PERSPECTIVES ON THE SOCIOLOGY OF SCIENCE

Karl Popper and Thomas Kuhn

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[A paper submitted to Dr. Keith Dixon
at Simon Fraser University
in the Spring of 1978[1]]

In the philosophy of science today, as in social and political philosophy,we badly need to understand better than we do the detailed processes of variation and selective perpetuation by which ideas and institutions change and displace one another.
S. Toulmin - "New Directions in the Philosophy of Science"

I see in science one of the greatest creations of the human mind. It is a step comparable to the emergence of a descriptive and argumentative language, or to the invention of writing. It is a step at which our explanatory myths become open to conscious and constant criticism and at which we are challenged to invent new myths.
K. R. Popper - "Objective Knowledge"

Without commitment to a paradigm, there could be no normal science.
T. S. Kuhn - "Structure of Scientific Revolutions"

Scientists try to eliminate their false theories, they try to let them die in their stead. The believer - whether animal or man - perishes with his false beliefs.
K. R. Popper - "Objective Knowledge"

INTRODUCTION

The pronouncements of two men, K. R. Popper and Thomas S. Kuhn, have sparked a debate in the social sciences which has continued for over two decades. Both of these men are interested in describing and explaining the processes by which "scientific" knowledge is acquired and subsequently, how it grows. Their findings obviously have serious implications for the sociology of knowledge (under which we would include the sociology of science) and it is this area which forms the basis for the present discussion. We will describe each man's

[1] Originally titled "*The Popper – Kuhn Debate: Implications for the Sociology of Knowledge,*" this paper was submitted in the spring of 1978. The text of the paper is unchanged (along with the insensitive use of gender!) and presents a glimpse into the kinds of issues at play in this field more than forty years ago.

position and survey its implications for the sociology of knowledge. It will also be useful to survey the literature for the various criticisms of both Popper and Kuhn which have emerged over the years. In so doing we will be able to more easily see the weak points involved in each perspective, keeping this in mind as we offer a personal critique of each.

Finally, we will endeavor to outline how both Popper's and Kuhn's ideas have affected the sociological study of science and offer what we see as the probable directions of future sociological investigation in this area. We have come to the conclusion that neither Popper nor Kuhn have been able to generate a theoretical stance which can adequately explain all of the aspects of scientific methodology and growth (although we would hold that there are important contributions to be gained from the judicious study of both investigations). By making use of the best and most useful ideas in each and rejecting the rest, we intend to show how the sociology of knowledge stands to gain by acquiring a proper grasp of the work of these two men.

SCIENCE ACCORDING TO KARL POPPER

Karl Popper has devoted at least 50 years of his life to the study of modern science and its philosophical basis. His interest over the years has been, *inter alia*, in discovering the criterion of demarcation between what we know as science and that which is described as non-science. He has come to the conclusion that the basic criterion of demarcation is not, as had been commonly assumed, the use of the inductive method to expose regularities and general laws in nature, but is, in fact, the falsifiability of theory. Those theories which are not falsifiable are, in Popper's opinion, unscientific - metaphysical.

Theories, then, must leave themselves open to serious testing. "Every serious test of a theory is an attempt to refute it. Testability is therefore the same as refutability, of falsifiability."[1] Provided the theory is formulated in a sufficiently precise manner, "…it should now be possible to seek out weaknesses in the theory by trying to falsify its predictions. If it passes the test, it is corroborated and can be retained for the time being."[2]

In all of this, Popper has brought to light what he feels to be the rules of the scientific game and delineated the errors which might lead to dogmatism and obscurantism.[3] Some scientists, notably J.C. Eccles, argue that were more of their colleagues to heed Popper's advice by designing experiments which encourage attempts at falsification, experimental effort would be greatly economized and the incidence of random probing into unfruitful areas be reduced.[4] Popper's main contention is that we can learn from our mistakes.[5]

We should not fear criticism, we should welcome it; for we know that theories must die sooner or later and be replaced by newer attempts at explanation, which take into account lessons learned in the past.

It is argued that the critical attitude, which Popper feels should be pervasive in scientific endeavor, is tied in with the rationality of science:

> … it is essentially (theories') critical and progressive character - the fact that we can argue about their claim to solve our problems better than their competitors - which constitutes the rationality of science.[6]

Instead of explaining the growth of science by the accumulation of knowledge, Popper holds that (science) "…grows by a method more revolutionary than accumulation – by a method which destroys, changes, and alters …"[7] that is, through uninhibited criticism. As we noted briefly already, Popper labels metaphysics "non-scientific" by virtue of its irrefutability. He does not feel, however, that this is sufficient to render metaphysics valueless. As noted by Bryan Magee, "not only can a metaphysical theory be meaningful, it may actually be true; but if we have no way of testing it, there can be no empirical evidence for it, and therefore it cannot be held to be scientific."[8] Popper includes theories such as Marxism and psychoanalysis among metaphysical theories because of the fact that they have been rendered irrefutable because of along succession of ad hoc adjustments made as a result of situations which could have falsified the theories.[9] These ad hoc adjustments, which Popper labels auxiliary assumptions,[10] supplementary hypotheses,[11] or conventionalist strategems,[12], may rescue the theory from refutation, "but it rescues (them) only at the price of destroying or at least lowering (their) scientific status."[13]

This, as we shall see, directly confronts Thomas Kuhn's description of what he calls 'normal science', where scientists spend the majority of their time endeavoring to substantiate the ruling 'paradigm' of the time instead of clamoring to falsify it. Scientists whose theories have been falsified, if they adhere to a Popperian *Weltanschauung*, will be able to continue their activities knowing that their work was not in vain: "When one's hypothesis has been falsified one should even rejoice, because in this denoument science has been well served."[14]

Popper is not particularly interested in the means by which one has come to formulate a particular theory. He is willing to concede that "intuition undoubtedly plays a great part in the life of a scientist, just as it does in the life of a poet. It leads him to his discoveries…" However, "science does not ask how he has got his ideas, it is only interested in arguments that can be tested by everybody."[15] This attitude, as we shall see subsequently, is not

satisfactory for the sociologist of knowledge who is interested in determining the effect of the scientists' social milieu on his work.

The best theory is, in Popper's opinion, the theory with the highest informative content instead of, as is usually argued, that with the highest probability of being true. As Popper points out, anyone can formulate hypotheses with a very high probability of turning out to be true, but useful theories must contain sufficient information to allow them to be tested. There is an inverse proportion, argues Popper, between informative content and probability; those with the highest informative content are the most likely to be proven false, and vice versa. Popper states his preference by noting "in short, we prefer an interesting, daring, and highly informative theory to a trivial one."[16] He comments, in addition:

> If high probability were an aim of science then scientists should say as little as possible, and preferably utter tautologies only. But their aim is to 'advance' science, that is, to add to its content. Yet this means lowering its probability.[17]

To Popper, knowledge begins, not with observations, but with problems which must be solved. In fact, in the words of Bryan Magee, Popperian analysis is nothing more than a logic of problem-solving.[18] Thus, "it is the problem which challenges us to learn; to advance our knowledge; to experiment, and to observe."[19]

Man is inherently a problem-solver, a seeker of regularity in the midst of chaos. (Here Popper espouses a doctrine very much the same as that of Claude Levi-Strauss as expressed in his anthropological classic, '*The Savage Mind*'.) In this, Popper's theory of knowledge is coterminous with a theory of evolution.[20] David Bloor observes:

> (In Popper's philosophy) the image of Darwinian struggle is prominent. Science is a projection of this struggle for survival, but one in which our theories die for us. To speed up the struggle for survival and the elimination of weak theories, we are enjoined to take intellectual risks… "[21]

As regards man's need for establishing regularity in his world, Popper remarks how he first noted this need among animals and children and then later also among adults. This need, he alleges, is so strong that it sometimes makes individuals "experience regularities even when there are none."[22] It also may lead individuals to cling to their expectations dogmatically for fear of losing the psychological comfort this regularity brings. All of man's observation must be carried out selectively. The reason for this is "the infinite wealth and variety of the

possible aspects of the facts of our world," which make it not only impossible to avoid a selective point of view, but "also wholly undesirable to attempt to do so."[23] Thus,

"observation needs a chosen subject, a definite task, an interest, a point of view, a problem."[24] This frame of reference is very close to Kuhn's idea of a particular scientific point of view, which he has called a paradigm. On their abhorrence of undirected, fruitless observation, Kuhn and Popper concur.

Popper spent a great deal of time pondering "Hume's problem " (concerning the paradox of induction) and came to the conclusion that induction gives no guarantee of proof because only one instance is sufficient as a counter-instance. Thus, the fact that water has boiled a dozen times successively when heated to 100°C, leading one to assume that a thirteenth instance would bring the same result, is not a fact of logic but merely of man's psychological nature.

Popper argues that induction is in reality a myth. Magee states that "although there is no way of demonstrating the validity of inductive procedures, we are so constituted psychologically that we cannot help thinking in terms of them."[25] In all of this, then, the fact "that the whole of science, of all things, should rest on foundations whose validity it is impossible to demonstrate has been found uniquely embarrassing."[26] Popper's solution to this dilemma has been to reject the importance of induction altogether in favour of his criterion of falsification as being the true demarcation between science and non-science.

One can easily see why Popper would consider Francis Bacon as an archenemy:

> Although sensitive to the history of science and fully aware of Bacon's achievement, Popper never misses the opportunity to criticize the Baconian inductive model of science. The 'dogma' of obser- vation, the 'myth of a scientific method that starts from observation and experiment' are stock phrases which Popper uses when discussing Bacon's concept of science.[27]

When comparing the merits of competing theories, Popper advises accepting that which possesses the highest informative content.[28] Those theories which stand up best to severe tests are, in Popper's words, "well-founded theories." Although there may remain anomalies with which a particular theory has trouble handling, a well-founded theory will be less troubled by anomaly. In spite of his criterion of falsifiability, however, Popper "also says we should not abandon our theories lightly, for this would be too uncritical an attitude

toward tests, and would mean that the theories were not tested as rigorously as they should be."[29]

> Those among our theories which turn out to be highly resistant to criticism, and which appear to us at a certain moment of time to be better approximations to truth than other known theories, may be described, together with the reports of their tests, as 'the science' of that time.[30]

Without the legacy of research bequeathed by tradition, Popper maintains that contemporary scientists would have to start from scratch, as it were, in their investigations: "Quantitatively and qualitatively, by far the most important source of our knowledge - apart from inborn knowledge - is tradition." Similarly "… without tradition, knowledge would be impossible."[31] Thus, an aspiring young scientist cannot start afresh if he desires to make a significant addition to scientific knowledge, but must pick up and continue a line of inquiry which others have long ago begun. In this way, one must "fall in with the tradition of science."[32] Popper, and, as we shall see later, Kuhn, both express the opinion that knowledge is not cumulative, as the majority of science textbooks would lead one to believe. Popper declares that "science does not develop by a gradual encyclopaedic accumulation of essential information... but by a much more revolutionary method; it progresses by bold ideas, by the advance of new and very strange theories... and by the overthrow of old ones."[33] Magee adds that the popular notion of the sciences as bodies of established fact is entirely mistaken: "nothing in science is permanently established, nothing unalterable, and indeed, science is quite clearly changing all the time."[34] Thus, the growth of knowledge - or the learning process - is seen not as a cumulative process but one of error elimination only.[35]

Popper eschews a dogmatic attitude toward one's favorite theory as being unscientific: "The dogmatic attitude is clearly related to the tendency to verify our laws and schemata by seeking to apply them and to confirm them, even to the point of neglecting refutations."[36] Those who make it their task to impart a definite doctrine, and to preserve it, pure and unchanged, can never call themselves scientists, in Popper's opinion.[37] The only 'dogmatism' Popper will accept is an honest defense of a theory against criticism; that is, a defense which keeps a theory from succumbing too easily, "before it had been able to make its contributions to the growth of science."[38] He continues by noting that "a good defense of a theory against criticism is a necessary part of any fruitful discussion, since only by defending it can he find out its strength and the strength of the criticism directed against it."[39] Popper is aware that a too- rigorous application of his falsification criterion would serve only to bring useful scientific endeavor to a halt.

Although scientists spend their time in the search for truth, Popper maintains that an individual can never be sure whether or not he has attained it. We can know however, that we are approaching the truth more closely, when we formulate theories which are higher in informative content and more difficult to falsify. Popper asserts that "... though we should seek for absolutely right or valid proposals, we should never persuade ourselves that we have definitely found them; for, clearly, there cannot be a criterion of absolute rightness - even less than a criterion of absolute truth."[40] (emphasis added)

We must not become disillusioned with science because of this uncertainty, however. Because of the fact that we can never actually know for sure; that we must be aware of the possibility of radical transformation of our conceptual scheme at any time, some have seen themselves helplessly adrift on a sea of relativity. Magee offers the conclusion, following Popper, that "a great deal of the disillusionment with science and reason which is so widespread in our age is based on precisely such mistaken notions of what science and reason are."[41] Those who expect truth from science, then, must be disappointed. As David Bloor so cogently puts it, "Truth is indeed the goal, but it is at an infinite distance."[42] This notion of conjectural, relative truth, although difficult to accept, is absolutely necessary if science is to maintain its growth and accomplishments. Those satisfied that they have arrived, that they now possess "the truth," most often lose their incentive to continue searching for new data.

One of Karl Popper's most interesting conceptual schemes is his idea of Worlds 1, 2, and 3; where, briefly,

- World 1 is the objective world of material things;
- World 2 is the subjective world of minds; and
- World 3 refers to a "world of objective structures which are the products, not necessarily intentional, of minds or living creatures, but which once produced, exist independently of them."[43]

It is Popper's notion of World 3, a world of objective testable knowledge independent of social context, which is of special interest to us in the present discussion. Popper himself describes World 3 as follows:

> (World 3) is largely autonomous, even though we constantly act upon it and are acted upon by it: it is autonomous in spite of the fact that it is our product and that it has a strong feedback effect upon us... It is through this interaction between ourselves and the third world that objective knowledge grows, and that there is a

> close analogy between the growth of knowledge and biological growth...[44] (emphasis added)

A study of the implications of this notion for the sociology of knowledge is thought to promise a vast treasure of future data and useful perspectives. We are sure that sociology has not heard the last of Popper's World 3.

As our knowledge grows, says Popper, so, too, our ignorance expands -- at an even greater rate. While our knowledge can be only finite, our ignorance must necessarily be infinite.[45] We must take heart in knowing that the challenge of understanding the universe around us is a perennial challenge; one which should constantly motivate scientists. As opposed to Thomas Kuhn, it is quite obvious that Karl Popper is a methodologist par excellence; a man more interested in the intellectually challenging and the abstract than in the mundane implications of a sociology of knowledge perspective such as that assumed by Kuhn.

Having made a necessarily brief overview of Popper's ideas regarding contemporary science, let us now turn to those of Thomas Kuhn.

KUHN'S ANALYSIS OF THE DEVELOPMENT OF SCIENCE

Kuhn's central concept is that of the paradigm. Defined in the preface to '*The Structure of Scientific Revolutions*' as "universally recognized scientific achievements that for a time provide model problems and solutions to a community of practitioners,"[46] scientific paradigms are seen as the means through which scientific investigation is carried out. The paradigm provides the frame of reference, the point of view, required for scientific analysis. In a postscript written seven years after original publication of "*The Structure of Scientific Revolutions*," Kuhn admits that his use of the term was, as his critics allege, ambiguous, saying:

> ... in much of the book, the term 'paradigm' is used in two different senses –
>
> - On the one hand, it stands for the entire constellation of beliefs, values, techniques, and so on shared by the members of a given community.
> - On the other, it denotes one sort of element in that constellation, the concrete puzzle-solutions which employed as models or examples, can replace explicit rules as a basis for the solution of the remaining puzzles of normal science.[47]

At the present time, Kuhn often uses the term "disciplinary matrix"[48] as a replacement for his original "paradigm." The exact meaning of the concept remains, for better or for worse, nebulous.

The paradigm is seen as the means whereby research is directed into specific areas. Its presence leads scientists into investigations of the natural world which, in its absence, would be virtually impossible. The paradigm orders and aligns previously random data and permits analysis "… in a detail and depth that would otherwise be unimaginable."[49] Kuhn, following Popper, points out that "... nature is vastly too complex to be explored even approximately at random. Something must tell the scientist where to look and what to look for."[50] That "something" is for Kuhn a "paradigm" and for Popper a "well-founded theory." Man does not seem to be constructed psychologically to be able to view the world around him piecemeal or item by item.[51] The "regularity" which Popper alluded to is here acknowledged by Kuhn as a necessary factor in the human psyche, revealed in the scientific world as a reigning paradigm.

A paradigm can be likened to a pair of spectacles through which the scientist must look in order to make sense of an otherwise chaotic world. Thus, "paradigms provide all phenomena, except anomalies, with a theory-determined place in the scientist's field of vision."[52] It becomes clear that a change in paradigm necessitated by factors which we shall be discussing shortly, involves a change in perception as well: "In learning a paradigm, the scientist acquires theory, methods, and standards together usually in an inextricable mixture. Therefore, when paradigms change, there are usually significant shifts in the criteria determining the legitimacy both of problems and of proposed solutions."[53]

Kuhn describes a scientist who must change his paradigm as being "much like the man wearing inverting lenses." Even though he may be confronting the same array of objects as previously, the very presence of the new paradigm, with its attendent change in perception, transforms those objects through and through in many of their details.[54] Assuming a scientific milieu where practitioners make use of two competing paradigms, it is argued that these scientists would be practicing their craft in two different worlds; that they would "see different things when they look from the same point in the same direction." Were they to be engaged in discussions of the relative merits of their particular point of view, Kuhn argues that most often the scientists would be talking through each other instead of making sense. Fortunately, "mature" sciences have reached the point where, generally, paradigms have been accepted by all but the most intransigent. Viewed diachronically, however, the use of commonly-accepted paradigms is a relatively new phenomenon in science: "… paradigms are a relatively late acquisition in the course of scientific development."[55] In fact, the

majority of social sciences, including sociology, are labelled "pre-paradigmatic" by Kuhn. Here, several schools of thought compete for supremacy and the discipline is unable to take a common body of belief for granted. Thus:

> The pre-paradigm period... is regularly marked by frequent and deep debates over legitimate methods, problems, and standards of solution, though these serve rather to define schools than to produce agreement.[56]

In the mature sciences, those possessing an agreed-upon paradigm (or paradigms), scientists no longer, in their major works, need attempt to "build their field anew, starting from first principles and justifying the use of each concept introduced."[57] Instead, they are free to pursue their research untrammelled by the acrimonious theoretical debate which most often serves only to sap a discipline's strength and vigor.

Scientists working within an accepted paradigm are involved in what Kuhn has described as "normal science." Here, falsification of the paradigm is not sought: "no part of the aim of normal science is to call forth new sorts of phenomena; indeed those that will not fit the box are often not seen at all. Nor do scientists normally aim to invent new theories, and they are often intolerant of those invented by others."[58] Here, Kuhn's description of science directly confronts that of Karl Popper; scientists are seen as working to strengthen the paradigm, not to falsify it. Thus, most scientists are engaged, throughout their careers, in "mopping- up" operations; endeavoring to seek out and explore the areas of investigation defined by the paradigm. In discussing this area, David Bloor observes that "Popper, for his part, does not deny the existence of 'normal science', but he does insist that it is hackwork."[59] By this, Popper implies that, although this sort of activity is necessary to the furtherance of science, real, substantive advance is possible only through bold hypothesizing and severe criticism. While Kuhn's scientist is busily at work trying to make his paradigm more precise and seeking data to confirm it, Popper's scientist would regard this as dogmatism and hence pernicious to real scientific advancement. Kuhn goes so far as to state unequivocally that "… it is only during periods of normal science that progress seems both obvious and assured."[60]

The Kuhnian scientist is concerned with puzzle-solving rather than Popper's problem-solving: "Normal science... corresponds to a state of mind which sees the furtherance of the research tradition as giving rise to puzzles rather than problems. To call something a puzzle assumes that a solution exists and... it carries the further implication that the terms of the solution will be similar to those that have already proved successful in the paradigm investigations itself."[61] These puzzles are defined by the framework provided by the

paradigm and would change if a new paradigm were to replace the old. In all of this we can see that Kuhn is at pains to demonstrate that paradigms become status quo and as such, difficult to overthrow. Although Kuhn, like Popper, observes that "by ensuring that the paradigm will not be too easily surrendered, resistance guarantees that scientists will not be lightly distracted."[62] He is willing to concede the existence of a far more obstinate clinging to the tried and true in scientific endeavor than Popper would ever be willing to admit. In Craig Shield's review of Kuhn (1962), it is stated that "too many men have a commitment, both in terms of training and reputation, to the original paradigm."[63] J. Urry concludes his summary of Kuhn's perspective by stating:

> (Kuhn) maintains that scientific research is best developed through a dogmatic reliance upon tradition; a closed rather than an open mind is the path to scientific utopia.[64]

Scientific education, with its reliance upon textbooks as the primary pedagogic tool, is seen as a major contributor to scientific dogmatism. Kuhn is convined that "scientific education inculcates what the scientific community had previously with difficulty gained - a deep commitment to a particular way of viewing the world and of practicing science in it."[65] Here, by means of studying paradigms, a student is prepared for membership in the particular scientific community with which he will later practice.[66] These students, Kuhn argues, accept theories on the authority of teacher and text, not because of evidence.[67] Throughout a budding scientist's education, right up to the very last stages, "textbooks are systematically substituted for the creative scientific literature that made them possible."[68] No wonder, then, that such an education generally produces an individual who is not able to discover a fresh approach to a particular set of problems. Kuhn is prepared to argue that "scientific education is a narrow and rigid education, probably more so than any other except, perhaps, orthodox theology."[69] (emphasis added)

Kuhn is far more interested than Popper in the scientist's social milieu, the community of practicioners and colleagues in which his work is pursued day to day. He points out that the relative isolation of the scientific community may have a significant impact when seen in a sociological perspective. Kuhn remarks that "... there are no other professional communities in which individual creative work is so exclusively addressed to, and evaluated by, other members of the profession."[70] Bloor, too, relates:

> The theme of 'community' is a pervasive one, with its overtones of social solidarity, of a settled way of life with its own style, habits, and routines... [71]

Paradigms may find their hegemony violated, or at least threatened, by the existence of too many worrisome anomalies. Similar to Popper's refutations or falsifying instances, Kuhn's anomalies tend to be played down in the practice of normal science. Kuhn explains that anomalies are usually set aside during scientific research and remain unimportant until the proper social conditions make them so.

Anomalies are violations of expectation based on the prevailing paradigm. If they can be shown to be sufficiently destructive to a particular paradigm, a new explanation will be sought. Without a paradigm, however, anomaly would be next to impossible to define because, in reality, anomaly is defined and brought into focus by the very paradigm which it threatens to destroy. Thus, according to Kuhn, "... novelty ordinarily emerges only for the man, who, knowing with precision what he should expect, is able to recognize that something has gone wrong. Anomaly appears only against the background provided by the paradigm. The more precise and far-reaching that paradigm is, the more sensitive an indicator it provides of anomaly and hence of occasion for paradigm change."[72] (emphasis added) Scientists who have begun to find a particular anomaly troubling will seek to magnify the breakdown, bringing it under the closest of scrutiny. The anomaly then becomes "a special focus of concern... the empirical aspects of the untaamed phenomenon will be examined with redoubled effort, and increasingly eccentric theorizing will be necessary in order to grasp its significance."[73] As Bloor notes, the conventional pattern of normal science becomes disrupted under such circumstances and a different atmosphere begins to prevail - an atmosphere which Kuhn calls "extraordinaryscience."[74]

Once such a crisis has begun, a proliferation of new theories usually appears, a phenomenon which seldom happens during the halcyon days of normal science. In Kuhn's words, "crisis loosesn the rules of normal puzzle-solving in ways that ultimately permit a new paradigm to emerge."[75] Once a new paradigm does emerge, however, a long period of heated debate over its virtues generally ensues; a debate in which, it is argued, the rationality of scientific decisionmaking becomes clouded by the exigencies of "political" behavior. As we have already mentioned, the fact that data and anomaly are paradigm-dependent (to a large degree) makes it difficult for adherents of competing paradigms to find common ground on which to conduct their discussions. Too often, scientists actually find themselves speaking different languages, as it were, and in Kuhn's terms, they speak "through" each other. Younger scientists especially may explore the ramifications of the new paradigm, having become impatient with the older paradigm's difficulty in dealing with anomalous instances.

It is noted that a new paradigm is often developed by those who are new to a certain scientific specialty or by those whose basic training was completed in a different area of

science.These, obviously, are individuals who have not been hardened into thinking in terms of one particular perspective. A new paradigm "must seem better than its competitors, but it need not, and in fact never does, explain all the facts with which it can be confronted."[76] The new paradigm must generally be proven to be able to handle the basic anomaly which gave it birth and to be more precise in its predictive power. Kuhn points out that it is only during periods such as these that there arises a sense of "pronounced professional insecurity"[77] invoking behavior which most associate with stereotypical scientific endeavor, hypothesis generation, searching for further anomaly and so on. The puzzle-solving mentality of normal science disappears - during periods of crises.

Once a new paradigm has been generally accepted, it begins to define new problems and areas of research, qualitatively different from those dictated by its predecessor: "when paradigms change, there are usually significant shifts in the criteria determining the legitimacy both of problems and of proposed solutions."[78] Acceptance of a new paradigm does not occur overnight, in fact, a full generation may pass before general acceptance is assured. Individual acceptance, on the other hand, is seen by Kuhn as to taking place almost instantly (and in an unstructured manner) much in the same manner as a Gestalt switch in psychology. Kuhn uses the term 'conversion' several times to describe this experience. His critics have been quick to note the subjective connotations of this word but Kuhn, nevertheless, is not prepared to substitute another. This conversion is an individual, not a group phenomenon. Kuhn describes the situation by commenting that "rather than a single group conversion, what occurs is an increasing shift in the distribution of professional allegiances."[79]

Those who do change their allegiance are declared to exhibit faith (another notoriously subjective quality) "… that the new paradigm will succeed."[80] A decision such as this, argues Kuhn, can only be made by faith. Because this changing of paradigm preference entails a choice between incommensurables, it is alleged that such a transition "cannot be made a step at a time, forced by logic and neutral experience."[81] Like a Gestalt switch, then, the change must occur all at once or not at all. David Bloor has acknowledged that it is this very concept which most separates Kuhn and Popper:

> ... Kuhn states...that what divides him from Popper is a Gestalt switch, the same facts are fitted together into a different picture.[82]

Changes in the ruling paradigm, called 'revolutions' by Kuhn, can be both large and small, although the majority of those discussed in "*The Structure of Scientific Revolutions*" are obviously large scale. Kuhn explains that some revolutions affect only the members of a

professional sub-specialty while other paradigm changes may be tremendously influential in a variety of disciplines. Some of Kuhn's critics have concluded that his concern was primarily with major revolutions, such as those associated with Copernicus, Newton, Darwin, or Einstein, and therefore did not present a balanced view of scientific activity. Kuhn's reply is found in the Postscript to the 1967 edition of "*The Structure of Scientific Revolutions*":

> A revolution is for me a special sort of change involving a certain sort of reconstruction of group commitments. But it need not be a large change, nor need it seem revolutionary to those outside a single community, consisting perhaps of fewer than 25 people.[83]

Thomas Kuhn's analysis brings into serious doubt the assumption that in science the data always influences theory formulation instead of vice versa. When he speaks of "forcing nature into conceptual boxes"[84] and "making nature fit a paradigm'[85] he is urging scientists to carefully review their actions since, if these comments are correct, many of science's most venerated methodological dicta are being horrendously violated. It seems that Kuhn's more sociological perspective has led him to describe scientific activity as it is affected by social forces on a latent level. In this, we would argue that although some of Kuhn's conclusions may embarrass contemporary scientists, he has endeavored to "unmask" their activity and in doing so has done them a service. Popper, on the other hand, seems loath to descend to such pedestrian concerns, and, as a result, presents what we feel to be an incomplete view of science.

POPPER AND KUHN COMPARED

Let us first summarize Karl Popper's findings. It is important to note that Popper argues that standards are external to a particular discipline, while Kuhn, on the other hand, expresses the opinion that they are internal and culturally relative. Popper has often been described in the literature as an intellectualist and an internalist, resulting in problems such as we have already noted. He "focuses on those aspects of science which are universal and abstract, such as its methodological canons and general intellectual values." Kuhn, alternately, "focuses on its local and concrete aspects, such as the specific pieces of work which provide exemplars for groups of practicioners."[86]

Popper calls himself a critical empiricist[87] and as such, has always been careful, as noted by Bryan Magee, "to make the distinction between the logic of scientific activities and their psychology, sociology and so forth."[88] In this, his perspective is a radical departure from Kuhn's. Popper desires to delineate that which he observes to be the 'rational unity of

mankind,' a concept implying that differences arising as a result of varying cultural backgrounds are, in fact, epiphenomenal, and that men from different areas can learn to understand each other, to overcome the potential relativism and ethnocentrism which could cause confusion.

As we have already noted, Popper is relatively unconcerned with the social origins of scientific innovation, arguing that such origins "(have) little to do with (their) scientific status or character."[89] Popper maintains that scientists are either verificationists or falsificationists, and places himself in the latter category. (Kuhn, obviously would be called a verificationist). David Bloor has developed another useful dichotomy in which to describe Kuhn and Popper. He describes them as being proponents of either Enlightenment or Romantic ideologies.[90] He continues:

> It is easy to demonstrate that Popper must be classed as an Enlightenment thinker and Kuhn as a Romantic. Popper is individualistic and atomistic, in that he treats science as a collection of isolated theories. Little attention is paid to traditions of theory construction, to continuities within traditions or to discontinuities between different epochs in science. His unit of analysis is the individual theoretical conjecture. The logical and methodological characteristics of these units appear to be the same in all cases… He is concerned with the timeless and universal attributes of good scientific thinking.[91] (emphasis added)

Kuhn, on the other hand, displays attitudes attributable to his status as a "Romantic." Here "individual scientific ideas are always part of the embracing 'whole' of the research tradition."[92] The community aspects of science are seen as important to an understanding of contemporary science as well as the authoritarian character of its educational process: "…in this account, there are no clearcut logical *cum* methodological processes of falsification."[93] Kuhn's description is basically a piece of sociological history,[94] "…an essay in the sociology of scientific discovery."[95] His long digression on a psychological experiment (studying the perception of anomaly in the viewing of certain playing cards) reveals a deep interest in the psychology of perception:

> In science, as in the playing card experiment, novelty emerges only with difficulty, manifested by resistance, against a background provided by expectation. Initially, only the anticipated and usual are experienced even under circumstances where anomaly is later to be observed.[96]

In Kuhn's analysis, the 'open-mindedness' thought characteristic of all scientists is played down through arguing that important 'political' considerations are often at work. In fact, a parallel is drawn between scientific and political revolutions when it is stated that "… as in political revolutions, so in paradigm choice there is no standard higher than the assent of the relevant community."[97] In addition, it is maintained that prerequisite to both political and scientific upheavel is a pervasive sense of malfunction.

Kuhn takes a close look at scientific *praxis*, discerning an inevitable feedback effect. His diachronic perspective brings the supposed cyclical nature of science into clearer focus. Progress is seen to occur in jumps, as it were. In contrast to Popper's version of science as a linear, homogeneous process (in which the same methods apply to all stages) Kuhnian analysis views science as "a cycle of qualitatively different procedures."[98] Popper and Kuhn, however, do agree on the relative nature of scientific truth (and facts) and concur that science, like evolution, has no inherent goals of its own other than those which men naively impute to it. Thus, in the words of David Bloor, "… there need be no such thing as Truth, other than conjectural, relative truth any more than there need be absolute moral standards rather than locally accepted ones."[99]

POPPER AND KUHN CRITICIZED

Popper's criterion of falsification has been criticized by Hilary Putnam who argues that certain theories, (such as universal gravitation), are "not strongly falsifiable at all; yet it is surely a paradigm of a scientific theory."[100] To this, states Putnam, Popper might reply that he is not describing what scientists do but what they should do. Nevertheless, Putnam is prepared to argue that the existence of unfalsifiable yet universally accepted theories "refutes Popper's view that what the scientist does is to put forward 'highly falsifiable' theories, derive predictions from them, and then attempt to falsify the predictions."[101] Therefore Putnam holds that Popper is not able to disprove "the standard 'inductivist' view that scientists try to confirm theories... by deriving predictions from them and verifying the predictions."[102]

In addition, Putnam scolds Popper for consistently failing to see that practice in science is primary; that ideas are not just an end in themselves. Instead, Putnam declares that "the primary importance of ideas is that they guide practice, that they structure whole forms of life." In a similar vein, Nicholas Maxwell observes that Popper's critics have sometimes argued, in effect, that since Popperian methodological prescriptions are not, or have not been, followed in actual scientific practice, "Popper's rules thus stand refuted by empirical evidence, and should be rejected."[103] He does allow, though, that it is possible, "… despite

the apparent enormous success of the empirical sciences, that most scientists have most of the time followed not the best of methodological policies."[104] In other words, if these same scientists had followed a Popperian methodological perspective, their successes could have been much more impressive.

Imre Lakatos notes how, in his opinion, Popper stubbornly overestimates the "…immediate striking force of purely negative criticism."[105] It is felt that Popper's assertion,"…once a mistake, or a contradiction, is pinpointed, there can be no verbal evasion"[106] is much too harsh. Maxwell, too, has noticed this in Popper's work and has come to the following conclusion:

> This assumes that in testing a theory our invariable concern is to falsify it. But this assumption is false. In testing a new theory in particular, our concern may be to develop the theory, extend the range of its successful applications, build up auxiliary hypotheses … We may be justified in actually ignoring, for a time, refuting instances of a theory. For even if we have good grounds for suspecting a theory to be false, it may be in our interests to develop the theory further, as this may indicate more clearly what a new theory must ultimately explain.[107]

Maxwell continues his critique of Popper's views by mentioning Popper's command that "an experimentally refuted theory must be rejected" is too drastic: "…in general it will not be in our interests to reject a theory that, in the past, has had considerable empirical success until there is an alternative more promising theory on the horizon."[108] Maxwell's main thesis is that if we wish to follow Popper's rules in the long run, then, in certain circumstances, we will be well advised to break these rules on a short term basis. Each of Maxwell's criticisms argues, in effect, that it is against our interests to enforce too rigidly Popper's essentially long-term strategic rules on the short-term, tactical level.[109]

Thomas Kuhn has been criticized, *inter alia*, for his contention that "…the acceptance and thus the validity of scientific theories is a matter of the consensus in a given epoch" because of the fact that from this, "… it follows that there are no universal intersubjective criteria for scientific knowledge, but only criteria which are determined by a social group." Skolimowsky declares this type of thinking to be sociolgism."[110]

A book reviewer for "Science" Journal warns that in Kuhn's "*The Structure of Scientific Revolutions*" "…objectivity and progress, the pride of traditional interpretations of science, have both been abandoned."[111]

> Indeed, Kuhn's relativism did not stop here; for not only is there no means of rationally assessing two competing paradigms, there is no way of comparing them at all, so different is the world seen through them.[112]

In Kuhn's later works, however, a clear withdrawal from his earlier relativism is in evidence. For instance, Kuhn endeavors to clarify his amorphous paradigm by defining it more specifically. In addition, he comes to the conclusion that perhaps he was mistaken in arguing that what counts as a scientific problem is not completely determined by the paradigm. Also, he begins to assert that there exists "a paradigm-independent objective world (nature) which presents problems that a paradigm must solve."[113] Toulmin maintains that Kuhn retreats farther than he need have done from his original position.[114] The crux of the problem encountered by Kuhn seems to be the inherent tendency to relativism which emerges whenever one adopts a sociology of knowledge perspective. This problem still has not been adequately dealt with and takes much of the time and effort of those who desire to make use of this potentially valuable sociological tool.

IMPLICATIONS OF THE DEBATE FOR THE SOCIOLOGY OF KNOWLEDGE

Sociologists have spent a great deal of time pondering whether science is a special case for sociological analysis. David Bloor contends that "... sociologists express their conviction that science is a special case, and that contradictions and absurdities would befall them if they ignored this fact."[115] King, too, is convinced that science provides a peculiar challenge for the sociologist:

> ... scientific thought, most sociologists concede, is distinguished from other modes of thought precisely by virtue of its immunity from social determination. Insofar as thought is scientific it is governed by reason rather than tradition, and insofar as it is rational it escapes determination by 'non- logical' social forces.[116] (emphasis added)

Other sociologists, (in cluding Kuhn), however, assert that "science is a social institution, with definite traditions and codes of practice to which its members are socially induced to conform." Therefore, "the question of whether a set of methodological rules is actually followed is as much a sociological question as a psychological one."[117] Popper declares that the objectivity of science is not assured by its practicioners attitudes but by the methods they employ. Here, the relativity trap can be neatly sidestepped.

Karl Popper represents a school of thought which holds the sociology of knowledge perspective to be of very little use. As noted previously, for instance, Popper argues that social surroundings and psychology have little to do with the discovery of new conceptual alignments. He declares unequivocally that, in his opinion, "… sociological relativism, which teaches that there are truths or sciences for this or that class or group or profession, such as proletarian science and bourgeois science… is absurd."[118] It is also claimed that "… according to the sociology of knowledge no intellectual bridge or compromise between different total ideologies is possible."[119] His reasoning is most clearly delineated in his "*Open Society and Its Enemies*" (Vol. II):

> …(the sociology of knowledge and psychoanalysis) are easy to handle and good fun for those who handle them. But they clearly destroy the basis of rational discussion and they must lead, ultimately, to anti-rationalism and mysticism… sociologists of knowledge have no idea that they are just repeating Hegel … If scientific objectivity were founded, as the sociologistic theory of knowledge naively assumes, upon the individual scientists' impartiality or objectivity, then we would have to say good-bye to it.[120] (own emphasis)

This objectivity, therefore, owes its existence, however tenuous, to a scientific methodology and not to the individual scientist's virtuous behavior. Thus, "the sceptical attack upon science launched by the sociology of knowledge breaks down in the light of scientific method. The empirical method has proved quite capable of taking care of itself."[121] To Popper, then, knowledge is based upon justified (not accepted) belief.

CONCLUDING COMMENTS

As we have seen, both Kuhn and Popper have provided us with many thought-provoking insights into the emergence and development of scientific knowledge. It seems possible that both Kuhn and Popper are viewing the same object from two different perspectives and that it is this, more than any fundamental theoretical conflict, which makes their work seem to conflict more than it perhaps really does. There is still need for a great deal of follow-up work to help clarify some of the issues which have arisen.

There seems to be room for a great deal of improvement in scientific historiography, especially if Kuhn's arguments as to its nature are correct. Scientific education, too, could gain from a greater diachronic emphasis than its textbooks now provide. Science has made great strides in the past and there is no reason why, given the adoption of the best of both

Kuhn's and Popper's analyses, its growth should not be even more impressive in days to come.

NOTES

1. Popper (1963), "Conjectures," p. 197.
2. Bloor (1976), "Knowledge," p. 49.
3. Bloor (1976), "Knowledge," p. 49.
4. Eccles (1964), "Neurophysiologial Approach," in Bunge (1964), p. 275.
5. Popper (1966), "Open Society, Vol II, p.376.
6. Popper (1963), "Conjectures," p. vii.
7. Popper (1963), "Conjectures," p. 129.
8. Magee (1973), "Popper," p.48.
9. Magee (1973), "Popper," p. 43.
10. Popper (1963), "Conjectures," p. 37.
11. Popper (1972), "Objective Knowledge," p. 360.
12. Putnam (1974), "Corroboration of Theories," in Schlipp (1974), p. 235.
13. Popper (1963), "Conjectures," p. 37.
14. Eccles (1964), "Neurophysiological," in Bunge, (1964), p. 275
15. Popper (1966), "Open Society, Vol II," p.16
16. Popper (1963), "Conjectures," p. 217.
17. Popper (1963), "Conjectures," p. 286.
18. Magee (1973), "Popper," p. 41.
19. Popper (1963), "Conjectures," p. 222.
20. Magee (1973), "Popper," p. 57.
21. Bloor (1976), "Knowledge," p. 49.
22. Popper (1972), "Objective Knowledge," p. 23.
23. Popper (1966), "Open Society, Vol. II," p. 261.
24. Popper (1963), "Conjectures," p. 46.
25. Magee (1973), "Popper," p. 20.

26. Magee (1973), “Popper,” p. 21.
27. Freeman and Skolimowski (1974), "Pierce and Popper", in P.A. Schlipp, *The Philosophy of Karl Popper*, p. 511.
28. Magee (1973), “Popper,” p. 36.
29. Magee (1973), “Popper,” p. 23.
30. Popper (1963), “Conjectures,” p. vii.
31. Popper (1963), “Conjectures,” p. 27.
32. Popper (1963), “Conjectures,” p.129.
33. Popper (1966), “Open Society, Vol. II,” p. 12.
34. Magee (1973), “Popper,” p. 27.
35. Popper (1972), “Objective Knowledge,” p.144.
36. Popper (1963), “Conjectures,” p.50.
37. Popper (1963), “Conjectures,” p. 149.
38. Popper (1972), “Objective Knowledge,” p. 30.
39. Popper (1972), “Objective Knowledge,” p. 266.
40. Popper (1966), “Open Society, Vol. II,” p. 386.
41. Magee (1973), “Popper,” p. 68.
42 . Bloor (1976), "Knowledge," p. 49.
43. Magee (1973), “Popper,” p. 60.
44. Popper (1972), “Objective Knowledge,”, p. 112.
45. Popper (1963), “Conjectures,” p. 28.
46. Kuhn (1962), “Structure,” p. viii.
47. Kuhn (1962), “Structure,” p.175.
48. Kuhn (1962), “Structure,” p.182.
49. Kuhn (1962), “Structure,” p. 24.
50. Class handout; not able to determine the source, p. 96.
51. Kuhn (1962), “Structure,” p. 128.
52. Kuhn (1962), “Structure,” p. 97.
53. Kuhn (1962), “Structure,” p. 109.
54. Kuhn (1962), “Structure,” p. 122.

55. Handout, previously cited, p. 87.
56. Kuhn (1962), "Structure," p. 47.
57. Kuhn (1962), "Structure," p. 19.
58. Kuhn (1962), "Structure," p. 24.
59. Bloor (1976), "Knowledge," p. 52.
60. Kuhn (1962), "Structure," p. 163.
61. Bloor (1976), "Knowledge," p. 50.
62. Kuhn (1962), "Structure," p. 65.
63. Shields (1972) Book Review: T.S. Kuhn, "Structure," pp. 827-829.
64. Urry (1973), "Thomas S. Kuhn," pp. 463.
65. Class handout, p. 82.
66. Kuhn (1962), "Structure," p. 10.
67 Kuhn (1962), "Structure," p.80.
68. Kuhn (1962), "Structure," p. 165.
69. Kuhn (1962), "Structure," p. 166.
70. Kuhn (1962), "Structure," p. 164.
71. Bloor (1976), "Knowledge," p. 51.
72. Kuhn (1962), "Structure," p. 65.
73. Bloor (1976), "Knowledge," p. 51.
74. Bloor (1976), "Knowledge," p. 51.
75. Kuhn (1962), "Structure," p. 80.
76. Kuhn (1962), "Structure," p. 17.
77. Kuhn (1962), "Structure," p. 67.
78. Kuhn (1962), "Structure," p. 109.
79. Kuhn (1962), "Structure," p. 158.
80. Kuhn (1962), "Structure," p. 158.
81. Kuhn (1962), "Structure," p. 150.
82. Bloor (1976), "Knowledge," p. 52.
83. Kuhn (1962), "Structure," p. 180.
84. Kuhn (1962), "Structure," p. 5.

85. Kuhn (1962), "Structure," p. 135.
86. Bloor (1976), "Knowledge," p. 54.
87. Popper (1963), "Conjectures," p. 154.
88. Magee (1973), "Popper," p. 41.
89. Popper (1963), "Conjectures," p. 140.
90. Bloor (1976), "Knowledge," p. 54.
91. Bloor (1976), "Knowledge," p. 56.
92. Bloor (1976), "Knowledge," p. 57.
93. Bloor (1976), "Knowledge," p. 57
94. Bloor (1976), "Knowledge," p. 67.
95. Barber (1963), Book Review. " T. S. Kuhn, The Structure" p. 298.
96. Kuhn (1962), "Structure," p. 64.
97. Kuhn (1962), "Structure," p. 94.
98. Bloor (1976), "Knowledge," p. 54.
99. Bloor (1976), "Knowledge," p. 143.
100. Putnam (1974), in Schlipp, "The Philosophy of Karl Popper," p. 222.
101. Putnam (1974), in Schlipp, "The Philosophy of Karl Popper," p. 227.
102. Putnam (1974), in Schlipp, "The Philosophy of Karl Popper," p. 227.
103. Maxwell (1972), "A Critique," p. 132.
104. Maxwell (1972), "A Critique," p. 133.
105. Lakatos (1974), "Popper on Demarcation" in Schlipp, "The Philosophy of Karl Popper," p. 241.
106. Lakatos (1974), "Popper on Demarcation" in Schlipp, "The Philosophy of Karl Popper," p. 241.
107. Maxwell (1972), "A Critique," p. 135
108. Maxwell (1972), "A Critique," p. 135.
109. Maxwell (1972), "A Critique," p. 136.
110. Skolimowsky (1974), "Karl Popper and the Objectivity of Scientific Knowledge" in Schlipp, p. 491.
111. Anonymous (1971), "The Paradigm Concept," p. 707.
112. Anonymous (1971), "The Paradigm Concept," p. 707.

113. Anonymous (1971), "The Paradigm Concept," p. 708.

114. Toulmin (1971), "Rediscovering History," p. 60.

115. Bloor (1976), "Knowledge," p. 2.

116. King (1971), "Reason, Tradition and the Progressiveness of Science," p. 3.

117. Alan C. Musgrave, 'Objectivism of Popper's Epistemology' in Schlipp, P.A., (ed.), *The Philosophy of Karl Popper*, The Library of Living Philosophers, Vol. XIV, Book I, La Salle Illinois, Open Court Publishing. p. 581.

118. Classroom Handout, op. cit., p. 95.

119. Popper (1966), "Open Society, Vol. II," P. 214

120. Popper (1966), "Open Society, Vol. II," p. 217

121. Popper (1966), "Open Society, Vol. II," p. 220.

BIBLIOGRAPHY

Anonymous, The Paradigm Concept. Book Review of T.S. Kuhn, The Structure of Scientific Revolutions and I. Lakatos, Criticism and the Growth of Knowledge, *Science*, Vol. 172, May 14, 1971, pp. 706-709.

Ayer, A.J., Professor Popper's Work in Progress, *New Statesman*, Vol. 65, February 1, 1963, p. 155.

Barber, B., Book Review T. S. Kuhn, The Structure of Scientific Revolutions. *American Sociological Review*, Vol. 28, April 1963, p. 298.

Bloor, D., *Knowledge and Social Imagery*, London: Routledge and Kegan Paul, 1976.

Bunge, M. (ed.), *The Critical Approach to Science and Philosophy*. London: Collier Macmillan. 1964.

DeGre, G., *Science as a Social Institution: An Introduction to the Sociology of Science*, New York: Random House. 1955.

Gluck, S.E., Book Review: Conjectures and Rufutations. K. R. Popper, *American Academy of Politics and Social Sciences*, Vol. 351, January 1964, p. 218.

Hall, M. B., Book Review – T.S. Kuhn, The Structure of Scientific Revolutions. *American Historical Review*, Vol. 68, April 1963, pp. 700-701.

King, M.D., Reason, Tradition and the Progressiveness of Science, *History and Theory*, Vol. 10, No. 1, 1971, pp. 3-32.

Kuhn, T.S., The Essential Tension:Tradition and Innovation in Scientific Research. In C. W. Taylor (ed.), *The Third (1959) University of Utah Conference on the Identification of Creative Scientific Talen*t, Salt Lake City. 1959.

Kuhn, T.S., *The Structure of Scientific Revolutions*. Chicago: University of Chicago Press. 1962.

Kuhn, T.S., 1962(a) Historical Structure of Scientific Discovery, *Science*, Vol. 136, June 1, 1962, pp. 760-764.

Kuhn, T. S. The Function of Dogma in Scientific Research' in A. C. Crombie (ed.), *Scientific Change*, London: Heinemann, pp. 347-369. 1963.

Kuhn, T.S., Comment, Comparative Studies. *Society and History*, Vol. 11, October 1969, pp. 403-412.

Lakatos, I. and Musgrave, A. (eds.), *Criticism and the Growth of Knowledge*. Cambridge: Cambridge University Press. 1970.

Magee, B., *Popper*. Glasgow: Fontana-Collins. 1973.

Maxwell, N., A Critique of Popper's Views on Scientific Method. *Philosophy of Science*, Vol. 39, June 1972, pp. 131-152

Michalos, A.C., *The Popper - Carnap Controversy*. The Hague: Martinus Nijhoff. 1971.

Petras, J., Popperism: The Scarcity of Reason, *Science and Society*. Vol. 30, Winter, 1966, pp. 1-10

Polanyi, M., Genius in Science, *Encounter*, Vol. 38, pp. 43-50, January 1972.

Popper, K. R., *The Logic of Scientific Discovery*, Hutchinson: London, 1959. (first published 1934).

Popper, K. R., *The Poverty of Historicism*, London: Routledge and Kegan Paul. 1960

Popper, K. R., *Conjectures and Refutations*, London: Routledge and Kegan Paul. 1963.

Popper, K. R., *The Open Society and Its Enemies*, Vol. II, London:Routledge and Kegan Paul. 1966.

Popper, K. R., *Objective Knowledge*, Oxford: Clarendon Press. 1972.

Purtill, R. L., Kuhn on Scientific Revolutions, *Philosophy of Science*, Vol. 34, March 1967, pp. 53-58

Schlipp, P.A., (ed.), *The Philosophy of Karl Popper*, The Library of Living Philosophers, Vol. XIV, Book I, La Salle Illinois, Open Court Publishing.

Shields, C. V., Book Review: T.S. Kuhn, The Structure of Scientific Revolutions, *American Anthropologist*, Vol. 74, August 1972, pp. 827-829.

Toulmin, S., Rediscovering History: New Directions in the Philosophy of Science, *Encounter*, Vol. 36, January 1971, pp. 53-68.

Urry, J., Thomas S. Kuhn as Sociologist of Knowledge, *British Journal of Sociology*, Vol. 24, December 1973, pp. 462-473.