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KUHN'S PHILOSOPHY OF SCIENTIFIC PRACTICE[1]

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The opening sentence of The Structure of Scientific Revolutions is often thought to be prophetic. Kuhn proclaimed that "history of science ... could produce a decisive transformation in the image of science by which we are now possessed" (1970, p. 1). In the decade or so after the book was published in 1962, the dominant philosophical conception of science, logical empiricism, was indeed substantially transformed. Moreover, although Kuhn's book at the time was only one among a half dozen prominent challenges to logical empiricism, it has in retrospect become the symbol for its own revolution, marking a transition to a post-empiricist era in the philosophy of science.[2] Citations to Kuhn are now ubiquitous in various contrasts between the supposed bad old days, and some more enlightened present conception of science.

Proclamations of revolution are often succeeded by revisionist debunking. That fate may well befall Kuhn's book. In the past decade or so, a number of scholars have convincingly called attention to important continuities between Kuhn's book and the work of his logical empiricist predecessors.[3] Others note that Kuhn and his most sympathetic readers have repudiated the most radical-sounding claims associated with the book.[4] In a still different vein, one scholar has argued that Kuhn's book was reactionary rather than revolutionary: Fuller (1999) claims both that Kuhn aimed to insulate science from public scrutiny and democratic control, and that contrary to its public image, the philosophical and social scientific work most influenced by Kuhn has had just that effect.

In what follows, I propose a different revisionist response to the opening proclamation of Kuhn's book. I shall argue that there was indeed implicit in Kuhn's book a potentially revolutionary transformation in the predominant conception of science. This revolution has not (yet) occurred, however. Philosophers and other theorists of science have not yet grasped, let alone achieved, a Kuhnian transformation in their conception of science. To say this is not to deny that important conceptual changes occurred in the wake of Kuhn's book, or that these actual developments in the philosophical understanding of science were important or illuminating. Rather, I would argue that these philosophical developments reflect attempts to accommodate Kuhn's claims and arguments within familiar conceptions of the philosophy of science. On my reading, however, The Structure of Scientific Revolutions is best understood as challenging the conceptual frame within which the book itself has been influentially read and interpreted. Thus, a truly decisive Kuhnian transformation in the current image of science will also transform the most familiar accounts of that book.

The pivotal question in my rereading of Kuhn concerns the subject matter of the philosophy of science. Traditionally, philosophy of science has been conceived epistemologically. Its subject matter is scientific knowledge, and the relevant philosophical questions concern the aim, structure, sources, methods, and justification of scientific knowledge. In his opening chapter Kuhn derided the conception of scientific knowledge as the subject matter of philosophical reflection as one derived from the presentation of science in pedagogical textbooks. An "image [of science] drawn mainly from the study of finished scientific achievements ... is no more likely to fit the enterprise that produced them than an image of a national culture drawn from a tourist brochure or a language text." Most readers of Kuhn have taken this claim to imply that we need a different conception of scientific knowledge. I take Kuhn to have proposed a more fundamental challenge to a textbook-driven image of science, aiming for "the quite different concept of science that can emerge from the historical record of

the research activity itself."[5] Kuhn then went on to say that this aim requires different questions to be asked about science and its history, and not merely different answers to the familiar questions that arise from the textbook image of science.

The alternative I propose is that Kuhn articulated a philosophical conception of science as "the research activity itself," or in the terms I prefer, of science as a practice. Kuhn himself was not always fully clear in articulating this distinction between epistemological and practical conceptions of science. Thus, readers who have understood Kuhn as offering novel answers to old questions have not altogether misunderstood him. He did not always fully grasp just how deeply his approach challenged familiar views, and there are coherent readings of the book that assimilate him to the tradition. But Kuhn was also well aware that the traditional conceptions and the questions they generate did not serve him well in articulating the alternative understanding of science for which he aimed. His misgivings about the adequacy of the very terms available for framing his discussion sometimes came out quite explicitly:

> In the absence of a developed alternative [to the epistemological viewpoint that has guided Western philosophy for three centuries], I find it impossible to relinquish entirely that viewpoint. Yet it no longer functions effectively.... (Kuhn 1970, p. 126)

Even this claim was ambiguous; it could be read as calling for a different epistemological viewpoint, rather than for a viewpoint on science that is no longer primarily epistemological. The best argument for the latter reading of Kuhn's misgivings is to show how his own book began to articulate such a more far-reaching alternative, and that is what I shall do here. This alternative is best recognized, however, by contrast to the more familiar epistemological interpretations. Hence, I shall begin with my best reconstruction of how Kuhn has been assimilated within a more traditional conception of science, and use that as a foil for my own preferred interpretation of the book.

The Familiar Kuhn

This section develops an interpretation of The Structure of Scientific Revolutions which I do not fully endorse. I nevertheless present it in my own voice, without further qualification. This presentation is not merely for rhetorical convenience. This reading of Kuhn offers a defensible interpretation of the book, and also presents a thoughtful and informative conception of science. Some aspects of it are surely worth retaining. I reject it not for any obvious inadequacy of its own, but for an alternative that offers a better reading of Kuhn in large part because it promises a better understanding of science.

Kuhn began by discussing "normal science," in which scientists forgo any dispute about the most fundamental concepts and theories in their discipline in order to extend and refine them. These concepts and theories function together as components of a paradigm, a set of theoretical commitments that had either originally established or subsequently reconstituted a whole field of inquiry. Challenges to the paradigm are rejected as a distraction from scientists' primary task of describing the world in these accepted terms. A paradigm offers a comprehensive worldview for those who accept it, and such worldviews serve crucial functions for normal scientific inquiry. Paradigms prescribe some core beliefs as essential to work in this field, and proscribe others as unacceptable. They determine which facts would be important to know, and what instrumental, methodological, and theoretical tools are worth acquiring. Paradigms also strongly suggest how to proceed with these tasks, but these suggestions require considerable creativity, ingenuity, and

hard work to carry out successfully. They constitute the puzzles whose solutions are the primary aim of normal scientific research.

The analogy between scientific research and puzzle solving is illuminating for Kuhn in multiple respects. Like jigsaw or crossword puzzles, scientific puzzles focus attention upon reasonably well-defined gaps or deficiencies within a comprehensive scheme. At the outset, the overall scheme may be only sketchily articulated, but as work proceeds successfully, other puzzles become more sharply and accessibly characterized. Yet such puzzle-solving can only be intelligibly undertaken through an unquestioned acceptance of the overall scheme. If one were to doubt the core commitments that define puzzles, or otherwise question the existence of unique, accessible solutions to them, the characteristically focused, dogged efforts of puzzle-solvers would be misplaced. Failures are taken to mark inadequate effort or ingenuity by the puzzle-solvers rather than erroneous features of the paradigm. Paradigms provide both normative and heuristic guides to scientific puzzle solving. Normatively, they indicate which puzzles are worth solving, what is the point of their solution, and thus what standards govern the acceptability of proposed solutions. Heuristically, they offer model problem solutions ("exemplars") that provide analogical guidance for how to extend past successes to new situations.

Kuhn also made constructive use of what might initially seem to be a disanalogy between puzzle-solving and science. Crossword or jigsaw puzzle-solving may seem trivial and self-absorbed by comparison to science, for they lack the intellectual and practical significance usually accorded to scientific achievements. But Kuhn found the more transcendent goals of science to be too remote from day to day work to be motivationally significant; the challenge of solving a difficult puzzle provides a more immediately relevant goal for most scientists. More important, Kuhn used the puzzle-solving analogy to explain why scientific work is (and perhaps should be) mostly insulated from the demand to satisfy externally defined goals. He thought scientific work could satisfy practical and intellectual goals defined by socially salient concerns only to the extent that these tasks could be recast as soluble puzzles within the terms provided by available paradigms. For Kuhn, this analogy thus explained and partly justified scientists' relatively insular specialization and partial professional indifference to larger social goals.

Paradigms are thus closely linked to inward-looking scientific communities that accept and use them. Indeed, Kuhn thought that neither paradigms nor research communities could be readily identified apart from one another: paradigms are the core commitments of scientific communities, whose boundaries are defined by their shared acceptance of a paradigm. Such definitions of communities are not just retrospective abstractions; Kuhn thought that scientists whose work does not conform to paradigmatic norms are effectively excluded from the activities of a normal scientific community. Their work is not seriously read or cited, and their objections to standard approaches are marginalized. The ability of normal science to proceed without extensive controversy over fundamental commitments is thus actively sustained through the exclusion of such dissent, rather than being dependent upon the absence of challenges. For Kuhn, such exclusions are constructive. They enable more focused inquiry using complex and sophisticated apparatus to investigate esoteric phenomena, as well as more effective communication through a specialized professional literature. Scientists can get on with such work by avoiding more far-reaching theoretical or evaluative disputes.

All active paradigms confront anomalies (apparent counterinstances or unexpected

failures) at all times. Paradigms are usually first accepted more on the basis of their future promise than of their inevitably limited initial successes. Indeed, their achievements can only be extended further by the more widespread efforts that result from their initial acceptance by a community of researchers. Such successes then introduce new, more esoteric and refined anomalies, since paradigmatic expectations can be more extensively and sharply defined, and more precisely assessed. This recurrent generation of new puzzles sustains the life of a research community. A paradigm without discrepancies or gaps would leave no role for further research. Recognizable anomalies are thus normally divided into accessible puzzles that should be solvable with sufficient ingenuity, and not yet accessible difficulties that can be bypassed for the time being. Yet sometimes, Kuhn thought, the persistence, proliferation, centrality, or recalcitrance of some anomalies can erode a community's commitment to a paradigm. The typical result is not outright rejection of the paradigm, but a willingness to loosen some of its standards or modify one or more of its marginal commmitments. Alas, such tinkering may further erode the community's confidence, since it can undermine the ability to work from common assumptions. Not everyone will make the same modifications, or agree about which if any modifications are sufficiently marginal. It then becomes less clear what beliefs can be taken for granted, what work is important, and when a puzzle has or has been been genuinely solved. Crisis thus to some extent dissolves the community itself, by breaking down what it had in common scientifically.

It is, however, revolution rather than crisis which provides the most philosophically striking features of Kuhn's account. One possible response to crisis is to propose a more fundamental violation of a paradigm's constitutive commitments (or to begin to take seriously a previously ignored proposal) in order to resolve its outstanding difficulties. Such proposals may split what remains of a community-in-crisis. The disagreements between proponents of such a proposal and the defenders of paradigmatic orthodoxy are not readily resolveable. Paradigms incorporate the values, standards, methods, and relevant factual background which govern the resolution of scientific disputes, but these features of a paradigm are precisely what are now in dispute. There is hence no commonly accepted basis for assessing the alternative positions; the new proposal is "not merely incompatible, but actually incommensurable with what has gone before" (Kuhn 1970, p. 103).

Kuhn noted various possible manifestations of incommensurability between competing paradigms. Their proponents may employ different vocabularies, or worse, the same vocabulary with different meanings or referents. They can each appeal to empirical evidence, but they may not take the same evidence to be important, and may even see and/or describe it differently. What one proposes as a solution to an outstanding puzzle may seem to another simply to redescribe the problem, or to assume what needs to be demonstrated. They may even disagree about what problems one really needs to solve, or at least about which ones are sufficiently important that their solution could settle the dispute. As a result, these proponents may "talk through one another," failing to grasp their opponents' arguments or even perhaps their conclusions. Without common standards or procedures, the reasons offered for each choice can at best be persuasive, and not rationally conclusive. Kuhn sometimes even likened the acceptance of a new paradigm to a religious conversion or a Gestalt switch, which in different ways exemplify sudden, unreasoned changes of belief and perception. The more troubling comparison, however, is to coercion. Since reasons for choosing one paradigm cannot be

conclusive, the outcome may be determined by whose proponents are sufficiently numerous or influential to be able to close ranks by ignoring and excluding their opponents. A new normal scientific community thereby emerges, with a new paradigm for its research.

Scientific revolutions thus complete a recurrent cycle from normal science, to crisis, to a revolutionary reconstitution of normal science under a new paradigm. In retrospect, and from within, this cycle inevitably appears progressive. The revolution's victorious faction can claim to have resolved the fundamental anomalies of the old paradigm, and to have renewed the prospects for successful research governed by shared assumptions. Indeed, the new community typically rewrites the textbooks, and retells its own history, to reflect this point of view. But from the standpoint of the losers, or even of those who look on impartially, such rewritings might seem to mark change without any genuine claim to progress, because there is no neutral standard from which to assess the merits of the change. The resulting body of knowledge is in any case not cumulative, since much of what was previously known (or merely believed) had to be excluded, without its ever having been conclusively refuted. One likewise cannot plausibly talk about revolutionary reconstitutions of science as aiming toward truth, for similarly, there can be no impartial formulation of standards for its assessment. The available justification of scientific knowledge after revolutions, couched in new terms according to newly instituted standards, may well be sufficient, but perhaps only because these standards and terms are now perforce our own.

The Kuhnian Revolution Yet to Come

Despite the familiarity and influence of the interpretation of Kuhn in the previous section, I believe there is a better way to understand Kuhn's account of science. This reinterpretation does not fundamentally reorganize the book, but it does revise many of its familiar concepts. The crucial underlying shift is toward a description of science as an activity, rather than of knowledge as a product derived from that activity. Thus, normal science is research in which scientists know their way around. Professional training and research experience give scientists a reliable sense of what they are dealing with, what can affect its relevant behavior, how it makes itself known, and what they can do with it. These abilities are held together by their practical grasp of one or more paradigms, concrete scientific achievements that point toward an open-ended domain of possible research. Paradigms should not be understood as beliefs (even tacit beliefs) agreed upon by community members, but are instead exemplary ways of conceptualizing and intervening in particular situations. Accepting a paradigm is more like acquiring and using a set of skills than it is like understanding and believing a statement.

Among the skills that might constitute the grasp of a paradigm are the appropriate application of concepts to specific situations; the deployment of mathematical tools (not just solving equations, but choosing the right ones, applying them correctly to the situation at hand, knowing their limitations and the ways those limitations can be circumvented, etc.); the use of instrumentation and experimental techniques and procedures; and the recognition of significant opportunities to extend these skills in illuminating ways to new situations. The reasoning most often involved in such work is analogical rather than deduction from general principles. Scientists must understand how to handle novel situations in ways modeled upon familiar treatments. General principles may well be invoked, but typically the analogical extension from one application to another explicates the principles, rather than depending upon a <u>prior</u> grasp of the principles to understand their application. To put the same point differently, general

principles are useful as relatively compact expressions, but the understanding they express is embedded within the disaggregated ability to grasp various situations in those terms. A parallel point in science education is quite familiar to science students: typically, one first comes to understand a chapter in a science textbook only by learning to solve the problems at the end, rather than learning how to solve the problems by first understanding the chapter.

Scientists use paradigms rather than believing them. The use of a paradigm in research typically addresses related problems by employing shared concepts, symbolic expressions, experimental and mathematical tools and procedures, and even some of the same theoretical statements. Scientists need only understand how to use these various elements in ways that others would accept. These elements of shared practice thus need not presuppose any comparable unity in scientists' beliefs about what they are doing when they use them. Indeed, one role of a paradigm is to enable scientists to work successfully without having to articulate a detailed account of what they are doing, or what they believe about it. Kuhn noted that scientists "can agree in their identification of a paradigm without agreeing on, or even attempting to produce, a full interpretation or rationalization of it. Lack of a standard interpretation or of an agreed reduction to rules will not prevent a paradigm from guiding research" (1970, p. 44). By recognizing analogies between a research project and the paradigmatic achievements that motivate it, scientists can develop their own research as well as understand the work of others, without having to spell out just how these analogies are supposed to work.

Paradigms are thus first and foremost to be understood as exemplars, "accepted examples of actual scientific practice—examples which include law, theory, application, and instrumentation together—[that] provide models from which spring particular coherent traditions of scientific research" (1970, p. 10). In working with these shared models of successful work, scientists open a field of research possibilities, a "disciplinary matrix." This matrix is the context or situation within which shared concepts, symbols, apparatus, procedures, and theoretical models are used. It articulates a domain of phenomena as a field of research possibilities, which present opportunities, challenges, and dead-ends. These opportunities and challenges are understood as the outcome of prior activities and achievements. Research science is always oriented toward the future, but it thereby continually reconstructs its past as having led toward the current matrix of possibilities.

There is room for considerable disagreement within such a research field. Even within normal science, scientists assess differently what is possible, plausible, or promising. They consequently go in different directions within the common field (even apart from the ways their research choices reflect their own distinctive strengths and limitations, and their prospects within the discipline). Yet such divergence is always held in partial check, both by the use of common resources, and by a more or less common sense of what is at issue in their field, why it matters, and what must be done to resolve these issues. The acceptance of a paradigm is thus not a matter of monolithic agreement within a community, but rather of sufficient common ground to make disagreement both intelligible and interesting. In retrospect, Kuhn even identified the concept of a paradigm with a move away from conceiving scientific communities as held together by common beliefs.

> I [once] conceived normal science as a result of a consensus among the members of a scientific community .... in order to account for the way they did research and, especially,

> for the unanimity with which they ordinarily evaluated the research done by others..... What I finally realized .... was that no consensus of quite that kind was required... If [scientists] accepted a sufficient set of standard [problem solutions], they could model their own subsequent research on them without needing to agree about which set of characteristics of these examples made them standard, justified their acceptance. (Kuhn 1977a, xviii-xix)

The result of this recognition is to think of scientific communities as composed of fellow practitioners rather than of fellow believers. Such communities do not include everyone whose training has given them a shared background understanding. Those trained in a field who do not undertake front-line research are not members of Kuhnian communities, while some scientists with different backgrounds do become acknowledged participants by undertaking the right kind of research. Kuhn has often been misread as insisting that members of scientific communities are in substantial agreement about fundamental issues in their field. What he actually says is that normal science rarely engages in controversy about such fundamentals. A lack of controversy is quite consistent with extensive disagreement, however, if research can proceed coherently and intelligibly without having to resolve the disagreements. Shared paradigms enable scientists to <u>identify</u> what has or has not already been done, and what is worth doing, without having to agree upon how to describe just what those achievements and projects are. Scientists are ignored by or excluded from a community of researchers not because they disagree with others' beliefs, but because their work does not mesh constructively with what others are <u>doing</u>. Scientists can hold heterodox beliefs about fundamental issues in their discipline, as long as their research can be taken into account and used by others. What matters is relevance and reliability: scientific communities share concepts, problems, techniques, and references, not orthodoxy.

I now turn to the analogy between normal scientific research and puzzle-solving. This analogy highlights the fact that such research usually addresses well-defined tasks with the presumption that they have a definite, attainable resolution. Such work primarily calls for ingenuity to satisfy multiple predetermined constraints, rather than unconstrained curiosity or skepticism toward received doctrines. But there is no single primary characterization of how puzzles arise. Kuhn mentions three common types of normal scientific research project. Sometimes one seeks to determine (or improve upon) facts, techniques, or procedures whose importance is highlighted by a paradigm. At other times, however, a problem can be important not for any intrinsic significance of the facts to be determined, but only because they provide an opportunity to assess paradigmatic expectations empirically. Such "tests" of the paradigm are diagnostic rather than evaluative. They show whether, where, and how a paradigm needs further articulation and refinement, rather than whether it should be accepted or rejected altogether. Finally, normal scientific research aims to develop such refinements and articulations of a paradigm. Such work includes extending paradigmatic solutions to apply to other phenomena, further developing paradigmatic theories or concepts, devising new experimental procedures or instruments, or otherwise extending the scope and power of the discipline's know-how. Philosophical readers of Kuhn often identify puzzles with anomalous facts or conceptual conflicts, but that is a misleading oversimplification. Anomalies and conflicts can only arise after considerable paradigm articulation has taken place. Philosophical preoccupation with testing and evaluating hypotheses betrays a retrospective emphasis upon the certification of

knowledge. That emphasis contrasts with the prospective orientation of scientific research toward the extension of understanding.

All paradigms confront obstacles to the development of normal scientific research. Anomalies, i.e., unexpected or unclear empirical results, are prominent among these obstacles, but it is a mistake to think of them as if they were counterinstances to a paradigm. Recognizing a counterinstance presupposes a clear understanding of what you are dealing with and its significance for one's approach to the field. Recognizing an anomaly involves the more limited awareness that something significant is not yet adequately understood or dealt with. To understand what the problem is, rather than just where it is, is to have already gone a long way toward resolving it. That is why Kuhn concluded that "assimilating a new sort of fact demands a more than additive adjustment of theory, and until that adjustment is completed ... the new fact is not quite a scientific fact at all" (1970, p. 53). Until the disturbance is more clearly characterized, one cannot know how it matters or what can be done with it, and hence, what it is. A great deal depends upon whether the disturbance can be localized within the research field. An anomaly that only shows up in a limited range of circumstances can be easily by-passed or dismissed as likely an artifact. More pervasive anomalies, or ones that affect widely used instruments or experimental procedures must instead be dealt with. Kuhn illustrates the latter point with Roentgen's recognition that a barium platinocyanide screen was glowing in the vicinity of his shielded cathode-ray tube (1970, p. 57). This unexpectedly glowing screen could not just be dismissed as a curiosity. Cathode-ray tubes were important research apparatus, and if the shielding used to prevent such effects was permeable, it meant that physicists did not really understand what was going on with their equipment. Until the nature and scope of that failure was clarified, further research with cathode ray tubes was pointless (indeed, the impending worry was that previous research was also rendered pointless, since one no longer knew what it showed).

Clarifying an anomaly is closely linked with determining how to respond to it. Sometimes a revision of theory or theoretical concepts is called for. In other cases, what is needed is a revision of experimental procedures or instruments, to bypass the difficulty without necessarily fully explaining it. This possibility highlights that anomalous phenomena are not yet in conflict with theory, but are instead practical difficulties (which, looked at from another angle, may also be opportunities to explore the world in revealing ways, as the Roentgen case illustrates). Such problems need to be resolved only to the extent that they continue to block meaningful research. If an anomaly is sufficiently obstructive, or if it offers interesting alternative possibilities for research, it can become a focus of subsequent work. More often, however, apparently anomalous facts can be construed as obstacles to be circumvented so as to get on with normal science without having to understand the anomaly more fully.

Anomalies may arise frequently, but most are resolved or circumvented quickly. Some nevertheless persist, and resist sustained efforts to accommodate them. If the significance of a persistent anomaly cannot be circumscribed (so that most scientists can ignore it), the result is disconcerting. Such persistent and pervasive anomalies suggest not merely that this particular phenomenon is not understood, but that whatever causes it, and whichever situations in which it can show up, are not reliably understood. Under these circumstances, a field may undergo a Kuhnian crisis, in which the intelligibility, reliability, and significance of its practices and

achievements comes into question. Crisis does not result merely because scientists cannot agree about what to believe about some phenomena. Scientists readily accommodate such uncertainty, because they expect it to be resolved eventually by further research. Crisis results only when scientists become unsure how to proceed—which research is worth pursuing, which background assumptions may be unreliable, which concepts and models are reliable guides to further work. Crisis is always partial, for without some sense of how to proceed, research would collapse altogether. There would be no coherent field of possibilities to explore. But crisis expands and blurs the bounds of the field, and thereby makes uncertain the significance of one's own activity. It makes sense to try more and different things, but it is less clear what sense these explorations may have.

Many incipient crises are resolved in ways that sustain the dominant research paradigm. Some crises, however, yield alternative paradigms. Such alternatives take the form of specific achievements that could provide a new focus and a different model for research. They need not involve anything like an overarching "worldview." New concepts and theories may well result from these exemplary achievements, yet even their proponents may not fully agree about how to specify them. The more basic issue between proponents of alternative paradigms concerns how to proceed with research: what experimental systems or theoretical models are worth using, what they should be used for, what other achievements must be taken into account, and what would count as a significant and reliable result. The conflict is not so much between competing beliefs as between competing forms of (scientific) life.

Such conflicts can be difficult to resolve precisely because the protagonists now work in different worlds. Kuhn's claims about changes of world are widely misunderstood and often mischaracterized, because insufficient emphasis is placed upon his reference to scientific work. Yet Kuhn was quite careful to distinguish the obvious sense in which the world does not change from changes in the world of scientists' research (1970, p. 112), the setting in which they "practice their trade" (1970, p. 150). Indeed, when Kuhn also talked about scientists seeing the world differently, this claim is often presented as a consequence of differences in their workworld and what they would characteristically do (1970, pp. 111-12, 118-122). What do I mean by a different workworld? Think about the differences expressed by phrases like "the business world" or "the academic world." Their inhabitants may well hold different beliefs, but the more important differences are in how they comport themselves, what is expected of them, and what is at issue and at stake in their dealings with things. Otherwise similar situations may then look quite different as a result of these differences in practical orientation. We should regard the differences between paradigms similarly. They reorganize the world as a field of possibilities, offering differently configured challenges and opportunities. If proponents of different paradigms do not fully communicate, it is not so much that they cannot correctly construe one another's sentences or follow one another's arguments. The problem is more that they cannot grasp the point of what the others are doing, or recognize the force of their arguments.

Kuhn's distinctions between normal science, crisis, and revolution are often misconstrued as a rigid periodization of the development of scientific disciplines. Normal science and crisis are instead ways of doing science. One or the other may typically predominate within a field at any given time, but they can also coexist. Some scientists may begin to articulate their

fundamental assumptions explicitly, and tinker with the ones that seem less essential, in response to problems that do not greatly disturb their colleagues. Others may blithely go on with familiar ways of setting and solving puzzles, even though their colleagues are no longer sure what to make of the results. In retrospect, historical judgment may discern sharp breaks and crucial turning points, but these almost inevitably blur when looked at closely, or without hindsight.

Revolution is likewise a matter of retrospective interpretation. Whether a new development amounts to a revolution rather than an articulation of a prevailing paradigm depends upon how one interprets that paradigm; some interpretations would make the shift more dramatic than others. That is why Einstein could say that special relativity only worked out the implications of Maxwell's electrodynamics, whereas most commentators regard it as a revolutionary reconstruction of classical physics. Einstein and his contemporaries all started out from Maxwell's theory, but Einstein's reading of Maxwell pointed toward rather different ways of dealing with electromagnetic phenomena. Such possible ambiguities between normal paradigm articulation and revolutionary shifts are heightened by the retention of many familiar experimental arrangements, procedures, calculations, and other practices across scientific revolutions. Many of the same procedures are employed in different contexts, but to somewhat different ends. The interpretive question is always whether to emphasize the continuities or the discontinuities, and that in turn is affected by where one foresees the next step to be.

Philosophical readers of Kuhn have tended to identify paradigm change first and foremost with theoretical change, despite the prominent example of Roentgen's discovery of X-rays, which revolutionized cathode-ray research without requiring fundamental changes in the underlying theory (Maxwell's). Such examples of instrumental and experimental revolutions abound, however. William Bechtel (1993) and Hans-Jorg Rheinberger (1997) have persuasively centered the shift in the 1940's and 1950's from classical cytology to modern cell biology upon the introduction of ultracentrifuges and electron microscopes. These instruments enabled biologists to ask fundamentally different questions about cells, moving from a structural taxonomy of cell components toward a functional dynamics of intracellular processes. More recent examples of comparable shifts in research practice and goals can be found in the successive developments of recombinant DNA technologies, the polymerase chain reaction (PCR), and gene-activation arrays.[6] These were technical rather than conceptual achievements, but they have dramatically changed how biologists approach their work, and to what ends.

Even revolutions that do initiate major theoretical change may only do so in concert with shifts in instrumentation and research practice. High-energy physics underwent a widely recognized revolution in the early 1970's. This shift has often been characterized in terms of the adoption of gauge field theories, and the conception of particles such as protons and neutrons as composed of charmed quarks. But gauge theories go back to 1954, and the first quark models were proposed in 1964. What better coincides with the acknowledged revolution are the shifts in experimental practice and its associated theoretical modeling which used beams of different kinds of particles (leptons like electrons, positrons and neutrinos, often colliding two beams head-on, rather than single beams of hadrons like protons and neutrons), or looked at different aspects of familiar particle beams (the relatively rare "hard" or sharp-angled scattering of hadrons, rather than the more common "soft" scattering in the vicinity of the beam).[7] Different phenomena were seen to be scientifically illuminating, and research practice was reorganized

accordingly.

Whether initiated theoretically or experimentally, scientific revolutions mark progress in research. The most basic problem posed by Kuhnian crises is not inconsistent belief but incoherent practice. Revolutions succeed by giving renewed impetus to research. That is why the sciences are not normally impeded by incommensurable theoretical disputes. The disclosure of new ways to explore the world in revealing ways usually overrides contrary theoretical convictions. Perhaps with ingenuity one could reconstruct a discarded paradigm such as phlogiston chemistry into a coherent system of beliefs and values, with its own reinterpreted body of supporting evidence. What one cannot so readily imagine is the reconstitution of phlogiston chemistry as a viable program of ongoing research. The insuperable objection to phlogiston theory was not its inconsistency or empirical falsification, but its inability to guide further inquiry into the new "airs" (gases) discovered in pneumatic chemistry. Scientific progress across revolutions is progress away from the impasse that initiated a crisis in normal scientific research.

Kuhn's conception of science as research practice offers a revealing insight into recent attempts to claim scientific credence for divine creation of species. Proponents of "creation science" have sought to place their views on a par with neo-Darwinist evolutionary theory by arguing that both are unproven theories. Even if such arguments were tenable, from Kuhn's perspective the wrong issue has been joined. Scientists' primary concern is not whether present beliefs are likely to be true, but instead whether available models of inquiry can effectively guide further research. If creationists claimed to offer not merely an internally consistent set of beliefs but an ongoing research program that promises to advance beyond its current understanding, only then would they have begun to contest evolutionary biology on its own terrain. The epistemological orientation of "textbook" views of science has mistakenly encouraged a conception of science that highlights the retrospective justification of belief.[8] Kuhn emphasizes instead the futural orientation of scientific understanding. In that light, evolutionary biology is so central to modern biology not because its current formulations are likely to be correct, but because it provides the best available understanding of how to explore a wide range of biological phenomena. Science does also look back, not primarily to vindicate beliefs, but to better secure its orientation toward future disclosure.

Conclusion

What difference would it make to read Kuhn in this way as a philosopher of scientific practice? I have been arguing that Kuhn reorients the philosophy of science toward an account of scientific practices rather than scientific knowledge. This shift does not diminish the intellectual or cognitive significance of science, but only reinterprets it. Kuhn encourages us to think about scientific <u>understanding</u> rather than scientific knowledge. Science aims not so much to produce justified beliefs as to transform human capacities to cope with the world practically and discursively. Biologists understand cells in the sense in which we say that a good mechanic understands cars. Biologists and mechanics can, if asked, produce many true sentences about what they work on, but that is hardly the point in either case.

That is why I have emphasized paradigms as achievements that one understands by <u>using</u> them as models for subsequent work. Scientific understanding is more a practical capacity to cope with an open-ended variety of relevant situations than it is the acceptance of purported

truths. Indeed, Kuhn's view encourages us to think of verbal articulation as an ongoing activity rather than as an artifact of that activity. Moreover, Kuhn denied that concepts and theories could be understood in abstraction from the practical contexts in which they are articulated, including the relevant instrumentation and material practices. The sciences are not just a network of statements, which coincidentally have been applied in the laboratory. Concepts and their material realization go hand in hand. Kuhn thus insisted that,

> An acquaintance with the tables [comparing theory and experiment] is part of an acquaintance with the theory itself. Without the tables, the theory would be essentially incomplete. (Kuhn 1977b, 185-86)

To understand a theoretical claim is to understand how it is to be used in various situations, including the limitations on that use.

Such limitations are not simply accepted as given, however. A distinctive feature of modern science has been its relentlessly reflexive application: scientific understanding is often directed toward research that aims to extend and enhance scientific understanding. That reflexivity helps explain both the phenomenal growth of scientific research, and the esoteric and apparently insular character of much scientific work.[9] Kuhn placed that reflexivity at the core of his conception of science; normal scientific research involves the further articulation of its own paradigms, while revolutions are the reconstitution of such research in response to its occasional breakdown. Thus, familiar readings of Kuhn that emphasize occasional revolutionary changes in scientific theory understate his point. Even normal science is oriented toward its own ongoing transformation, although its practitioners sometimes anticipate more continuity within that transformation than actually results.

Kuhn is often read as having challenged the "rationality" of science, and as lending comfort to skepticism or relativism about scientific knowledge. Such readings are imposed upon Structure of Scientific Revolutions, however. Kuhn did not even ask such questions about the wholesale justification of scientific knowledge, let alone answer them. We can now see better why these questions did not arise for him. Such questions presuppose a retrospective, epistemological orientation, which stands back from scientific work to ask whether its achievements really are genuine. Kuhn adopted the implicit standpoint of scientific practitioners, rather than that of philosophical spectator. Their questions concern which projects to pursue and what concepts, theories, and instruments to use, and these questions can only be formulated against the background of an extensive practical understanding of what one is dealing with, how it might function or break down, and what is at stake in its success or failure. To have doubts about the whole of one's grasp of the field is to doubt not just one's answers, but one's ability to ask intelligible questions or try to answer them.

Kuhn is more plausibly read as a critic of scientific realism, i.e., as one who denies that science aims to provide a correct representation of a world independent of human concepts and practices. But anti-realists often hold their view because they regard truth and reality as inaccessible, forever obscured by the effects of human language, culture, and perception. Such views are often ascribed to Kuhn, but they apply only if one reads him in the more familiar epistemological way. When Kuhn asks whether we should "imagine that there is some full, objective, true account of nature and that the proper measure of scientific achievement is the extent to which it brings us closer to that ultimate goal" (1970, 171), his objection is to the

finality and completeness of this conception, not to its putative independence. He does indeed argue that scientific understanding engages the thoroughly human workworld of research practice rather than nature-in-itself, but that does not mean that paradigmatic concepts and perceptual gestalts intervene between us and the world. The intelligibility of scientific concepts and ways of seeing are as much dependent upon ongoing interaction with the world as vice versa. The realist can posit a world "beyond" language and culture only by mistakenly thinking we can have a definite language and culture distinct from how we engage the world.

Does Kuhn's thorough identification with a practitioner's standpoint then indicate an uncritical complacency on Kuhn's part, an uncritical commitment to the project of modern science? Perhaps. But that would be so only if it were possible for us in practice to disengage sufficiently from scientific understanding of the world in which we live and work so as to put it in question as a whole. If the practices, achievements, and norms of science are sufficiently integral to the workworld of everyone in modern scientific cultures, then critical questions about science might now only be intelligible from within a broadly scientific culture.

The difficulty of such wholesale detachment might well be reinforced if we take seriously the implications of Kuhn's concept of a paradigm. If paradigms are mistakenly identified with theoretical representations, worldviews, or "conceptual schemes," epistemological detachment and skeptical questions might seem perfectly plausible. But when we consider the extent to which materials, practices, and concepts from the sciences have been built into our everyday world, then wholesale detachment from scientific understanding might undermine the intelligibility of philosophical questioning rather than the justification of our answers.

That does not mean that Kuhn's philosophical perspective cannot accommodate far-reaching critical attitudes toward the sciences and their pervasive role in our world. Here are some examples. One might argue that the sciences have placed too much emphasis upon the artificially controlled and simplified circumstances achievable in laboratories, and given inadequate attention to the more complex, messy, and uncontrolled aspects of the world outside. One might argue instead that the reflexive extension of scientific paradigms has increased both the costs and the benefits of scientific research in ways that have unjustly and dangerously reinforced intranational and international divisions of wealth and opportunity. Or one might argue that the extraordinary enhancement of those intellectual and practical capacities that can sustain traditions of normal-scientific puzzle-solving has led to the neglect of other human capacities in ways that leave many people's lives morally or spiritually impoverished. What is common to these, and many other critical perspectives one might take toward particular scientific practices and achievements, is that formulating the criticism and any adequate assessment of or response to it will extensively utilize the very scientific understanding whose alleged consequences are at issue. My point is not to endorse any of these hastily sketched critical concerns. Rather it is to emphasize that a Kuhnian shift in philosophical focus from scientific knowledge to scientific practice might transform not just what we think science is, but how we think philosophically about science. Epistemological conceptions of science have led to debates that are largely disconnected from issues within particular sciences, and from the larger contexts in which science matters to us as human beings or as scientists. To understand sciences as practices might help reorient philosophical discussion toward questions of how science matters, and what kinds of science we ought to do.

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NOTES

1. Rouse (1987, ch. 2; 1998) provides earlier articulations of many of the claims and arguments developed in this paper, aimed at different audiences.

2. Other roughly contemporary challenges to logical empiricism included Hanson 1958, Polanyi 1958, Toulmin 1962, Feyerabend 1962, and Popper 1957 (the first English translation of a then-forgotten work written in German in the 1930's). In the background were three extraordinarily influential challenges to the broader philosophical conception underlying logical empiricism: Quine 1953, Wittgenstein 1953, and Sellars 1963 (first published in 1956).

3. Earman (1993) explicitly emphasizes the similarities now apparent between Kuhn and Carnap. Other work on logical empiricism that might encourage greater rapprochement between Kuhn and his predecessors is collected in Giere and Richardson 1996 and Friedman 1999.

4. Kuhn 1977c, 2000; Musgrave 1980; Hacking 1984.

5. All citations are from Kuhn 1970, p. 1; emphasis is mine, however.

6. For a book-length argument that multiple fields of biology have been more fundamentally reorganized by recombinant DNA technology rather than by any theoretical reconceptualization, see the widely used textbook, Molecular Cell Biology (Darnell, Lodish and Baltimore, 1990); for the impact of PCR, see Rabinow 1996; gene-activation arrays are too recent an innovation to have been chronicled in this way.

7. For accessible accounts of these transformations in high-energy physics, see Crease and Mann (1986) and Pickering (1984).

8. Ironically, given Kuhn's challenge to the "textbook view" of science, the most prominent controversies over the status of "creation science" have concerned its presence or absence from science textbooks and curricula. Despite their almost uniform hostility to creation science, the dominant philosophical models of science have encouraged creationists' underlying fideistic conception of science education as a matter of imparting beliefs (or "information"). Taking more seriously the conception of science as oriented toward future disclosure rather than retrospective justification could constructively reorient science education.

9. The reflexivity of research does not by itself explain why modern societies have provided the resources necessary to sustain the phenomenal growth of science. Nor does it explain the differences between successful and unsuccessful attempts to extend scientific understanding. It does, however, show how the aspiration to such growth has been built into much of scientific practice.