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Introduction

Thomas S. Kuhn's *The Structure of Scientific Revolutions*, along with his essays collected in *The Essential Tension: Selected Studies in Scientific Tradition and Change*, and Ludwik Fleck's *Genesis and Development of a Scientific Fact* raise and attempt to solve at least three problems which are of relevance for the philosophy of science. Since Kuhn's and Fleck's concerns overlap, these three problems may be identified as follows:

(1) The *historical* problem involves how one may account for changes in the conceptual frameworks of various scientific disciplines. Kuhn's *The Structure of Scientific Revolutions* contains the most systematic treatment of this question. As is evident from the title, Kuhn is primarily concerned with mapping out the essential "structure" of conceptual changes in the sciences with the hope that the specific changes which occur in the sciences can thereby be accounted for.

(2) The *sociological* problem involves the disagreements which arise among scientists concerning which conceptual frameworks should be accepted and which should be refused or abandoned. It asks to what extent these disagreements can be explained sociologically, taking the scientific community as the sociological group under investigation. Conversely, it inquires into the extent to which a consensus, i.e., an agreement among the members of a scientific community regarding a conceptual framework, can also be explained sociologically. These issues point to the more general problem of how agreements and disagreements among scientists may be accounted for and, additionally, challenges the assumption that scientists investigating the same phenomenon should, in principle, necessarily arrive at the same conceptual results.

(3) The *epistemological* problem is concerned with how any disagreements which arise in the scientific community may be resolved and with how a consensus regarding valid knowledge can be obtained. However, the issues raised by the historical and sociological questions above respectively pose two further problems for epistemology. First, once it is observed that the conceptual frameworks of scientific disciplines do in fact change historically, what then is the epistemological status of those conceptual frameworks which have been abandoned? And, perhaps more importantly, it if can be predicted on the basis of past observations that the new conceptual frameworks which replace the abandoned ones may at some future time be replaced by newer, more adequate conceptual frameworks, what then is the epistemological status of those conceptual frameworks which are presently adhered to? Second, are the epistemological directives of science in themselves capable of providing the foundation for a consensus among the members of a scientific community? Or, even when all the methodological directives have been followed precisely, can it be shown that all possi-

ble sources of disagreement may not yet have been accounted for, and that legitimate divergences in perspective can coexist within the same scientific community?

This paper will show how Kuhn and Fleck addressed themselves to these three problems and will attempt to draw some conclusions from their work about how each of these problems might be resolved.

I. The Historical Problem

Kuhn's *The Structure of Scientific Revolutions* is specifically concerned with the problem of conceptual changes in the history of science. Kuhn uses the phrase "normal science" to describe the activities which are engaged in by sufficiently developed and advanced scientific communities, and defines it as

> ... research firmly based upon one or more past scientific achievements, achievements that some particular scientific community acknowledges for a time as supplying the foundation for its further practice.[1]

When the achievements which Kuhn is here speaking of possess two characteristics, he refers to them as "paradigms." To possess these two characteristics, the achievements must (1) be relatively original, and have the capacity to draw the attention of a group of scientists away from other types of scientific activity and towards the new achievement, and (2) be, in Kuhn's words, "... sufficiently open-ended to leave all sorts of problems for the redefined group of practitioners to resolve."[2]

Kuhn's use of the term "paradigm," as Kuhn himself later acknowledged, is problematic because of its ambiguity. Having defined it in the above manner, he is not entirely consistent in using it in this way throughout *The Structure of Scientific Revolutions*. Subsequent reflections on the problem in Kuhn's later writings shed some light on what he meant by the term. Kuhn writes in his essay, "Second Thoughts on Paradigms," that although one critic had delineated twenty-two different ways in which he had used the term "paradigm" in *The Structure of Scientific Revolutions*, there are, according to Kuhn, only two substantially different senses of the word which merit further clarification. The first sense refers broadly to all the shared elements – theoretical assumptions as well as observed data – shared by a scientific community. The second refers more specifically to those particular solutions to scientific problems which stimulate further research.

The first usage is tied closely to the conception of a scientific community. By a "scientific community," Kuhn does not mean, in either *The Structure of Scientific Revolutions* or "Second Thoughts on Paradigms," the scientific community as a whole, i.e., all of those who rightfully or wrongfully call themselves "scientists," or all of those who could be said to be engaged in scientific activities. Rather, Kuhn is thinking of specific groups of scientists who investigate a similar set of problems related to a particular scientific field. Hence, within the larger "scientific community," there are many separate subdivisions of scientific communities whose concerns are generally distinct, though occasionally overlapping; in other words, the activities which a scientist engages in are centered around a specific area of specialization which is also an area of specialization for other scientists. The scientists who actively participate in such an area of specialization constitute

the "scientific community" in question.

In "Second Thoughts on Paradigms," Kuhn notes that the concept of a "paradigm" was set forth in *The Structure of Scientific Revolutions* in order to answer the question: "What shared elements account for the relatively unproblematic character of professional communication and for the relative unanimity of professional judgment?"[3] But in this same essay he suggests that less confusion may result if the term "paradigm" is replaced in the sense indicated here with the phrase "disciplinary matrix." Kuhn then singles out three components of a disciplinary matrix which are shared by members of a scientific community: (1) *symbolic generalization,* i.e., the usually mathematized general formulas which can be applied to specific observed data, (2) *models,* which function either as analogies for heuristic purposes, or, at the other extreme, as metaphysical commitments, and (3) *exemplars,* which are concrete solutions to specific problems and which subsequently serve as convenient devices for rationally organizing observed data. Kuhn asserts that this third component of disciplinary matrices, namely exemplars, may be used synonymously with the more specific sense of the term "paradigm" – that is, more specific relative to the larger sense of the term which would include *everything* of a scientific nature that a scientific community agrees upon as prerequisite to its scientific activity.

Keeping in mind that these distinctions regarding the use of the term "paradigm" were made by Kuhn *after* the writing of *The Structure of Scientific Revolutions,* we may proceed with the main argument of that book. We have already mentioned Kuhn's definition of "normal science" and his intended use of the phrase "scientific community." When these concepts are linked, and when it is added that the basic factor which insures relative agreement among the members of a scientific community is the members' commitment to a paradigm, we have, in Kuhn's estimation, a picture of the essential features which constitute the scientific enterprise as it is practiced in such communities. A paradigm, as Kuhn indicated, must be "sufficiently open-ended" for the members of the scientific community to pursue further research, employing the basic concepts which the paradigm offers. Thus, Kuhn describes the normal activities of scientists as being those which are occupied primarily with what he calls "puzzle-solving."

In puzzle-solving activities, the scientists attempt to apply the basic elements of the paradigm to data which hitherto had been unknown or uncollected or they attempt to show how the basic elements of the paradigm can explain or account for data which is already available. Puzzle-solving activities may provide the impetus to collect additional data or to engage in extensive experimentation, but they rarely result by themselves in major conceptual novelties – though, as shall be seen, when combined with certain other factors they can and do. Paradigms have the effect of bringing into focus the central problems which the scientific community sets out to solve, but they can also, in Kuhn's words, "... insulate the community from those... important problems that are not reducible to the puzzle form, because they cannot be stated in terms of the conceptual and instrumental tools the paradigm supplies."[4]

The impetus for a change in the paradigm a scientific community clusters itself around, Kuhn states, comes initially from the awareness of an anomaly – in Kuhn's own words, "... with the

recognition that nature has somehow violated the paradigm-induced expectations that govern normal science."[5] Such anomalies cannot be adequately resolved by appealing to the existing paradigm. They may emerge from research undertaken under the auspices of the existing paradigm, yet even in such situations with the paradigm as his guide, a scientist may not be able to predict or perceive in advance the circumstances which will produce the anomaly. The central question becomes then, for Kuhn, how new paradigms can arise when normal science is not primarily concerned with their production, but instead with the puzzle-solving problems of normal science. Anomalies account, in part, for the rise of new paradigms, but this must be accompanied by a breakdown in the old paradigm in accounting for the data which is possessed.

Kuhn notes that at this stage there may arise a variety of different paradigms which compete to replace the old one. The epistemological question as to what criteria are employed, according to Kuhn, in assessing the validity of the new paradigms which are set forth will be discussed in more detail in the third section of this paper. Since our present concerns are simply with describing how in Kuhn's view conceptual changes occur, we can merely note at this point, that out of the various different paradigms which are set forth, one is selected as being the most adequate for accounting for the data as it is presently known and for accounting for the anomalies which normal science more or less "accidently" uncovers. The new paradigm must meet initially the conditions which Kuhn views as being essential to any paradigm – that is, it must draw the attention of scientists away from the old paradigm, and it must be sufficiently "open-ended" to allow for new research. Once the new paradigm has been adequately tested with respect to its ability to account for known data and for new anomalies, it becomes accepted by the scientific community and replaces the old paradigm as a basis for new research. This, in a nutshell, is Kuhn's view of how conceptual changes in the sciences can be accounted for historically.

Kuhn's account can be criticized on the grounds that it rests upon and gains much of its plausibility from the fact that it limits itself to a consideration of the "hard" sciences, i.e., the physical and biological sciences. Kuhn at times seems to relegate the social sciences, for example, to the status of what he calls "pre-paradigm" disciplines, that is, disciplines in which the practicing members do not have a basis for consensus in a generally agreed-upon paradigm. This may, however (as further research might indicate), point not to inadequacies in the social sciences, but rather to inadequacies in any attempt to expand Kuhn's discussion of paradigms to fields outside of those he specifically treats. If the mistake is avoided of attempting to ground all sciences – the social sciences included – on the model of physics in an all-encompassing "unity of science" movement (as was proposed by Carnap), Kuhn's treatment contains within it the prospects for further investigations into how the conceptual frameworks of the social sciences undergo change. However, since the subject matters and methods are substantially different, a separate inquiry utilizing examples from the social sciences would be necessitated.

Kuhn's strength still lies, however, in the fact that his account of conceptual changes in the sciences limited itself in its essential features to the physical and biological sciences, despite the above criticism. This contrasts somewhat with Fleck's account of conceptual changes in *Genesis and*

Development of a Scientific Fact, in that while Fleck incorporates examples from the history of the concept of syphilis in his account and uses this history as the occasion for his more explicitly philosophical remarks, his use of such terms as "thought style" and "thought collectives" seems to betray his intention of finding a formula for accounting for conceptual changes in a much wider field. While the editors of the English edition of Fleck's work suggest that both its novelty and that which distinguishes it from the work of Weber, Scheler, and Mannheim, lies in Fleck's application of the principles of the social conditioning of thought to scientific knowledge,[6] one still finds traces in Fleck's book, for example, of a conception of a "thought style" which seems much closer to Mannheim's use of the term "Weltanschauung." than to Kuhn's use of the term "paradigm." Although the concept of a "paradigm" is in Kuhn's earlier writings still somewhat nebulous, he later managed to lend a certain amount of precision to that term which Fleck did not or could not do for the term "thought style."

A discussion of Fleck's concept of a thought collective will be reserved for the next section of this paper, but it is important here to attempt to bring into clearer perspective what Fleck meant by a thought style, as this concept figures largely in his account of conceptual changes in the sciences. A thought style, for Fleck, resembles Kuhn's paradigm in that both are seen as providing the foundation for a consensus in scientific matters among a scientific community. Fleck's conception of thought styles has an additional element, however, which Kuhn's conception of paradigms deliberately omits, namely, that a thought style is inescapably linked to the world-view of a particular group within a particular age. Thus, in his historical account of the development of the concept of syphilis, Fleck notes that the earliest conceptions of it were dependent upon a world-view in which the disease was seen as being the result of God's prohibition against fornication, and the concept was at that point in history laden with pre-scientific, mythical overtones. The thought style of the age, however – and this is where the distinction between Kuhn's paradigms and Fleck's thought styles becomes most clear – includes both whatever is actually known scientifically about the concept, however little this may be, *and,* for Fleck, the mythical elements which surround the concept, however farfetched these may also be.

These mythical elements may play an important role, according to Fleck, in the development of any scientific concept and Fleck uses the term "proto-ideas" to describe them. Proto-ideas serve much the same function in Fleck's account of conceptual changes in the sciences, as Kuhn's concept of pre-paradigm states serve in his account. Since proto-ideas are based more upon speculation than upon evidence, it is possible for several conflicting proto-ideas to coexist in any primitive "scientific" community. These proto-ideas compete for the allegiance of supporters; but while investigations based upon evidence can come a long way in eliminating those proto-ideas which can in no way be regarded as viable, Fleck argues that the acceptance of any concept which has developed out of the "proto-idea stage" must be preceded by a readiness on the part of the community to assimilate that concept into the existing thought style. Hence, a concept which is "ahead of its time" cannot be assimilated by the community because the thought style of the community is incapable of assimilating it. Therefore, while thought styles have the positive effect of

stabilizing the state of knowledge at any given moment in history, they also have the negative effect of impeding the development of a particular scientific concept.

A fact, for Fleck, is characterized, in his own words, as a "... signal of resistance opposing free arbitrary thinking ...."[7] This "resistance," however, is not itself arbitrary, but is based upon the evidence obtained by scientific investigation. It is on the basis of this evidence that concepts can undergo change, according to Fleck – that is, when enough evidence has been accumulated which has the effect of breaking the resistance established by the old concept. The new evidence may be resisted by the prevailing thought style, and hence a new thought style must be created if the new concept is to be successfully assimilated by the scientific community. Fleck is not entirely clear on what he takes as being the exact relationship between a thought style and a scientific fact, in that there are times when he seems to indicate that new evidence directly elicits changes in a scientific community's thought style and other times when he seems to indicate that a new concept (presumably based upon the new evidence) cannot be assimilated unless a receptive thought style is already in place. The safest interpretation of Fleck's position might be to say that scientific advancements encounter the least resistance when new concepts and new thought styles develop concurrently, though this thesis is one which would require more precise definitions of the terms "concept" and "thought styles."

Both Fleck and Kuhn emphasize the importance of an appreciation for history in coming to understand the conceptual advances of the sciences. A knowledge of the history of science, according to these writers, can dispel what they feel are certain myths about the status of scientific concepts, such as those generated, for example, by logical positivism. Each author also attempted to show that scientific concepts undergo change and each attempted to describe the process by which this change occurs. Up to this point the emphasis in this paper has been upon scientific concepts *per se*, rather than upon the scientific communities which produce those concepts. We turn now to Kuhn's and Fleck's accounts of how social factors within scientific communities relate to the scientific enterprise.

II. The Sociological Problem

The sociological problem raised by both Kuhn and Fleck is two-fold: first, one must account for how the scientific community as a social group achieves a consensus among its members regarding conceptual frameworks; and second, one must account for how it is possible for there to be divergent points of view regarding conceptual frameworks among members of a scientific community. The emphasis by both Kuhn and Fleck on describing scientists as being formed into social groups seems to indicate that both writers also thought that these questions could be answered in part by sociology. They also point to epistemological issues for both authors, but these will be discussed in detail in the next section.

We have already mentioned Kuhn's concept of a scientific community and have intimated that, for Kuhn, the scientific community's commitment to a paradigm partially insures consensus among the members of that community. Consensus is further maintained, according to Kuhn, by the educational procedure of using textbooks to train the next generation of scientists. Kuhn initially discusses the role of textbooks in a scientific education in

connection with his contention that textbooks typically present only those scientific findings which have been firmly established on the basis of the current state of research. This accounts, he states, for the tendency among scientists not to have an appreciation for the history of science, and hence not to have a sufficient appreciation for the manner in which paradigms become established. While such an appreciation is not absolutely essential for the carrying out of research in normal science or even for introducing paradigm-shifts, a lack of it gives one a distorted view, in Kuhn's estimation, of the viability of present theories. In other words, present theories do not appear as having to undergo similar paradigm-shifts as those in the past did, and this has the effect of increasing resistance to any innovations in a community's paradigm structure.

This effect is not altogether negative, however. What textbooks also point to, according to Kuhn, is the continuity of the scientific tradition. Through textbooks a prospective researcher becomes acquainted with the state of scientific knowledge as it is presently conceived. The mastery of this knowledge is a prerequisite for any serious research which will be assumed by the researcher after he has obtained the necessary intellectual credentials to do so. Only after one has been fully inducted into the scientific tradition, can one begin to undertake research which may result in a paradigm-shift. Kuhn notes that for many, even most scientists, the great majority of their time will be engaged in puzzle-solving activities centered around the paradigms they have learned from the tradition. Only a few, according to Kuhn, will some day find themselves in a position in which their research will require them to begin looking for new paradigms to cover the data their research uncovers. The scientific tradition itself then, as socially transmitted from generation to generation via the use of textbooks, serves as its own guarantor of consensus among the members of a scientific community.

Evidence for one's theories also play a large role in assuring their acceptability to others. After being assimilated into a scientific discipline, the researcher has more of an opportunity to test the theories which he has inherited from the tradition, and it is through this process of testing that the researcher will be able, based upon the further evidence which he has uncovered, to either expand the present state of knowledge which surrounds a paradigm, or, if anomalies must be explained, to introduce innovations into the existing paradigm. The process by which paradigms change, according to Kuhn, has already been described, but Kuhn also notes the sociological factor that resistance towards new paradigms may be encountered from older members of the scientific community who, according to Kuhn, may be more attached to the paradigm they themselves have inherited and have spent a longer time working with. (I do not think Kuhn meant to establish a direct correlation between one's age and one's ability to accept new paradigms; if he did, there are serious difficulties with this view). This resistance is also healthy, however, in that it assures that the members of the scientific community who are proposing a new paradigm will be sure to have their theories carefully examined by those firmly rooted in the tradition before the new theories can be widely accepted.

Thus, according to Kuhn, agreements among members of a scientific community can be accounted for in terms of the intellectual tradition, or more specifically for Kuhn, the paradigm, the

members inherit and they all share in common. No one begins from scratch, so to speak, in formulating scientific theories, and the paradigm is present to guide the researcher. Disagreements among members of a scientific community can be explained in light of what happens when a firmly established paradigm begins to weaken and is no longer held as being entirely viable by the members. New data and new evidence also play an important role in this process, but once the old paradigm comes to be regarded by some members of the scientific community as no longer being valid, they are obligated either to reconcile the new evidence with the old paradigm or to construct a new paradigm which will account for the evidence better. A variety of new paradigms might be proposed, and out of these one must be selected which in the estimation of the scientists involved will be the most adequate. Disagreements, then, can also arise between scientists who set forth conflicting paradigms to account for the new evidence, in addition to the disagreements which arise between those advocating a paradigm-shift and those arguing for the viability of the old paradigm. One can see then a shift from a scientific community in which there is a basic agreement regarding a paradigm to one which is fragmented into competing camps, each struggling to establish the superiority of its own proposed innovations. How these disagreements can be resolved is an epistemological question, but at this point we are concerned only with the process by which consensus is either maintained or broken.

Kuhn speaks of such competition among members of the scientific community at various points in history as constituting the "essential tension" which must exist in any scientific community if progress is to be made in that field. In his essay entitled "The Essential Tension: Tradition and Innovation in Scientific Research," Kuhn makes a clear distinction between "convergent" and "divergent" thinking.[8] "Convergent" thinking is that which is engaged in by a scientist who is attempting to further scientific advancement through his commitment to the scientific tradition. This same scientist may also, however, engage in "divergent" thinking when he believes that such advancement can best be achieved by breaking in some measure with the tradition, i.e., by drawing conclusions which the tradition will not be able to entirely support, at least initially. For there to be "divergent" thinking, however, it must be divergent from something which is established and stable. Hence, the "divergent" thinking of a scientist must always be directed towards those problems which the tradition has uncovered but has been unable to successfully resolve, and it must always seek solutions which have the potential, with further testing, of being reabsorbed by the tradition. "Divergent" thinking which is divergent merely for the sake of being different will not result in fruitful innovations, according to Kuhn.

Turning to Fleck's account, one can see that for Fleck a consensus among the members of a scientific community can be explained in terms of the shared thought styles in any particular group. The notion of a thought style is closely related to Fleck's conception of what he calls a "thought collective" – a term which in Fleck's account replaces Kuhn's notion of a scientific community. We have already remarked on the differences between Kuhn's conception of a paradigm and Fleck's of a thought style, and the differences between a scientific community and a thought collective for the two writers can be reconstructed along much the same lines. Whereas for Kuhn, the

scientific community includes only those specialists who share a scientific commitment to a paradigm, a thought collective for Fleck also includes individuals who are not specialists in a field, but who maintain an interest in it. A thought style is shared both by the specialist and the nonspecialist alike. We turn now to Fleck's analysis of the members who comprise a thought collective.

A thought collective for Fleck is comprised of at least two types of individuals: (1) those who are associated with the thought collective exoterically, and (2) those who are associated with it esoterically. The exoteric circle, according to Fleck, includes all of those who have an interest in a subject, even those who are only familiar with popular presentations of a discipline. The esoteric circle is comprised of those who have undertaken special training in order to acquire the fullest possible knowledge of the subject; these are the specialists and they are the ones who are actually responsible for the continuance and development of a tradition. A person, according to Fleck, may belong to several exoteric circles, though it is unlikely that he will belong to more than one or two esoteric circles. This suggests that in any given society there are a variety of thought collectives which overlap, in the sense that one individual may participate in more than one thought collective at the same time. These thought collectives may center around a religion, a political outlook, or – of special relevance here – a scientific discipline. One can see here why it was mentioned earlier that Fleck's conception of a thought collective could be seen as having some associations with Mannheim's notion of a "*Weltanschauung.*"

It is possible for disagreements to arise between thought collectives whose thought styles are seen as being incompatible – as might be the case, for example, in conflicts between religion and science. Fleck's account also seems to suggest that when disagreements arise between the members of a scientific thought collective, there actually must be two separate thought collectives involved. This can be seen most clearly when one thought collective succeeds another historically, but Fleck does not provide a clear conception of how divergent points of view can arise within the same thought collective. In other works, Fleck's account has no exact parallel to Kuhn's conception of the debates which might take place within a scientific community regarding which of the alternative paradigms should be accepted. Any idea of an "essential tension," such as that found in Kuhn, remains vague and only partially developed in Fleck's book.

Having confronted the fact that disagreements of a nontrivial sort can arise within a scientific community, the essential epistemological question for both Kuhn and Fleck becomes how these conflicts can be resolved. Both writers seem to repudiate the simplicity, however, of any epistemology which is incapable of taking both the historical and the sociological issues they uncovered into account. Hence, for both authors a broader epistemological orientation is required, and to that subject we turn next.

III. The Epistemological Problem

The simplistic notion of epistemology which Kuhn and Fleck both criticized in their own ways may be characterized as follows: The world presents a given, which is comprised of data which must be observed and collected by scientists. Any individual with sufficient mental capacities will be able to observe and collect the same data, and from this data true theories may be derived which,

if certain methodological procedures are followed carefully, will be valid for all scientists at all points in time. The relationship is essentially one in which an observer confronts the world and bases his theories solely upon the evidence there obtained. For both Kuhn and Fleck, however, this formulation does not take the historical and sociological elements just discussed adequately into account. Consider this succinct comment of Fleck's which introduces the notion of a comparative epistemology to replace the above model:

> In comparative epistemology, cognition must not be construed as only a dual relationship between the knowing subject and the object to be known. The existing fund of knowledge must be a third partner in this relation as a basic factor of all new knowledge.[9]

There is nothing inherently wrong in the epistemological formulation which both Kuhn and Fleck criticize; for them it is more a matter of formulating an epistemology which is capable of addressing itself to the findings of the history and sociology of science. Concepts such as an intellectual tradition, paradigms, thought collectives, etc., must be successfully incorporated into a new epistemological orientation, whereas before they had been either ignored or designated as insignificant.

In his article "Objectivity, Value Judgment, and Theory Choice," Kuhn is careful to point out that the epistemological view he expressed in *The Structure of Scientific Revolutions* does not entail that since he thinks the history and sociology of science can play a larger role in epistemology, he thereby endorses a "mob psychology" view of epistemology, i.e., the view that whatever is agreed upon by a scientific community as being true, must therefore be true. In this article Kuhn points out at least five tests which can be applied to any theory to check for its validity. A theory must be (1) accurate with respect to the evidence, (2) consistent both with itself and with other accepted theories, (3) broad in scope, in that it must extend itself beyond particulars, (4) simple, in a sense akin to that of Occam's razor, and (5) fruitful for further investigations. Can a consensus among members of a scientific community be inexorably established on the basis of these five tests alone? According to Kuhn, there are two factors which make an unequivocal "yes" to this question problematic, and he states them as follows:

> Individually the criteria are imprecise: individuals may legitimately differ about their application to concrete cases. In addition, when deployed together, they repeatedly prove to conflict with one another; accuracy may, for example, dictate the choice of one theory, scope the choice of its competitor.[10]

It is unlikely, then, that every theory which is regarded as being valid will be able to meet all five criteria, although some of these criteria undoubtedly must be met. Initially Kuhn accepts a distinction between objective and subjective factors which play a role in theory choice. The objective factors, such as the five mentioned above, are generally shared by the members of a scientific community, but Kuhn notes that the importance which has been attached to each individual criterion has varied historically. Furthermore, Kuhn is willing to say that factors such as "... idiosyncratic factors dependent on individual biography and personality"[11] – elements which

may be regarded as subjective – also play a role in determining which theories will be accepted. Kuhn states this as a matter of historical fact and offers several examples to support this contention. If this is so, an epistemology is required which can take these subjective elements into account, and not merely relegate them to the status of human imperfections in the scientific enterprise.

These issues seem to have been brought into clearest focus for Kuhn when he discussed in *The Structure of Scientific Revolutions* the disagreements which arise in a scientific community when a shift in paradigms is underway. Disagreements cannot be resolved by appealing to either the existing paradigm or the new one which is attempting to take its place. In Kuhn's words, "The competition between paradigms is not the sort of battle that can be resolved by proofs."[12] While the five tests mentioned above will play a role in determining the acceptance or rejection of a newly proposed paradigm, Kuhn suggests that two scientists who accept different paradigms will see the evidence in different lights:

> Practicing in different worlds, the two groups of scientists see different things when they look from the same point in the same direction. Again, that is not to say that they can see anything they please. Both are looking at the world, and what they look at has not changed. But in some areas they see different things, and they see them in different relations one to the other. That is why a law that cannot even be demonstrated to one group of scientists may occasionally seem intuitively obvious to another. Equally, it is why, before they can hope to communicate fully, one group or the other must experience the conversion that we have been calling a paradigm shift. Just because it is a transition between incommensurables, the transition between competing paradigms cannot be made a step at a time, forced by logic and neutral experience. Like the gestalt switch, it must occur all at once ... or not at all.[13]

A shift in paradigms, then, is similar to a conversion in Kuhn's estimation. The evidence remains constant, but the manners of interpreting it differ.

If it is recognized that such differences can coexist within a scientific community at a given point in history, such differences become much more recognizable when considering them in light of the various theories which have been adhered to at various points in history. It hardly needs to be said, using the example of the development of the concept of syphilis provided by Fleck, that different theories are thought of as being valid in different historical eras. What effect does this have on any notion of validity? This question was taken up in greater detail by Fleck, and he suggested that an awareness of the historical development of scientific concepts necessitates a new epistemological orientation. The problem is brought out very clearly in this passage from *Genesis and Development of a Scientific Fact*:

> The history of any scientific concept could be immaterial to those epistemologists who consider, for instance, the errors of Robert Mayer of no significance to the development of the law of conservation of energy. Against this we would argue that there is probably no such thing as complete error or complete truth. Sooner or later a modification of the law of

conservation of energy will prove necessary, and then we will perhaps be obliged to fall back upon an abandoned "error."[14]

For Fleck this observation does not mean that science does not come to have an increasingly accurate picture of the world. Nor does it mean that one theory is as good as any other in describing reality. As Fleck's book shows, the modern concept of syphilis is much more adequate than say, medieval conceptions of it, partly because the modern age does in fact know more about syphilis and partly because the results which have been obtained in curing the disease seem to confirm this conclusion. What further distinguishes the modern concept of syphilis from its precursors is that the thought styles into which the various concepts of the disease have been assimilated are entirely different. A recognition of this seems to require for Fleck a different understanding of the concept of validity. Yet, while Fleck adopts an essentially historicist view of validity, suggesting that whatever is regarded as being valid is so regarded because of its harmony with the prevailing thought style, the accumulation of additional evidence and the results of further research are indeed what makes progress in the sciences possible.

Some additional comments need to be made about the notion of an "accumulation of knowledge," and these comments can serve to round out and close our discussion of the epistemological issues at hand. For both Kuhn and Fleck, the progress of knowledge cannot be seen as the mere accumulation of new knowledge which is simply added to the knowledge which already exists. As knowledge progresses, theories which were once regarded as being valid will later have to be rejected in favor of new theories which better account for what is known. For Kuhn, the concept of accumulation is most notable in the puzzle-solving activities of scientists in which they endeavor to find new data to support an existing paradigm. But when paradigms shift, much that was previously thought of as being valid will have to be rejected. In *The Structure of Scientific Revolutions,* Kuhn makes it appear, however, as if a paradigm shift involves a rejection of all that past achievements had accomplished. Kuhn's later writings (such as the article "The Essential Tension" discussed above) dispel this idea somewhat by noting that present achievements are always built upon past achievements and that remnants of past achievements may continue to exist in modified forms in the present state of knowledge. Stephen Toulmin, in *Human Understanding: The Collective Use and Evolution of Concepts,* criticized Kuhn for not taking note of this in *The Structure of Scientific Revolutions.* Conceptual change may not be as "revolutionary" as Kuhn thought, according to Toulmin, since changes are always modifications on what is already known at any given point in time by the sciences. Fleck took notice of this fact when he wrote, "... every age has its own dominant conceptions as well as remnants of past ones and rudiments of those of the future."[15]

It is interesting to note, however, how both Kuhn and Fleck adopted an evolutionary view of knowledge, and in fact acknowledge a connection between their theories of conceptual change and the biological theories of Darwin. Fleck, for example, speaks of the "adaptability" of concepts to an existing thought style; he describes conceptual changes as "mutations."[16] In *The Structure of Scientific Revolutions,* Kuhn presents a rather thorough comparison of his own ideas with

Darwin's. The really radical claim which Darwin had made, according to Kuhn, was not that new species evolve out of lower forms of life, but that the evolutionary process has no ultimate goal set by nature in advance. Does knowledge, then, have the ultimate goal of constructing a perfect picture of the nature of the world, as was thought, for example, by Wittgenstein in his *Tractatus Logico-Philosophicus*? It would seem, however, that such a goal must be present in the minds of scientists if they are to have any notion that this concept, this theory, or this paradigm is better than ones prior to them. And Kuhn is treading on dangerous ground, perhaps, when he minimizes the importance of such goals. What Kuhn's and Fleck's works show, however, is that there are many factors present within the scientific enterprise which present obstacles to the attainment of any such goal. As descriptive accounts, Kuhn and Fleck succeed in pointing out what these obstacles are, but if Kuhn's and Fleck's writings are taken in a normative sense, they have still left unsolved the epistemological problems they have in fact merely taken notice of.

Concluding Remarks

Kuhn and Fleck have opened up new areas of research for the philosophy of science by noting that the history and sociology of science are subjects which must be taken into account if any accurate picture of what takes place in the scientific enterprise is to be obtained. To use Kuhn's own phrase, a "paradigm shift" in our understanding of the scientific enterprise has been introduced by both Kuhn and Fleck which stands in direct contrast to that offered by, say, logical positivism. To pursue the analogy even further, if such a "paradigm shift" has in fact taken place, it is now susceptible to further debate, additional research, and more concrete evidence. Though this paper has not concentrated on contrasting Kuhn's and Fleck's positions with those of logical positivism, one who is familiar with both perspectives can see that the ancient controversy between Parmenides and Heraclitus has been restated in modern terms. Opposed to a static conception of knowledge, Kuhn and Fleck have proposed a view of knowledge in which knowledge is seen as dynamic and in a state of flux, though developing towards increasingly more adequate scientific conceptions of the world.

NOTES

1. Thomas S. Kuhn, *The Structure of Scientific Revolutions,* 2nd ed. (Chicago: The University of Chicago Press, 1970), p. 10.
2. *Ibid.*
3. Thomas S. Kuhn,"Second Thoughts on Paradigms," in *The Essential Tension: Selected Studies in Scientific Tradition and Change* (Chicago: The University of Chicago Press, 1977), p. 297.
4. Kuhn, *The Structure of Scientific Revolutions,* p. 37.
5. *Ibid.*, pp. 52–53.
6. See Ludwik Fleck, *Genesis and Development of a Scientific Fact,* ed. Thaddeus J. Trenn and Robert K. Merton, trans. Fred Bradley and Thaddeus J. Trenn (Chicago: The University of Chicago Press, 1979), p. 163.
7. *Ibid.*, p. 101.
8. See Thomas S. Kuhn, "The Essential Tension: Tradition and Innovation in Scientific Research" in *The Essential Tension, op. cit.,* pp. 225–239.

9. Fleck, p. 38.
10. Thomas S. Kuhn, "Objectivity, Value Judgment, and Theory Choice," in *The Essential Tension, op. cit.*, p. 322.
11. *Ibid.*, p. 329.
12. Kuhn, *The Structure of Scientific Revolutions*, p. 148.
13. *Ibid.*, p. 150.
14. Fleck, p. 20.
15. *Ibid.*, p. 28.
16. *Ibid.*, pp. 25–26.

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