Popper and Kuhn on the Evolution of Science

by Pat Duffy Hutcheon as published in *Brock Review* (1995) ol. 4, No. 1/2, p.28-37.

ABSTRACT

Popper and Kuhn have tended to view their models as radically contradictory in their epistemological premises and practical implications. However, it is possible to identify key similarities that may point to a promising complementarity concerning the defining characteristics of science and its evolutionary role. Although Kuhn believed the function of a scientific test is to affirm increasing verisimilitude, while Popper maintained that only falsification can ensure validity and reliability, both recognized the prerequisites of commensurability and testability. And they agreed on the essentially cumulative nature -- not of scientific *theory*, but of the *facts* which a fruitful theory unearths.

KEY TERMS: Pragmatism -- Logical Positivism -- explanations -- facts -- falsification -- verification -- commensurability -- "normal science" -- natural selection of hypotheses -- scientific revolutions -- disciplinary matrix -- paradigm -- ideology -- proto-science -- the method of scientific inquiry

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Three of the most creative minds of the twentieth century were involved with the same subject. John Dewey, working at the century's turn, refined and elaborated the Pragmatism of Charles Peirce. This was a philosophy of science built on Hume's work which gradually won out over the last vestiges of the older Realism once so valiantly defended by Bertrand Russell. With the help of Karl Popper, in early mid-century, an updated version of Dewey's approach eventually challenged the previously popular Logical Positivism as well. Popper extended and refined this new perspective by providing insights on how scientific knowledge accumulates through a process akin to natural selection. Thomas Kuhn has expanded on it also -- showing how science, as a social endeavor, follows a path marked not by an even continuity of small steps, but by great leaps forward interspersed with plateaus.

Although these two major contributors to modern philosophy of science were critical of one another's work, it may be that their approaches are more complementary than contradictory. It can be argued that both men have contributed valuable insights concerning the nature of science and the way in which scientific knowledge evolves -- even though they were driven by different questions. Popper sought to spell out the difference between ideological and

scientific theories, and to explain why only the knowledge generated by the latter is cumulative. Kuhn has been interested in shedding light on the historical process by which ideology can evolve into science, and on how the scientific *explanations* of one era (but not the *facts* unearthed by them) can become obsolete. Popper's key concept is the determining criterion of "falsification", operating as the mechanism of natural selection in the scientific sphere. Kuhn's is the power of the disciplinary matrix, or "paradigm", the social source of its emergence, and the pursuit of "normal science" which it makes possible.

The roots of all these ideas have been around for a long time. Poincare once defined science as a rule of action that succeeds. And we find a youthful C.P. Snow musing as follows: "For the scientific process is nothing more nor less than a hiatus between 'pointer readings': one takes some pointer readings, makes a mental construction from them in order to predict some more...and if the prediction turns out to be right, the mental construction is, for the moment, a good one. If it is wrong, another mental construction has to be tried (Snow 1934, 170)." In describing the sudden success of the quantum mechanics model in physics in 1925, Snow wrote that "anomalies ceased to be anomalies, with this new clue; facts which had puzzled us before now fitted in completely (Snow 1934, 172)." He marveled at how physics had apparently suddenly fallen into place.

As a student of Snow's generation, Popper became intrigued by the issue of the demarcation between ideological thinking and that which is scientific. He looked to the prevailing philosophies of science for answers, and, although he felt a degree of affinity with both Pragmatism and Logical Positivism, he decided to stake out an original position somewhat different from both.

Popper did not formally acknowledge that he was building on Pragmatism. However, he claimed the originator of that philosophy, Charles Peirce, as his intellectual hero, and a close reading of Popper's work indicates that he had only one major disagreement with his hero's ideas. This had to do with the crucial "test for truth" on which every philosophy must take a definitive stand. The Pragmatists had declared that the test of verisimilitude is what "works" to permit dependable expectations of results. In other words, a proposition expressing a regularity in human experience has a tentative "truth claim" to the extent that it is instrumental in allowing us to control the consequences of our acting on the basis of it.

Popper agreed that " it is not incorrect to say that science is ... an `instrument' whose purpose is ... to predict from immediate or given experiences to later experiences, and even as far as possible to control them (Popper 1934,100)." But, for him, scientific theories were not *merely* instruments -- nor were they merely linguistic devices, as the Logical Positivists liked to claim. "They are genuine

conjectures about the world," he said (Popper 1983, 110). He differed from the mainstream science of his time, however, in that he considered this a statement of faith, and neither a Kantian *a priori* nor a logical prerequisite as Russell had claimed.

Popper also believed that the Pragmatists were being inconsistent in defining their criterion in terms of *verifiability*. David Hume had already demonstrated that verification is not possible by the test of experience. The only thing that *can* be accomplished conclusively by the empirical process of science, concluded Popper, is *falsification*!

He went on to explain that any scientific theory must be in the form of a universal statement capable of being refuted by experience. It has to be possible, by deducing implications of the theory and attempting to match them against observations, to argue from the singular assertion, or description of the observed event, to the falsity of the original statement. This is strictly a deductive reasoning process, even though it proceeds in a seemingly inductive direction: from the data of experience back to the hypothesis. In this manner only, concluded Popper, is the logical dilemma posed by Hume resolved.

Popper claimed that it is indeed possible to conclude that some propositions are more reliable than others, simply because they explain more things more comprehensively; and because, over time, they have stood up better to disciplined attempts to refute them. Their "fitness" has been demonstrated by the very fact of their survival. It follows that the aim of the empirical method can never be to test for the *truth* of theories, but to corroborate them through failed efforts to prove them false. It must be "not to save the lives of untenable systems, but, on the contrary, to select the one which is by comparison the fittest, by exposing them all to the fiercest struggle for survival (Popper 1934, 42)."

Popper's argument can be summed up as follows. A theory, to be scientific rather than merely ideological, must clearly rule out specific possible occurrences, so that there will be no question as to whether or not it is indeed falsified if these events do, in fact, come to pass. The more a theory survives attempts to refute it, the more highly corroborated it becomes. It is thus increasingly reliable as a guide to predicting future events, and one can ever more confidently hope that, to some degree, it reflects the regularities actually out there. But there is no guarantee that it is a complete and *true* reflection.

Thomas Kuhn was a graduate student when he first encountered Popper's ideas. They inspired him to launch a study of his own, into the actual historical development of science, and the sociological processes influencing it. He decided

to go beyond Popper's separation of science from ideology and attempt to identify the process by which the one might evolve into the other; and, subsequently, to progress in the cumulative manner unique to scientific knowledge.

In his first book Kuhn traced the development of the earliest roots of science in the tradition of explaining observed phenomena and requiring that the culture's prevailing cosmology be founded on these explanations. He noted that it was the Greeks who first established this as a criterion for a psychologically satisfying world view. Primitive cultures made no such demands on their cosmologies; consequently, they had no built-in mechanisms for correction and change.

Kuhn suggested that, in modern culture, explanations are the foundation stones for imaginative conceptual schemes which define not only the pursuit of knowledge, but the very way people perceive and experience reality. He claimed that ultimately these schemes stand or fall not on logical criteria, but on the basis of (1) their adequacy as efficient devices for summarizing bits of information, (2) their ability to provide emotional satisfaction and thus to inspire commitment, and (3) their fruitfulness for generating predictions of additional observations; that is, "their effectiveness as guides to research, and as frameworks for the organization of knowledge (Kuhn 1957, 40)."

Eventually, as in the case of the changeover from Ptolemy's geocentric model of the universe to that of Copernicus, the hypotheses generated by the old theory turn up evidence that undermines it. They reveal so many anomalies that the time becomes ripe for a contending model with the power to provide satisfying explanations for all available observations -- the old as well as the new. According to Kuhn, science advances because "each new conceptual scheme embraces the phenomena explained by its predecessors and adds to them. But though the achievements of Copernicus and Newton are permanent, the concepts that made those achievements possible are not. It is only the list of explicable phenomena that grows; there is no similar cumulative process for the explanations themselves. As science progresses, its concepts are repeatedly destroyed and replaced (Kuhn 1957, 264-5)."

Kuhn implied that, in relatively minute and self-contained areas of study where the conceptual framework is not intimately connected to the culture's ultimate premises, a single incompatible observation by a scientist might be sufficient to raise grave doubts about a theory, and its "unfitness" would soon be demonstrated. This is entirely consistent with Popper's recognition of falsification as the equivalent -- for contending hypotheses in scientific development -- of environmental feedback in the process of natural selection.

Kuhn concluded, however, that different rules apply in the case of really significant scientific revolutions like those of Copernicus and Newton, where a major aspect of reality is at stake. These inevitably necessitate a revolution in philosophy and morality as well. Here, he said, the history of science leads us to expect a lengthy period of conflict and instability within the profession following the original scientific breakthrough, and perhaps centuries before the new world-view begins to dominate within the culture at large. Whenever these comprehensive defining frameworks are involved, the evolution of science and that of the cosmology following in its wake is no more likely to be tidy and painless than is the case for any other type of revolutionary change. Although Kuhn did not discuss biology in this context, his ideas shed considerable light on the continuing public reluctance to accept Darwinism -- and on the stubborn resistance, even within the social sciences, to its implications for human behavior and cultural evolution.

In his second book, Kuhn introduced the idea that it is the attainment of a common "paradigm", by the community of scientists working in a given problem area, that distinguishes science from other forms of study. He used the term in a very specific way to denote four defining characteristics of a uniquely *scientific* conceptual framework. These are: (1) a set of shared symbolic generalizations; (2) a common model of reality; (3) shared values as to standards and legitimate procedures; and (4) shared exemplars in the form of concrete problem solutions typical of the approach of the relevant scientific community. He explained that it is the assimilation of these standards and exemplars, rather than the conscious acquisition of formal rules, which is the end result of the socialization process by which a student becomes a member of the community of professional scientists.

Kuhn suggested that, after a particular theory has won out over all contenders to achieve paradigm status within the problem area concerned, the practitioners of what is now a mature science behave, for all practical purposes, as if their disciplinary matrix were indeed coincident with reality. And the paradigm is initially assimilated by candidates to the field as if it *were* reality. In fact, science is often taught as dogma, in courses quite devoid of epistemological sophistication. Kuhn called this "normal science", because he had concluded that it is indeed the norm for most scientists most of the time.

Karl Popper disagreed with this. In his estimation, Kuhn's wholesale acceptance of what he called "the myth of the framework" was both mistaken and dangerous. However, Popper admitted that the concept of "normal science" is valuable as a warning because it is unfortunately true of some occurrences in modern laboratories and university departments. But, he maintained, it is neither the norm nor is it science. He pointed out that, on the contrary, the best scientists (such as

Bohr in 1913 and Einstein in 1916) realize the tentative nature of their conjectures and expect that they will be superseded in time. He observed that Kuhn's version of "normal science" has only recently become a significant aspect of the behavior of those who work in the field and if, indeed, it should ever represent routine practice, it would signal the *end* of science!

Kuhn argued that, for the individuals concerned, the pursuit of "normal science" may well inhibit the doubt and originality that would seem to be a prerequisite for scientific progress during revolutionary periods. But, he added, it may also be the source of the strength of science. For it means that working scientists can dispense with questions of definition, philosophical assumptions and non-replicability, and concentrate directly on puzzle solving. They are superbly prepared to push the theory as far as it will go and to recognize anomalies in the results. In this process of refining and elaborating the theory, they function as "worker bees": the indispensable knowledge builders in the cumulative scientific enterprise. They are also armed with considerable resistance to radically different perspectives on the problem, according to Kuhn, and this means that propositions challenging the established paradigm will not be readily accepted. This is what protects the enterprise from being blown off course by current cultural fads. Only after expectations are consistently violated by the research results will the established paradigm be questioned, and only when a more adequate contender is available will the paradigm be given up.

"By focusing attention upon a small range of relatively esoteric problems, the paradigm forces scientists to investigate some part of nature in a detail and depth that would otherwise be unimaginable. And normal science possesses a built-in mechanism that ensures the relaxation of the restrictions that bind research whenever the paradigm from which they derive ceases to function effectively (Kuhn 1970, 24)." Kuhn explained that, as long as the conceptual tools provided by the paradigm continue to solve the problems it defines, there is rapid progress. Inevitably, however, there comes a time when anomalies begin to appear at the leading edge of research. These result in a series of extraordinary investigations that reveal inconsistencies demanding a different explanation. The evolution of knowledge in the area concerned seems to have hit a plateau, while participating scientists scramble to explain their findings. This pre-revolutionary crisis phase is one of treading water. Its resolution -- allowing the accumulation of reliable knowledge to begin again -- only happens with the appearance of a thoroughly tested new theory that incorporates all the previously observed regularities and accounts for the currently puzzling ones as well.

Popper's explanation of the evolution of science is really not too different, although his emphasis is on the continuity of all knowledge-building endeavors,

rather than on periodic breakthroughs. According to him, the growth of human knowledge closely resembles the process of natural selection. It is *the natural selection of hypotheses* -- whether these are the formally expressed conjectures of science or the largely unexpressed guesses of the child. "Our knowledge consists, at every moment, of those hypotheses which have shown their (comparative) fitness by surviving so far in the struggle for existence; ... [the idea of] a competitive struggle which eliminates those hypotheses which are unfit ... can be applied to animal knowledge, pre-scientific knowledge, and to scientific knowledge (Popper 1979, 261)."

He agreed with Kuhn that a single falsification discrediting an entire scientific theory is not normally accepted at once. "It is usually not accepted until the falsified theory is replaced by a proposal for a new and better theory. As Max Planck remarked, one must often wait until a new generation of scientists has grown up (Popper 1983, xxv)." Elsewhere Popper described (in terms not too different from Kuhn's what is demanded of a new theory before its acceptance as a replacement for an earlier one. He wrote that, "(1) It must solve the problems which its predecessor solved at least as well as did its predecessor; [and] (2) it should allow the deduction of predictions which do not follow from the older theory; that is to say, crucial experiments ... The two demands together ensure the rationality of scientific progress, that is, an increase in verisimilitude (Popper 1977, 149-9)."

According to him, it is these demands that allow us to distinguish science from ideology; that rescue us from relativism and radical skepticism; and that allow us to assume the general reliability of science without dogmatizing specific results.

Popper and Kuhn also seem to agree on the significance, for scientific advance within any particular problem area, of a close-knit verbal community with precise rules of communication. As Popper explained it, "In order to avoid speaking at cross-purposes, scientists try to express their theories in such a form that they can be tested, i.e. refuted (or else corroborated) by experience (Popper 1962, 218)." Other types of assertions are unacceptable to scientists, because they do not lend themselves to a replication of the process by which claimants have arrived at their findings, and thereby to test and possible refutation. It is this friendly-hostile cooperation of the community of scientists that makes possible the orderly process of hypothesis testing and theory building, and the accumulation of global knowledge that results. Popper pointed out, however, that under these conditions the theory that eventually prevails does so *not* because of the persuasive or political power of a group of proponents, as happens in the case of an ideology. It has won out because of its power to explain and predict observable regularities while withstanding worldwide attempts to refute it.

Popper was more knowledgeable than Kuhn about the nature, prevalence, and power of ideology. Kuhn had underestimated the desire among social scientists to define their disciplines as mature sciences in a revolutionary crisis phase rather than as the pre-scientific ideologies which both he and Popper recognized them as being. And Kuhn had underestimated, as Popper had not, the danger involved in ideology posing as science -- particularly for the future possibility of any legitimate *social* science. Popper was aware that, while scientific knowledge, through the power it gives us, can and does allow humanity to change the world, ideological beliefs have consequences too. By rendering their proponents politically powerful but rationally and *instrumentally* impotent, they can throw up insurmountable barriers to reasoned and value-guided social change.

Consequently, Popper drew a clear boundary between the ideological and scientific approaches to reality. Whereas Kuhn is satisfied with an observable *cultural* distinction, Popper was committed to identifying the underlying *logical* one. He insisted that it would be a major disaster for humanity if we were to accept " the replacement of a rational criterion of science by a sociological one (Popper 1974, 1147)." Kuhn -- more concerned with the actual historical process by which a proto-science evolves into a science -- may have inadvertently blurred the crucial logical distinction between the two ways of knowing.

In the end, Kuhn's essential criterion is paradigm consensus within the relevant specialized problem area. Sounding very much like Popper, he maintained that, in the case of a science, " the resolution of revolutions is by selection within the scientific community of the fittest way to practice future science (Kuhn 1970, 170)." But clearly, the key issue is how such "fitness" is determined. Popper believed it is by means of the uniquely scientific testing process that selects out those hypotheses which fail to survive attempts to refute them. Kuhn believed that it is, instead, by established procedures of verification. "Verification," he said, "is like natural selection: it picks out the most viable among the actual alternatives in a historical situation (Kuhn 1970, 146)."

But exactly what did Kuhn mean by verification and how does he see it operating in the selection process? To verify means to confirm the truth of a proposition; that is, the accuracy with which it reflects reality. It is a concept consistent with the older Realist philosophy of science. Yet, Kuhn indicated that, "the notion of a match between the ontology of a theory and its 'natural' counterpart in nature now seems to me illusive in principle (Kuhn 1970, 206)." Like Popper and the Pragmatists before him, Kuhn appeared to view scientific knowledge as neither relative nor absolute, but rather, as the cumulative and increasingly effective product of an irreversible and non-directional evolutionary process.

Kuhn was critical of Popper for denying the existence of definitive verification procedures, but Popper had done this thoughtfully and deliberately. Unless one wishes to claim that scientific evolution is progressing toward a humanly knowable goal of correspondence with the "essential" nature of reality (and Kuhn agreed with Popper that such a premise is unwarranted) then one must give up the idea of verifiability. All that a scientist can ever know about the dependability of a hypothesis is whether it has continued to withstand the only logically possible test of scientific selection: the unequivocal test of *falsification*!

Only one ingredient was missing from Kuhn's powerful organizing principle, but it was an important one, as he himself came to recognize. A paradigm common to all the members of the relevant professional community is the *necessary* condition for science, but it is not a *sufficient* one. The most enduring ideologies (such as Marxism and Freudianism) could lay claim to something almost like a paradigm. Popper's criterion of falsifiability is the crucial link missing from Kuhn's original theory of knowledge. A scientific paradigm is distinguished from an ideological model by the fact that the hypotheses generated by it are amenable to falsification. And it is this that determines the "workability" posited by the Pragmatists. Science, unlike the ideology which sometimes seeks to emulate it, has a self-corrective mechanism at its very heart.

Kuhn wanted to distinguish between *particular* falsifying instances (which can be relatively insignificant to the pursuit of "normal science") and falsification as a logical principle or determining event resulting in the immediate rejection of an entire theory. Popper made no such distinction, for he simply identified the crucial selection mechanism underlying the evolution of science in general. He was not interested in explaining periodic revolutionary disruptions to an entire field, or in tracing the historical course of a specific occurrence. Far from contradicting Popper's theory, Kuhn contributed a major refinement to it when he introduced the idea of a steady accumulation of knowledge punctuated by plateaus -- as the guiding framework begins to show signs of inadequacy -- followed by wholesale readjustments in the paradigm, and then a great leap forward. This is analogous to the idea of "punctuated equilibrium" in biological evolution. But it no more refutes Popper's theory of the evolution of science than does the Gould and Eldridge hypothesis pose a fundamental challenge to Darwinism.

By combining the key elements of the Popper and Kuhn theories, we can get a clearer picture of the difference between ideology and science than either offers on its own. According to Popper, scientific rigor does not depend on the objectivity or critical attitude of individual scientists, but on the *method* determined by the process itself. Kuhn was saying the same thing, except that he spoke of the standards and values of science as dictated by the paradigm, and maintained by

the professional community.

Because ideologies claim to represent truth, their models and reinforcing communities focus, instead, on the transmission and interpretation of unchallengeable doctrine. They are thus incapable of generating a means by which corrections can be made as circumstances change, or as our instruments for measuring events are sharpened. Legitimate science does not aim for "true" theories purporting to reflect an ultimately accurate picture or "essence" of reality. It leaves such pretensions of infallibility to ideology. That is why Popper was right in concluding that scientific tests can never be tests of verisimilitude.

Popper, and the Pragmatists before him, have shown compellingly that the tests of science must be in terms of workability and falsifiability, and that the conjectures of science are accordingly tentative in nature. And he elaborated a theory of the natural selection of these conjectures. Kuhn has demonstrated how a successful scientific theory, while guiding the research in a specific problem area, is continuously elaborated, revised and refined, until it is superseded by the very hypothesis-generating and testing process that it had defined and sharpened. Like Marx's version of capitalism, it sows the seeds of its own destruction. Eventually the former paradigm disappears into the mists of history, but *the facts to which it led remain to accumulate and inform humanity*. They are the survivors in the only conscious process yet devised by humankind for guiding cultural evolution. Surely, if Poincare, Peirce, Dewey and Snow were alive today they would be proud to claim these two philosophers as their intellectual descendants!

REFERENCES :

Horwich, Paul. (Ed.) 1993. *World Changes: Thomas Kuhn and The Nature of Science*. Cambridge MA: MIT Press.

Hutcheon, Pat Duffy. 1996. *Leaving the Cave: Evolutionary Naturalism in Social Scientific Thought*. Waterloo, ON: Wilfrid Laurier University Press.

Kuhn, Thomas S. 1957. *The Copernican Revolution: Planetary Astronomy in the Development of Western Thought*. Cambridge, MA: President and Fellows of Harvard College.

----------. 1970. *The Structure of Scientific Revolutions*. (2nd. edition enlarged) Chicago: University of Chicago Press.

----------. 1970. "Logic of Discovery or Psychology of Research?". I. Lakatos and A. Musgrave. (Eds.) *Criticism and the Growth of Knowledge*.

London: Cambridge University Press, 1-23.

----------. 1970. "Reflections on my Critics". I. Lakatos and A. Musgrave. (Eds.) *Criticism and the Growth of Knowledge*. London: Cambridge University Press, 231- 78)

.----------. 1974. "Second Thoughts on Paradigms". Frederick Suppe. (Ed.) *The Structure of Scientific Theories*. Chicago: University of Illinois Press, 459-482 and 483-517.

----------. 1977. *The Essential Tension: Selected Studies in Scientific Tradition and Change*. Chicago, IL: University of Chicago Press.

----------. 1980. "The Halt and the Blind: Philosophy and History of Science". *British Journal for the Philosophy of Science*. 31, 181-192.

----------. 1982. "Commensurability, Comparability, Communicability". *PSA* Volume 2. East Lansing: Philosophy of Science Association, 669-688 and 712-16.

----------. 1992. *The Trouble With The Historical Philosophy of Science*. Cambridge, MA: Department of History of Science, Harvard University (occasional paper).

Popper, Karl. 1934. *The Logic of Scientific Discovery*. London: Hutchinson and Co. (1959).

----------. 1957. *The Poverty of Historicism*. New York: Harper and Row. (1964).

----------. 1943. *The Open Society and its Enemies*. Volume 1. "The Spell of Plato<". princeton, nj: princeton university press. (1962).

----------. 1943. *The Open Society and its Enemies*. Volume 2. "The High Tide of Prophecy: Hegel. Marx and the Aftermath.". Princeton, NJ: Princeton University Press. (1962).

----------. 1972. *Objective Knowledge: An Evolutionary Approach*. Oxford: Clarendon Press. (Revised 1979).

----------. 1974. "The Autobiography of Karl Popper". Paul Arthur Schlipp. (Ed.) *The Philosophy of Karl Popper*. La Salle, IL: The Library of Living Philosophers, 3- 182.

----------. 1974. "Replies to my Critics". Paul Arthur Schilpp. (Ed.) *Ibid*. 949-1180.

----------. and John Eccles. 1977. *The Self and its Brain: An Argument for Interactionism*. London: Springer International.

----------. 1983. "Realism and the Aim of Science". *Postscript to the Logic of Scientific Discovery*. Totowa, NJ: Rowman and Littlefield. i-260.

Scott, Peter. 1985. "An Occasional Revolutionary". *The Times Higher Education Supplement*. July, 11.

Snow, C.P. 1934. *The Search*. London: Macmillan and Co. (5th. edition, 1963).