within science. These are dependent on the personal judgements of individuals which in turn are guided by the tacit and explicit forms of their relevant knowledge. Polanyi re-emphasises the informal but continued importance of induction, based upon the mental analysis of experimental or observational data, in contrast to Popper's seemingly negative attitude to this. On falsification Polanyi stresses the importance of the intermediate exploratory steps as fruitfully extending our understanding of the natural world, even although the theory being evaluated will ultimately show its limitations. Some people seem motivated to obtain as much new knowledge as possible from a given amount of information, whereas others seem particularly concerned to avoid error in the analysis. Polanyi and Popper show these respective different emphases. This being taken into account, it seems not unreasonable to claim that they share a common goal despite the mutual differences expressed in their lifetimes.

3.2. Thomas S. Kuhn

3.2.1 Introduction

Thomas Kuhn is both an historian and philosopher of science whose seminal work *The Structure of Scientific Revolutions*[8] was published in 1962. A second edition appeared in 1970 and included a postscript that answered comments on, and criticisms of, the first edition. This second edition what is referred to below as SSR. After the analysis of the historical evidence relating to a number of critical stages in the progress of science, Kuhn made a distinction between what he termed 'normal' and 'revolutionary' science. Kuhn highly praises PK in SSR and his analysis clearly relates to a number of aspects of science discussed at length by Polanyi. These include the role of the scientific community or its sub-communities as the repositories of accepted understandings and also as the consensual source of scientific agreements, the importance within the community of the agreed body of scientific knowledge at a particular time, and the difficulties that can arise between protagonists of traditional and innovative understandings.

3.2.2 'Normal' and 'revolutionary' science

Polanyi described how an essential aspect of the training of a student scientist is to become very familiar with the theories and experimental methods extant at that time, such as Newton's classical mechanics plus Clerk Maxwell's theory of electromagnetism (and the associated experimental techniques) which constituted the framework of physics at the end of the nineteenth century. These agreed features Kuhn described collectively as constituting the dominant paradigm of the time. Once well established, the theories and methods incorporated within such a paradigm can be used efficiently to extend understandings into many related fields. Kuhn terms this type of period as one of 'normal' science. He describes the individual steps in making such progress as the solving of 'puzzles' in relation to the original paradigm; I prefer to describe these steps as successful explorations using the paradigm. This is because the endeavour is not just an intellectual one, but is also of value in terms of the resulting new understandings.

However, there can come a time when experimental phenomena are encountered that are difficult to account for through appropriate additions to the existing paradigm. Originally, as Polanyi has described, these would tend to be put aside as anomalies that may find future explanation within the paradigm, or be noted for reinvestigation in hopes that the experimental procedures were erroneous. If, however, repeated efforts fail to account for the anomaly, or a related set of such anomalies, then interests begin to focus on these. Different individuals begin to make suggestions that could account for the anomalies but require the modification of one or more features of the original paradigm. In Kuhnian terms a 'revolution' is in the offing which can become manifest if an alternative theory emerges that accounts for the anomalies and also shows promise of providing modified explanations of most of the phenomena successfully accounted for by the original paradigm. Once a field of science has become mature, e.g. physics after Newton, chemistry after Priestley, Lavoisier and Dalton, or biology after Darwin, the strong paradigms that ensue provide satisfactory explanations for very wide ranges of phenomena. The overthrow of a paradigm that possibly generations of scientists have used with confidence is no light matter. and many will hang on to the original in hopes that it can yet accommodate the anomaly. At this level a successful new theory requires a reconstruction of the field from new fundamentals. As a result it becomes increasingly difficult for the two opposed groups, the 'radicals' and the 'conservatives', to understand each other and no logical common ground can be found to choose between the

alternatives. Kuhn says that it is as if the two sets of protagonists are talking to each other in different languages, or in related languages in which some of the same words are used with different meanings.

An alternative to the two-languages model for the controversy is to liken the situation to alternative mathematical developments based on different axioms. Again it would be very difficult for the one group to accept the views of the other if they disagree on the axioms. This type of model provides a good description of the 'revolution' from classical mechanics to relativity theory; for the former is based on Euclidian geometry whereas the latter makes use of Riemannian geometry. Specific physical differences in these cases are that in Newtonian mechanics mass is independent of velocity, and energy and mass are separately conserved, but under relativity theory mass becomes dependent on velocity (when the latter approaches the speed of light) and only energy/mass is conserved according to Einstein's famous equation $E = mc^2$.

Under these types of circumstances Kuhn describes the two theories as being incommensurable and that for further progress it becomes necessary for one of the sides to win over the scientific community by persuasion. This suggests the possibilities of the effects of personal influences, political manoeuvring etc., and has led to misunderstandings to be discussed below.

3.2.3 The transition period

As the new proposed paradigm has much work to do in order to show how it can alternatively explain the wide range of phenomena already accounted for by its predecessor, it can take a considerable period – even up to a generation – for it to become widely accepted. If all this is successfully achieved, the new more flexible paradigm finally takes over from its predecessor and is now used as the basis for further fruitful explorations. The transition from classical mechanics to quantum mechanics provides a good example of this process.

Classical mechanics had been unsuccessful, despite multiple efforts, in attempting to account for the distribution of energy in the electromagnetic spectrum as a function of temperature. Max Planck showed that this could be explained if it were assumed that energy is emitted in certain discrete packets rather than continuously. He became a worried man because he realised that, if such an assumption were to be generally applicable, much of classical Newtonian mechanics would have to be recast. Such proved to be the case, but in return the more general quantum mechanics that ensued enabled an understanding to be reached of the physics of the microscopic world of atoms, molecules, electrons etc. (of which the electromagnetic spectrum was one manifestation) which was not possible before.

The new theory also provided an explanation for the detailed electromagnetic spectra of individual atoms or molecules, an area which thereby became a major field of productive research in this century with important applications in chemistry, biology and medicine as well as in physics itself.

The new paradigm also provided perspectives on, or (as it had to) understandings of, the phenomena previously satisfactorily accounted for by classical mechanics. In Popperian language the anomaly that caused the problem could be said, to have led to the falsification of Newtonian mechanics. This use of the term 'falsification', however, needs qualification in the sense that the equations of classical mechanics reappeared as special cases of those of quantum mechanics when masses are sufficiently large, e.g. are of a magnitude encountered

in the normal macroscopic world, and the appropriate small packets of energy (the quanta) are small enough to give an approximation to the classically assumed continuous distribution of energy. Newtonian mechanics remains an economical way of accounting for the behaviour of the macroscopic world even though it is not capable of describing the behaviour of the microscopic world. If an engineer wishes to design a new aircraft, the computers, even today, are kept busy using Newtonian equations. If the aim is to design a new alloy for the aircraft, or an improved fuel, then the evaluation of these atomic or molecular properties requires the use of quantum mechanics. We saw above that Newtonian mechanics also re-emerges as a special case of relativity theory for problems concerned with the normal everyday world. Finally quantum mechanics and relativity were combined into a single theory by Dirac, and in the process led to an understanding of the theoretical basis of chemical bonding.

3.2.4 The differences between 'normal' and 'revolutionary' science: Does relativism limit theory-choice in science?

Kuhn's 'revolutionary' science undoubtedly differs from his 'normal' science in the scale of the change that has to be made to the theoretical framework as a result of the 'revolution'. In SSR Kuhn pointed out, quite correctly, that the accounts of the development of science given in the normal student texts are highly simplified in their depiction of a seemingly smooth and inevitable progress of science. Such a picture is very much at variance with the detailed studies of 'revolutionary' type transitions by historians. In this context he argued at some length that progress across the boundary between one paradigm and its successor is of a nature different from that involved in changes within 'normal' science, where the adjustments fall within an existing paradigm. Nevertheless he concluded that rational scientific progress undoubtedly does occur across paradigm boundaries, as instanced above in the account of the replacement of classical mechanics by quantum mechanics and relativity theory, despite the much greater disjunctions that occur there. It should be noted however that Popper's hypothetico-deductive analysis and Polanyi's informal method of problem-solving appear to be applicable to both 'revolutionary' and 'normal' science. The use of the revolutionary analogy has caused some sociologists to conclude that such incommensurable theory-differences, because they cannot be settled on purely logical grounds, are susceptible to political types of persuasion. If in this respect science is no different from other activities then, they suggest, relativism has to be accorded to scientific theories, as is assumed to be the case in post-modern accounts of many other fields of thought. However, as elsewhere in science, these theory disputes are in fact settled in the Polanyian informal manner involving first personal, and later group, evaluations of the merits of the proposed new paradigm with respect to the overall scientific evidence. In addition to producing convincing explanations for the original anomalies, the new paradigm has to relate equally well to the wide field of experimental or observational phenomena satisfactorily accounted for by its predecessor – a very demanding requirement in terms of evidence from the natural world. In the postscript to the 1970 edition of SSR Kuhn expresses his unhappiness with the suggestion that, even in his 'revolutionary' cases, relativism limits ultimate rational choice between scientific theories. In *Meaning*, Polanyi and Prosch expressed pleasure at the extent to which Kuhn in SSR supported the views set out in PK. It seems likely that Polanyi would have been as equally opposed as Kuhn to the suggestion of relativism within scientific theory-choice. But he certainly would have agreed with the sociologists that science is very much a social activity that should be studied as such. A perceptive sociology of science is to be welcomed.

3.2.5 A further look at the revolutionary/normal science distinction.

Philosophers are well known to be unhappy about the informal nature of classifications and in

this context a further look at the meaning of 'revolutionary' might shed light on the nature of the wide area of 'normal' science. For the revolutionary analogy to hold convincingly it is necessary for the earlier long-standing paradigm to be given up when the new one replaces it. This clearly applies, for example, to the conceptual changes such as were involved between the views associated with the names Ptolemy and Copernicus and Newton (the end of epicycles); Priestley and Lavoisier; 'God' and Darwin; Newton/Clerk Maxwell and Einstein; and Newton and Planck.

However, many very important advances in science involve high gain but no loss of an original paradigm. Examples include Dalton's and Mendeleev's contributions to chemistry; Faraday's bringing together of electricity and magnetism; Mendel's contributions to genetics; Rutherford's discovery of the nucleus-based atom; the Crick and Watson elucidation of the structure of DNA, etc. All of these advances were received with enthusiasm and with little or no resistance and so it is seen that the description 'normal' encompasses much of the vital content of science. The word should not be equated with the meaning of second-rate or of minor importance. Another point of interest with respect to 'normal' science relates to cross-correlations between the different sciences within the 'normal' science umbrella. As has been seen earlier in this paper, there is a hierarchy within the sciences in the sequence physics, chemistry and biology. Advances in experimental techniques in chemistry can lead to major conceptual advances in biology, or those in physics can do the same for chemistry and biology. For example, during this century the physical techniques of X-ray diffraction and spectroscopy have so advanced the efficiency of the determination of molecular structure that the scope of chemistry has been transformed; this in turn now makes major contributions to the study of the very large molecules of biology. As an example, the Crick and Watson elucidation of the structure of DNA was brought about by the application of the routine-in-physics technique of X-ray diffraction to the demanding problem of the structures of large key molecules in biology. Thus 'normal' work on an experimental technique in one branch of science can lead to a major advance in another.

4. CONCLUSIONS

Within their publications there appears to be disagreement between Polanyi and Popper over the importance or otherwise of the concept of falsifiability in relation to theory-advancement. However, it has been argued here that the principal differences between them relate to their respective emphases on informal and formal procedures in their progress towards similar goals. In this sense their two approaches are complementary. To coin a metaphor, perhaps it could be said that, in order to understand science in action, it is necessary to clothe Popper's (formal) skeleton with Polanyi's (informal) flesh-with-a-human-face.

Polanyi's emphasis on informal procedures has also reinstated induction, derived from experimental or observational data, as an important source of hypotheses for subsequent evaluation using Popper's hypothetico-deductive procedure. The importance of the experimental aspect of science has also been shown in the discussion of 'normal' science given earlier. An additional merit of experimentation is that thereby unexpected observations are frequently made which lead research into quite new directions. This is termed 'serendipity' and, as I have myself experienced, it provides a good example of how nature itself continuously intervenes to control the direction of scientific progress. Serendipity itself does not contradict the Popperian principle about the theoretical content of observations because, as Pasteur long ago pointed out, such discoveries come to minds prepared for something else. Serendipity also shows that, as Polanyi emphasises, successful science is very far from a solely formal and logical pursuit of knowledge. Most of the philosophers of science pay

selective attention to the role of theory rather than experiment within science. Polanyi, with his examples of experimental advances cited in PK, and Hacking in his book *Representing and Intervening*[14], are exceptional in giving explicit consideration to the important role of experiment in advancing the frontiers of scientific knowledge. Others could profitably follow suit.

There is much common ground in the descriptions of science given by Polanyi and Kuhn, with the latter making the additional distinction between 'normal' and 'revolutionary' science. In this paper we have given particular attention to the former in order to make clear that 'normal' science as conceived by Kuhn also contributes in a very important, and not just routine, way to the advancement of science. Kuhn, in the second edition of *SSR*, has expressed concern that his classification of rival theories as incommensurable at a time of 'revolutionary' paradigm-change has led some social scientists to propose that relativism applies to scientific theories. I, too, have argued against this point of view and feel confident that Polanyi would also have been very much in agreement with Kuhn's concern.

The author's overall conclusion is that Polanyi's ideas in the area of the philosophy of science deserve to be very much centre-stage with those of Popper and Kuhn, and not in the symbolically-referred-to margin as is often the case.

References

9. Gillot, J., and Kumar. M., (1995) *Science and the Retreat from Reason*, Merlin Press, London.
10. Popper, K.R., (1968) *The Logic of Scientific Discovery*, (revised), Hutchinson & Co., London.
11. Magee, B (1975) *Popper*, Fontana/Collins, Glasgow.
12. Hume, D., (1739-40) *Treatise of Human Nature*. Book I. Part III, quoted by Popper, ref. 9.
13. Gregory, R.L, (1984) *Mind in Science*, Penguin Group, London.
14. Hacking R., (1983) *Representing and Intervening*, Cambridge University Press, Cambridge, England.

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<http://www.ch.bme.hu/chemonet/polanyi/>

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