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An active member of the Faculty of Education and of the Faculty of Arts and Sciences at Harvard University from 1952 until his retirement in 1992, Professor Scheffler also served as co-director of the Philosophy of Education Research Center at Harvard University (PERC) and studied topics related to leadership, training and education for the Mandel Institute, as part of its collaborative program of scholarly research with PERC.

EDITOR'S NOTE

Current debate over the nature and limits of objectivity has challenged some of our fundamental assumptions and values. This debate presents a strong challenge to education. The way objectivity is viewed by educators and educational policy-makers has serious implications for many areas of educational practice, including the teaching of science, education for democracy, moral and religious education and the study of education itself.

It was in order to address this challenge that the Mandel Institute turned to Professor Israel Scheffler and invited him to share his thoughts with educators and scholars in Jerusalem. Professor Scheffler has carefully analyzed the idea of objectivity and its implications for education and has published on this topic in a number of his works, including "Conditions of Knowledge: An Introduction to Epistemology and Education" and "In Praise of the Cognitive Emotions".

During his visit to Jerusalem, Professor Scheffler delivered the second lecture in the Mandel Institute's "Distinguished Lecturer Series." The lecture, entitled "Change and Objectivity in Science; Reflections on Kuhn," was also the subject of several meetings between Professor Scheffler and educational leaders from Israel and the Diaspora under the auspices of the School for Educational Leadership (SEL) and the Center for Advanced Professional Educators (CAPE). It appears here as the second of our "Monographs from the Mandel Institute."

D. Marom

On change and objectivity in science: Reflections on Kuhn

By Israel Scheffler

Twenty-nine years have now passed since my book *Science and Subjectivity* appeared, offering a reinterpretation and defense of the ideal of objectivity in science.[1] Prompted by the then mounting criticisms of a fixed observational given, a constant descriptive language and a shared methodology of theory evaluation, I took as my task to show how, if at all, scientific objectivity is indeed possible. For my conviction was, and is, that without objectivity as a guiding ideal for practice, there can be no science or, indeed, any rational deliberation whatever.

The overall tendency of the attack on objectivity thus called into question the very conception of scientific thought as a responsible enterprise, threatening us with the extreme alternative that theory is not controlled by data, but that data are manufactured by theory; that rival hypotheses cannot be rationally evaluated, there being no neutral court of observational appeal nor any shared stock of meanings; that scientific change is a product, not of evidential appraisal and logical judgment, but of intuition, persuasion and conversion; that reality does not constrain the thought of the scientist but is rather itself a projection of that thought.

My book offered a many-sided critical response to the assault on scientific objectivity, in one chapter of any book, I dealt with anti-objectivist arguments basing themselves on the history of science in particular, and it is these arguments I shall address here, as presented in Thomas S. Kuhn's important book, The Structure, of Scientific Revolutions[2]. Professor Kuhn's work has been enormously influential, and it has affected the thinking of various specialists in different disciplines of inquiry as well as the attitudes of the general public. Indeed, I believe that Kuhn's conceptions and lines of argument are now even more influential than they were when my dissenting views first appeared 29 years ago. In a recent paper, the physicist Steven Weinberg has

written, for example, that "there are postmoderns who not only doubt the objectivity of science but dislike objectivity, who would welcome something warmer and fuzzier than modem science....In this respect [writes Weinberg] my friend Thomas Kuhn has a lot to answer for. He distances himself from the postmoderns and the social constructivists but he is endlessly quoted by them."[3]

In the presentation to follow, 1 shall (1) expound Professor Kuhn's view, then (2) review the main criticisms of this view offered in my book, and (3) discuss some of the ensuing replies and rejoinders, ending with (4) a concluding statement on scientific objectivity.

1. Professor Kuhn's View

I turn first to an exposition of Professor Kuhn's ideas as a preliminary to my critical discussion. A basic distinction that he draws is that between normal science and scientific revolutions. Unlike the earlier prevalent view, which sees science as all of a piece, Kuhn's treatment emphasizes a marked division between the normal and the revolutionary variety. Normal science is based on certain past scientific achievements involving "law, theory, application, and instrumentation together." Such achievements "provide models from which spring particular coherent traditions of scientific research" (p. 10). It is these achievements that Kuhn terms "paradigms" and it is the study of such paradigms... [that] mainly prepares the student for membership in the particular scientific community with which he will later practice... Men whose research is based on shared paradigms are committed to the same rules and standards for scientific practice. That commitment and the apparent consensus it produces are prerequisites for normal science, i.e. for the genesis and continuation of a particular research tradition" (pp. 10-11).

Paradigms "attract...adherents away from competing modes of scientific activity.., [but are] sufficiently open-ended to leave all sorts of problems for the redefined group of practitioners to resolve" (p. 10). "Like an accepted judicial decision in the common law, [a paradigm] is an object for further articulation and specification under new or more stringent conditions" (p. 23). Indeed, a paradigm is severely limited upon its first appearance, having gained its repute in solving a few recalcitrant problems, while holding out the promise of further progress with continued application. Such continued application in response to the promise of a paradigm is what Kuhn calls "normal science." Thus, he writes, "The success of a paradigm—whether Aristotle's analysis of motion, Ptolemy's computations of planetary position, Lavoisier's application of the balance, or Maxwell's mathematization of the electromagnetic field—is at the start largely a promise of success discoverable in selected and still incomplete examples. Normal science consists in the actualization of that promise, an actualization achieved by extending the knowledge of those facts that the paradigm displays as particularly revealing, by increasing the extent of the match between those facts and the paradigm's predictions, and by further articulation of the paradigm itself" (pp. 23-24).

The activity of normal science is puzzle-solving, a puzzle being unlike a genuine problem in being both assured of a solution and restricted by rules, both features provided by the dominant paradigm (pp. 36-42). Normal science is highly cumulative but "does not aim at novelties of fact or theory and, when successful, finds none" (p. 52). However, it leads to anomaly, which, when sufficiently pervasive, constitutes a crisis, "the persistent failure of the puzzles of normal science to come out as they should" (p. 68).

Confronted by crisis, how do scientists respond? Faced by even severe and prolonged anomalies, they do not renounce the paradigm that has led them into crisis. However, "they may begin to lose faith and then to consider alternatives" (p. 77). The activity of normal science begins to give way. For in this situation, "scientists take a different attitude toward existing paradigms, and the nature of their research changes accordingly. The proliferation of competing articulations, the willingness to try anything, the expression of explicit discontent, the recourse to philosophy and to debate over fundamentals, all these are symptoms of a transition from normal to extraordinary research" (p. 91). Eventually a new paradigm emerges, sometimes foreshadowed by such extraordinary research, sometimes all at once "in the mind of a man deeply immersed in crisis" and in a manner that may remain forever inscrutable (p. 90). The transition to a new paradigm, incompatible with the old one, is a revolution.

Like a political revolutionary situation, a revolutionary situation in science involves "a choice between incompatible modes of community life… the choice is not and cannot be determined merely by the evaluative procedures...of normal science, for these depend in part upon a particular paradigm, and that paradigm is at issue. When paradigms enter, as they must, into a debate about paradigm choice, their role is necessarily circular. Each group uses its own paradigm to argue in that paradigm's defense....the status of the circular argument is only that of persuasion. It cannot be made logically or even probabilistically compelling for those who refuse to step into the circle....As in political revolutions, so in paradigm choice—there is no standard higher than the assent of the relevant community" (p. 94).

Paradigm debates are thus "consistently" characterized by an "incompleteness of logical contact" (p. 110) between the proponents of competing paradigms. Circularity

is, as we have seen, the rule, each paradigm being defended in terms of "the criteria that it dictates for itself" (p. 110), while disagreements among the competing schools as to what is a suitable problem and what is a solution guarantee that "they will inevitably talk through each other when debating the relative merits of their respective paradigms" (p. 109). Since the choice between rival paradigms lies beyond the capacities of normal science to resolve, hinging upon considerations external to it, the issue in a paradigm debate is indeed revolutionary, involving a fundamental reconsideration and potential redefinition of normal science itself.

Paradigm debates, moreover, reconstitute not only science but nature as well (p. 110), for the transition to a new paradigm occurs "not by deliberation and interpretation, but by a relatively sudden and unstructured event like the gestalt switch. Scientists then often speak of the 'scales falling from the eyes' or of the 'lightning flash' that 'inundates' a previously obscure puzzle, enabling its components to be seen in a new way that for the first time permits its solution" (p. 122).

Since scientists see the world differently after a paradigm change, and since they have recourse to the world only "through what they see and do, we may want to say that after a revolution scientists are responding to a different world" (p. 111). For example, we may well say that "after discovering oxygen, Lavoisier worked in a different world" (p. 118).

A new paradigm emerges first in a single mind, or in a few. "How are they able, what, must they do, to convert the entire profession or the relevant professional subgroup to their way of seeing science and the world?" (p. 144). The transition from one to another is "a transition between incommensurables" and hence "cannot be made a step at a time, forced by logic and neutral experience. Like the gestalt switch, it must occur all at once (though not necessarily in an instant) or not at all" (p. 150). Very often it never occurs. In any event, Kuhn states "that in these matters neither proof nor error is at issue. The transfer of allegiance from paradigm to paradigm is a conversion experience that cannot be forced" (p. 151). However, a general shift of paradigm may take place through a spread of conversions. "Conversions will occur a few at a time until, after the last holdouts have died, the whole profession will again be practicing under a single, but now a different, paradigm" (p. 152).

What factors account for the change in viewpoint of the new generation? Some factors, says Kuhn, lie outside science altogether, some relate to personality, nationality or- reputation (pp. 152-3). Other factors relate to claims that the new paradigm will solve the crisis problem or will prove aesthetically appealing. But such claims cannot be sufficient. A decision is called for, and "that decision must be based less on past achievement than on future promise. The man who embraces a new paradigm at an early stage must often do so in defiance of the evidence provided by problem-solving. He must, that is, have faith that the new paradigm will succeed with the many large problems that confront it, knowing only that the older paradigm has failed with a few. A decision of that kind can only be made on faith" (pp.157-8).

The result of a scientific revolution must, however, be progress. As Kuhn sees it, "Revolutions close with a total victory for one of the two opposing camps. Will that group ever say that the result of its victory has been something less than progress? That would be rather like admitting that they had been wrong and their opponents right. To them, at least, the outcome of revolution must be progress, and they are in an excellent position to make certain that future members of their community will see past history in the same way" (p. 166),

Nevertheless, it is not wholly true that might makes right in science. The authority for choice of paradigm is vested in the scientific profession, which strives for general agreement, and which is, moreover, concerned that the new paradigm should not lose previously acquired problem-solving ability and should seem, further, to solve new problems not otherwise soluble (PP. 167-169).

2. Criticisms of Kuhn's View

Professor Kuhn's treatment evoked a number of critical responses. Some writers found the notion of a paradigm ambiguous or obscure. Margaret Masterman, for example, claimed 21 different senses of the term in Kuhn's writing.[4] Others, for example Stephen Toulmin, criticized the distinction between normal and extraordinary research,[5] and still others, for example Leonard K. Nash, who had been a teaching collaborator with Kuhn at Harvard, argued against the notion of scientific revolutions, defending instead the idea of an evolution of scientific thought.[6] My own critique focused particularly on Kuhn's claim that debates over rival paradigms are not matters of deliberation and interpretation but rather matters of conversion and persuasion. The rise of a new paradigm is described by Kuhn as consisting in a large series of individual conversion experiences - "the transfer of allegiance from paradigm to paradigm is a conversion experience" (p. 151) - producing "an increasing shift in the distribution of professional allegiances" (p. 158). The picture is rather like that of an epidemic.

Evaluative arguments over the merits of alternative paradigms are vastly minimized in Kuhn's treatment. By contrast, he lays particular stress on the alteration of thought and experience concomitant with adoption of a new paradigm. Such alteration, being "intuitive" "relatively sudden" and "unstructured" is assimilated more naturally to gestalt reorganization of perception, he argues, than to the "piecemeal" articulations associated with deliberation or interpretation" (pp. 122-3).

In response, I claim that this sort of consideration is irrelevant to the critical issue, which concerns not the psychology of one who originates or adopts a new paradigm but rather the public procedures by which a new paradigm is assessed. Even if it is true that, as Kuhn declares, "No ordinary sense of the term 'interpretation' fits these flashes of intuition through which a new paradigm is born" (p. 123), it does not follow that the notion of interpretation also fails to apply to the processes by which the new paradigm is defended and criticized in the scientific community to which it is addressed.

If a new paradigm produces a reorganization of perception, such reorganization is not typically accepted without argument as constituting a self-evident advance. Scientists do not simply appeal to their own conversion experiences in defending a new paradigm. On the contrary, the very existence of paradigm debates testifies to their belief that independent reasons are available to them which can sustain themselves in critical discussion of alternatives.

It is logically conceivable, to be sure, that scientists are simply deluded about their own arguments, that their paradigm debates are always without substance and, at bottom, a mere interplay of persuasive rhetorical effects. One could not, however, show this merely by arguing from the psychology of perception; one would need to point to the content of the debates themselves. Nor would particular examples, from the history of science, of debates conducted at cross-purposes suffice. For the existence of common evaluative criteria is consistent with borderline regions where such criteria yield no clear decisions. Moreover, even the availability of clear decisions is compatible with honest differences of judgment, not to mention plain misunderstandings.

Professor Kuhn, however, offers another argument to show that paradigm change is always a matter of conversion rather than deliberation; this argument hinges on his contrast between normal science and what lies outside normal science. "Paradigms," he writes, "are not corrigible by normal science at all" (p. 122). Or, again, "the choice between competing paradigms regularly raises questions that cannot be resolved by the criteria of normal science. To the extent, as significant as it is incomplete, that two scientific schools disagree about what is a problem and what a solution, they will inevitably talk through each other when debating the relative merits of their respective paradigms" (p. 109). If significant debate based on normal science is thus ruled out on general grounds, it seems plausible by this line of reasoning, to have recourse to conversion as the ultimate determinant of paradigm change.

I submit, however, that Kuhn's argument begs the question. Normal science, after all, has been explained by him as just the sort of research that is paradigm-bound. Is it any wonder then that the higher-level issue of paradigm choice outstrips the capability of normal science? To assume further, however, that deliberation and interpretation are restricted to normal science begs the very point at issue. For the

question is just whether deliberation and interpretation are not also relevant to the extra-normal problem of paradigm choice. If scientific schools inevitably talk through each other when arguing from within their respective paradigms, it is not further inevitable that they do always argue from within their respective paradigms. What independent reason can there then be for supposing paradigm debates to be mere persuasive displays without deliberative substance in the face of contrary testimony by the participating scientists themselves?

Kuhn replies to this question by reference to the so-called incommensurability of different paradigms. Competing paradigms, he elaborates, are addressed to different problems; they embody different standards and even differing definitions of science. They are based on different meanings and operate in different worlds (pp. 147-150). Thus "the proponents of competing paradigms are always at least slightly at cross purposes" (p. 148). A fundamental idea suggested in such passages is that competing paradigms, like radically different languages composed of wholly different elements, do not allow a point-by-point translation of content from one framework to the other. "Before they can hope to communicate fully," according to Kuhn, "one group or the other most 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 (though not necessarily in an instant) or not at all (p. 150).

This argument seems to me, however, to harbor the following difficulty: If competing paradigms are indeed based in different worlds, and address themselves to different problems with the help of different standards, in what sense can they be said to be competing? To declare them in competition is, after all, to place them within some common framework, to view them within some shared perspective supplying, in principle at least, comparative and evaluative considerations applicable to both. But if this is in fact the case, a basis for reasonable comparison must indeed be presumed to exist.

The main point, I emphasize, is that lack of commensurability, in the sense here considered, does not imply lack of comparability. Comparison is not limited to an

effort at what Kuhn calls "communication across the revolutionary divide" (p. 149); it need not translate, step by step, one paradigm into another, any more than art criticism need translate one work of art into another. Having appreciated the differing potentials of competing paradigms, the scientist, like the art critic or indeed the historian, may step back and consider the respective bearings of the paradigms with regard to issues he holds relevant. Such consideration is not formulated within, nor is it bound by, the paradigms which are its objects. It is, instead, an exercise in what W. V. Quine calls "semantic ascent," belonging to a second-order reflective and critical level of discourse.[7] The incommensurability of paradigms is thus no bar to their reasonable comparison.

To be sure, such comparison presupposes a certain sharing of standards at the second-order level if it is to characterize paradigm debates within the scientific community and not simply the process of comparison within the mind of a single scientist; but is such sharing possible? Are not scientists of rival schools also divided by incommensurable comparative paradigms of the second order? Will not any two scientists of differing first-order persuasions inevitably step back to differing evaluative positions?

Certain passages in Kuhn's treatment suggest such a view. Thus, "in learning a paradigm" he writes, "The scientist acquires theory, methods, and standards together... Therefore, when paradigms change, there are usually significant shifts in the criteria determining the legitimacy both of problems and of proposed solutions....the choice between competing paradigms regularly raises questions that cannot be resolved by the criteria of normal science.... in the partially circular arguments that regularly result, each paradigm will be shown to satisfy more or less the criteria that it dictates for itself and to fail short of a few of those dictated by its opponent" (pp. 109-110). This passage suggests that the sharing of second-order standards is impossible. For to accept a paradigm is to accept not only theory and methods but also standards or criteria that serve to justify the paradigm as against its rivals. Paradigm differences are thus inevitably reflected upward, in criterial differences at the second level. Each paradigm is thus, for Kuhn, inevitably self-justifying, and paradigm debates must in the end consist merely of mutual efforts to induce conversions in opponents with whom one cannot actually communicate.

This argument, as I argued in my book, rests on a confusion of standards or criteria internal to a paradigm with those by which the paradigm is itself judged. On the one hand, we are told that a paradigm incorporates not only information about the entities nature contains, but also standards representing criteria of legitimacy for scientific problems and proposed solutions. The paradigm, as Kuhn puts it, provides scientists "not only with a map but also with some of the directions essential for map-making" (p. 109). So far, we are concerned with standards internal to a paradigm.

On the other hand, when we turn to the latter part of the passage just quoted, we are no longer considering standards of the same sort. For the issue is no longer the choice of problems or solutions within the scientific domain in question, but rather the choice of paradigms themselves; these are, in effect, external criteria. In terms of Kuhn's map metaphor, our problem is now not to construct a map of some region in accord with a designated set of directions for map-making, but rather to compare and evaluate alternative sets of directions for mapping. It is clear that the criteria by reference to which the latter task is carried out are of a different order from, and independent of, the criteria embodied in any set of mapping rules. For no such set of rules implies how it is itself to be evaluated in comparison with alternative sets. And, certainly, no such set implies that it is itself to be ranked as superior to its alternatives. But then it is simply gratuitous to suppose that each paradigm "dictates" such second-order criteria which serve always to justify it as against its alternatives, thus precluding objectivity in a shared process of paradigm evaluation. There is no necessity that paradigm differences must be reflected upward at the second level or that, generally, differences at any level must be reflected upward to the next highest level. There is thus no need to suppose that paradigm debates at any level necessarily must fail of objectivity.

Professor Kuhn offers, however, one final reason for characterizing paradigm debates in terms of conversion rather than deliberation: this is the apparent resistance of paradigms to falsification. "Once it has achieved the status of a paradigm", Kuhn writes, "a scientific theory is declared invalid only if an alternate candidate is available to take its place" (p. 77). Furthermore, acceptance of the new paradigm defies the evidence of the greater problem-solving ability of the old paradigm, and depends on faith (p. 158). 'The competition between paradigms" says Kuhn, "is not the sort of battle that can be resolved by proofs" (p. 143).

To this, I reply that acceptance in science is never a matter of proof, whereas acceptance of a relatively unsupported hypothesis is compatible with the acknowledgment of tests to which future experience will subject it. The notion of acceptance, moreover, is ambiguous. It need not always mean the taking of a proposition to be true; it may imply simply taking the proposition seriously enough to use it as a leading principle of research and experimentation without commitment as to its truth value. In any case, interest in objectivity concerns not the state of mind of "the man who embraces a new paradigm at an early stage" and who, according to Kuhn, "must often do so in defiance of the evidence provided by problem-solving" (p. 158). What is crucial is the existence of shared institutions of control by which paradigms, once adopted, are tested.

Kuhn argues that rejection of a paradigm in crisis always awaits a new paradigm. But the notion of rejection is as ambiguous as that of acceptance. It may mean rejection of a proposition as true, or loss of confidence in its truth. It may also mean rejection of a proposition as a guide to research or as a practical tool. It is quite consistent to reject a proposition as true but yet to continue to use it as a tool of research, experimentation or technology. It is further consistent to reject a proposition as true but to hold true a modification of it that removes its conflict with counterinstances.

In fact, Kuhn seems to me to admit the critical point in allowing that, although scientists "do not renounce the paradigm that has led them into crisis," they "may begin to lose faith and then to consider alternatives" (p. 77), taking a "different attitude toward existing paradigms," expressing "explicit discontent" and even having "recourse to philosophy" (p. 91). Such response amounts to, or approximates, a rejection of the paradigm as true, but does not therefore constitute a rejection in other senses of the word, The significance of this point is masked by the global use of such notions as acceptance, rejection and the term "paradigm" itself, which embraces not only theories but also modes of practice.

In fact, as it seems to me, Kuhn ends by reintroducing the very ideas he began by denying. Despite his stress on incommensurability, for example, he writes that "the successful new theory must somewhere permit predictions that are different from those derived from its predecessor. That difference could not occur if the two were

logically compatible" (p. 97). He wants, in this quotation, to deny the cumulativeness of theories, but, in the course of this denial, he allows a predictive criterion as relevant to their comparative evaluation. Further, he seems not to notice that, for two theories to be logically incompatible, they must share enough language to express the proposition in contention and its denial; they cannot be wholly incommensurable.

Kuhn opposes the received notion of falsification, but himself introduces the concepts of anomaly and crisis, which play a parallel role in his account. He downgrades deliberation in paradigm change but yet allows the importance of claims that the new paradigm will solve the problems that led the old one into crisis (p. 153), implicitly granting that the very same problems can be acknowledged by both. He criticizes the notion of cumulative science, yet insists that "new paradigms…. usually preserve a great deal of the most concrete parts of past achievement" (p. 169), again, despite himself, implying commensurability. Finally, he offers several considerations relative to the critical evaluation of theories: the predictive criterion, as we have seen, the existence of anomaly and crisis, the preservation of previously acquired problem-solving abilities and the promise "to resolve some outstanding and generally recognized problem that can be met in no other way" (p. 169). Such conditions of evaluation seem to me to contradict the main thesis, appealing to the history of science, with which we have been concerned—namely, that paradigm change in science is not generally subject to deliberation and critical assessment. On the contrary, they seem to me to show objectivity to be an operative and controlling ideal of scientific practice.

3. Replies and Rejoinders

Professor Kuhn replied to criticisms of his work in an essay entitled "Postscript - 1969," included in the second edition of *The Structure of Scientific Revolutions* as well as in an article called "Reflections on My Critics" in the Lakatos and Musgrave volume, *Criticism and the Growth of Knowledge.*[9]

In the latter article, he claims that there are many "good reasons for theory choice"—"reasons of exactly the kind of philosophy of science: accuracy, scope, simplicity, fruitfulness, and the like.[10] They are, however, he insists, "values" rather than "rules of choice", and may be applied differently by different scientists. "Simplicity, scope, fruitfulness and even accuracy can be judged quite differently (which is not to say they may be judged arbitrarily) by different people".[11]

In my paper "Vision and Revolution" (1972), [12] I replied that I had never argued that there must be rules of choice applied uniformly by scientists. The main point, it seemed to me, is that if the "good reasons" Kuhn mentions are not, in being conceived as values, to be construed as utterly free off all constraints, or else themselves paradigm-dependent, then they allow a shared comparison of rival paradigms, and make paradigm debates intelligible. In denying that such reasons "may be judged arbitrarily" Kuhn apparently agrees that they are neither free of all constraints nor paradigm-dependent. But this view undercuts his earlier view that proponents of competing paradigms "will inevitably talk through each other" (p.109).

In his "Postscript", Kuhn writes, "Because I insist that what scientists share is not sufficient to command uniform assent…, I am occasionally accused of glorifying subjectivity and even irrationality" (p. 186). My reply is that the issue "is not uniformity but objectivity, and objectivity requires simply the possibility of intelligible debate over the comparative merits of rival paradigms."[13]

4. A Concluding Statement

The two controlling metaphors used by Kuhn to replace the traditional categories of interpretation and deliberation in describing scientific change are those of vision and revolution. But they are incongruous and neither, taken alone, in fact supports the view of paradigm change offered by Kuhn. What supports it is rather a hybrid metaphor that resembles neither vision nor revolution, but is violated by both. Take first the topic of vision. Kuhn plausibly compares the alteration of thought accompanying a change of paradigms to a gestalt reorganization of perception. However, he employs the metaphor of revolution in conjunction with that of the gestalt switch. Alluding to the reversible duck-rabbit figure. Kuhn declares that "What were ducks...before the revolution are rabbits afterwards" (p. 111). And the notion of revolution is further elaborated: competition is visualized as combat, with victory the prize. Moreover, victory always defines scientific progress because the winning camp rewrites history as portrayed in the dominant textbooks.

Now the metaphors of vision and of revolution are at odds with one another. To be sure, the reversible figure and the case of revolutionary conflict both display a certain mutual exclusiveness of elements. That is, you see either the duck or the rabbit at any moment, and you side with one party or the other in a revolutionary conflict. But the notion of an opposition of claims applies only to the latter case. A revolution involves opposed loyalties and allegiances, conflicting judgments and claims. However, there are no analogous questions of loyalty, allegiances or conflicting claims in the case of a reversible figure. There is no question of arguing over the relative merits of the duck or the rabbit as the proper and exclusive view of the duck-rabbit figure; there is no duck-party seeking victory over the rabbit-party or aiming to consign it to the dustbin of history. Nothing in this situation corresponds to paradigm debates, nor are there anything like "good reasons" offered to support the one or the other side.

The case of revolution is quite different. Each side seeks victory, demands exclusive allegiance, propounds arguments and rebuts the arguments of the opposition. To reduce revolutionary combat to a gestalt switch leaves out the critical aspects of

advocacy and opposed loyalties, claim and counterclaim. Whereas to offer the gestalt switch as a case of revolutionary transition is to import inapplicable concepts of advocacy, commitment and party combat. If, finally, we imagine that the spontaneous intuitive process by which the duck replaces the rabbit carries with it a commitment that is exclusive, we have formed a hybrid notion true neither to gestalt perception nor to revolution.

Now, if we apply this hybrid notion to paradigm change in science, we are led to think of such change as determinative of commitment but itself immune to deliberation. This approximates Kuhn's view of paradigms: they are neither acquired nor altered through interpretation, but it is they which determine party allegiances. Adopting a new paradigm is, for Kuhn, not just like seeing the rabbit for the first time in the duck-rabbit figure. The paradigmatic vision demands acceptance; it seeks to triumph over other paradigms and, when victorious, stamps itself into the records and textbooks under its control

The hybrid conception is true neither to vision nor to revolution, but it perpetuates an old philosophical mistake: deriving values from visions. Values, however, cannot he seen, nor are visions judgments or the premises of judgments. Value judgments are certainly judgments; they make claims and invite discussion, interpretation and deliberation. When paradigms are in opposition, the conflict involves warring claims; it is thus not analogous to the duck-rabbit situation. Here there is evidence to be weighed and a judgment reached. Nor is such process of resolution relatively sudden or intuitive. Crises of theory are not, as Kuhn supposes, terminated by gestalt switches (p. 122). The gestalt switch is only the beginning. The new paradigm has to be formulated, argued, defended and examined by colleagues with divergent preconceptions and with access to various sorts of evidence. The idea of revolution, with its notion of prolonged struggle seems, in this respect, more apposite.

But there is, after all, a place for the vision metaphor, as applied to the understanding of a theory, that is, seeing its point. This process may indeed be relatively sudden and spontaneous. Understanding does not however, in itself, imply advocacy or commitment, nor is it excluded by rejection of the theory in question.

This is indeed, I believe, a central point of the scientific attitude. Understanding a

theory, our acceptance or denial of it is not thereby prejudged. Our advocacy or rejection itself depends upon the outcome of tests and argumentation; it is not predetermined simply by our comprehension. In sum, vision may perhaps serve as a metaphor for comprehension of a paradigm or theory, though not for its testing and acceptance or rejection. The latter involve advocacy and claims, debates and counterdebates, a period of testing and ultimate ascendancy or decline. In these respects, the revolutionary metaphor is clearly more fitting.

But revolutionary debate is not necessarily scientific debate. Advocacy in itself is not necessarily fair, logical or responsive to competent argument or relevant evidence. In these respects, science can be compared only in very limited ways to revolution. The quality of scientific deliberations makes for a special and rare form of argumentation, it demands responsibility to evidence, openness to argument, commitment to publication, loyalty to logic, and an admission in principle, that one may turn out to be wrong. These special features go far beyond the gestalt switch analogy and they outstrip the revolution metaphor as well. An understanding of science requires appreciation of these special features, a recognition that science itself marks a revolution in the quality of human thought.

Notes

1. Israel Scheffler, *Science and Subjectivity* (Indianapolis, Hackett, 1967; Second edition, 1982). References to this book in the text will uniformly be to the second edition.
2. Thomas S. Kuhn, *The Structure of Scientific Revolutions* (Chicago, The University of Chicago Press, 1962; second edition, 1970). Page references in parentheses following citations in the text will uniformly be to the second edition.
3. Steven Weinberg, "Night Thoughts of a Quantum Physicist," in Bulletin of *The American Academy of Arts, and Sciences* Vol. 49, No. 3 (December, 1995), p. 61.
4. Margaret Masterman, The Nature of a Paradigm," in I. Lakatos and A. Musgrave, eds., *Criticism and the Growth of Knowledge* (Cambridge, Cambridge University Press, 1970), pp. 61-66.
5. Stephen Toulmin, "Does the Distinction between Normal and Revolutionary Science Hold Water?" in Lakatos and Musgrave, *op. cit*, pp. 39-47.
6. Leonard K. Nash, *The Nature of the Natural Sciences* (Boston, Little Brown and Company, 1963), pp. 284-296.
7. W. V. Quine, *Word and Object* (New York, John Wiley, 1960), p. 270.
8. *Op. cit.*
9. *Op. cit.*
10. Lakatos and Musgrave, *op. cit.*, p. 261.
11. Ibid., p, 262.