The Kuhnian Image of Science

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Time for a Decisive Transformation?

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Chapter 6

Kuhn, Pedagogy, and Practice: A Local Reading of *Structure*[1]

Lydia Patton

1. INTRODUCTION

Moti Mizrahi has argued that Thomas Kuhn does not have a good argument for the incommensurability of successive scientific paradigms. With Rouse, Andersen, and others, I defend a view on which Kuhn primarily was trying to explain scientific practice in *Structure*. Kuhn, like Hilary Putnam, incorporated sociological and psychological methods into his history of science. On Kuhn's account, the education and initiation of scientists into a research tradition is a key element in scientific training and in his explanation of incommensurability between research paradigms. The first part of this paper will explain and defend my reading of Kuhn. The second part will probe the extent to which Kuhn's account can be supported, and the extent to which it rests on shaky premises. That investigation will center on Moti Mizrahi's project, which aims to transform the Kuhnian account of science and of its history. While I do defend a modified kind of incommensurability, I agree that the strongest version of Kuhn's account is steadfastly local and focused on the practice of science.

2. IMAGES OF SCIENCE

Science may move through time by gathering results and facts, and developing increasingly sophisticated methods for dealing with them. Scientists may become better over time at describing, understanding, and explaining the phenomena they encounter. Those phenomena are real and publicly available, and successive scientific theories hone in on increasingly accurate analyses and predictions of their properties and behavior.

Thomas Kuhn's *The Structure of Scientific Revolutions* unsettles this image of science. Kuhn begins by asserting that the behavior of *historians* of science cannot be explained on a cumulative picture of science. Historians may attempt to reconstruct science's past on the assumption that science is a cumulative, continuous practice. But that assumption quickly becomes a hypothesis, which is falsified as the historians dig deeper.

Kuhn cites comprehensive historical studies of research traditions in optics, electromagnetism, and related fields, by Alexandre Koyré and numerous others, which reveal breaks and discontinuities in the description of scientific practice revealed by its history.[2] Those discontinuities are found in the behavior and practice of the scientists who were working at the time. Kuhn thus defends two hypotheses: the first about scientific practice, and the second about the behavior of historians of that practice. In both cases, I will argue, his aim is to describe, and then to explain, human behavior.

Kuhn constructs a framework with which to explain scientific practice in the past, intended to extend to scientific practice generally. According to that framework, most science is essentially conservative, in the sense that it preserves the achievements that are taken as models for scientific work. Scientists are trained in a way of approaching problems and puzzles that ossifies great scientific achievements, turning them into the skeleton of a research tradition. Flesh is put on the bones by laboring researchers.

Scientists in the wake of a great scientific achievement are trained to rebuild the skeleton of that achievement, before they begin to fill out that skeleton as mature researchers. Examples of scientific achievements on the scale Kuhn analyzes are Lavoisier's *Chemistry*, Newton's *Principia*, and Franklin's *Electricity*. Thousands of scientists of the past have been trained to reproduce the achievements of these books, so that the strategies found therein become working scientific instruments.

According to Kuhn's original definition in the second chapter of *Structure*, a paradigm is a scientific achievement that becomes a textbook.[3] Books like *Principia*, *Chemistry*, and *Electricity* lay down firm results, but are also open-ended, so that scientists can find intriguing problems to solve using those results and achievements as a springboard. A young scientist doesn't just learn established theory by being taught from Newton's *Principia*. That scientist learns how to become an active researcher, how to approach problems, how to think about and represent the phenomena under investigation, and how to use instruments to conduct experiments.

What many researchers miss in Kuhn's definition are the link to *scientific practice* and the link to *teaching science*.[4] A "paradigm" often is treated as if it is identical to a "background theory," and then paradigms are fed into the machine of confirmation, testing, and assessment of theories. But Kuhn wanted paradigms to be about future research, and to be linked to scientific practice.[5] When a paradigm guides scientific practice in the right way, it makes scientific research and progress possible, and it goes beyond accepted theory.

Scientists who learn how to use a scientific achievement as a playbook are doing "normal science." Kuhn's description of normal science as conservative and dogmatic is notorious.[6] Critics complained right away that normal science only extends a paradigm that is taken as given, and does not subject that paradigm to rigorous testing. On Popper's view, for instance, if a research practice does not include at least the possibility of falsifying the theory in the background or the hypotheses under investigation, it is not scientific.

However, paradigms, in the 1962 *Structure*, are neither theories nor hypotheses. They are model scientific achievements. Strictly speaking, a theory cannot be a paradigm in the sense discussed earlier. Theories can be tested precisely because they contain sets of assertions that have fixed, knowable truth-values. In this sense, theories are closed. If an assertion of a theory does not have a truth-value that can be assessed in principle, or if that assertion cannot be proven, the assertion is not a result of the theory. Thus, if Newton's *Principia* contained only a theory or theories, it could not be a paradigm. *Principia* is a paradigm because, along with the stable results it states, the work implicitly expresses a new way of doing science. Newton's achievement results from a novel orientation to scientific practice, an orientation that can be learned and can be the source of a tradition of research.

In 1959, Kuhn described the interplay between tradition and innovation as the "essential tension" without which science cannot operate. The essential tension is the source of the theoretical posit at issue in a recent exchange between Moti Mizrahi (2015a, 2015b, 2015c), James Marcum (2015a), Vasso Kindi (2015), and myself (Patton 2015b): the incommensurability of successive paradigms.

3. INCOMMENSURABILITY

On Kuhn's account, the only reason for a working scientist to question a paradigm comes when there is a persistent anomaly: when experiments set up using the paradigm begin to fail, when the paradigm fails to solve new problems, and so on. The first task of a working scientist is to resolve anomalies within the paradigm. If that fails, then the scientific community must—reluctantly—find a way to conceive of a new paradigm to resolve persistent anomalies.

One puzzling feature of Kuhn's "essential tension" is the dual role of the architects of a paradigm. On Kuhn's account, paradigm architects are treated with reverence by the generations who follow them. The main lines of their research programs are traced and retraced by generations of scientists. We might think of graduate students learning to reproduce Fourier transforms, or learning to use Hamiltonians in simplified cases, as monks copying texts in a scriptorium. Even more profoundly, scientists being trained in a paradigm learn how to *think about* and how to *conceive of* scientific phenomena through this training, including how to describe target phenomena so they are tractable by scientific methods (see Andersen 2000).

On the other hand, it is only a matter of time before any given architect of a paradigm loses his or her place in the pantheon. No program of normal science is immune to persistent anomaly. It is an axiom of Kuhn's account[7] that no paradigm can deal with all the phenomena. The works of Aristotle were treated by the scholastics with reverence. But once prominent cases were made that the effectiveness of the Aristotelian paradigm was limited, Aristotle had to be displaced in a revolution. Kuhn's account turns Aristotle, Newton, and Einstein into warring Greek gods.

Kuhn's picture of the history of science replaces serene continuity with political upheaval. A paradigm is constructed only when persistent anomaly dogs the old paradigm: when there is a recognized crisis. The only way to deal with crisis is to change the fundamental approach to the problem. Another approach must be achieved that changes how scientists interact with the phenomena in practice.

Scientific revolutions are best explained as practical decisions, from either inside or outside an existing paradigm (Patton 2015b). Scientists will change their paradigm only when forced to do so, according to Kuhn. When they do, it is because the existing paradigm no longer works. Scientists have been trained to approach the phenomena in a certain structured and artificial way, a way not limited to accepting the claims of the background theory. The approach may involve being trained into practical ways of modeling the phenomena and ways of setting up experiments, for instance, practical know-how that Michael Polanyi calls "tacit knowledge."[8]

Scientists are trained as well in what Ludwik Fleck calls "vademecum science" and Kuhn calls "textbook science," in contrast with "journal science."[9] Textbook science is the image of science sketched earlier in the text, in which students are initiated into their scientific community's way of approaching the phenomena and of solving problems. As Hoyningen-Huene puts it, this training even gives students "access to the (region of the) phenomenal world relevant to the work of his or her community."[10]

Once students have been trained, they are set loose on the world as researchers, and, again, the phenomena are not always tractable by a given paradigm. Researchers will find anomalies and misfits in practice, most of which can be resolved. But if a crisis occurs, and then a revolution, then there must be a "paradigm shift." Paradigm shifts, for Kuhn, involve changes to the conceptual and semantic categories in play. A new paradigm may even mean that scientists educated in the former tradition may need to "reeducate" themselves to perceive the world differently (Kuhn 2012/1962, 112).[11]

The earlier Kuhn of *Structure* presented incommensurability as a kind of practical impossibility. Imagine a scientist, Alessandra, has been trained in the fluid theory of electricity.[12] Alessandra knows how to work with an early kind of battery called a Leyden jar, a glass jar filled with a fluid (acid) with immersed wires. As Alessandra has been trained to conceive of it, the fluid is necessary to produce a current across the wires. Now imagine Alessandra is in her lab, looking for a battery to use in her experiment. There is a stack of dry cell batteries in the corner, which are composed of a chemical paste and metal contacts. But she looks past them, and says, "There aren't any batteries here." To her, a dry cell cannot be a battery, because there is no fluid involved.

For Alessandra to be able to use the dry cells to generate a current for her experiment, or to draw any conclusions from that experiment, she will need to change her way of working with electricity. That need for change to a scientist's way of working is a practical result of incommensurability: former paradigms clash with practices, claims, or structures that emerge under new paradigms. Scientists can conduct certain experiments or prove certain results within one paradigm and not another. The paradigms can be compared to each other, but there is no common measure that reduces one to the other.[13] Certainly, it is true that a scientist can change her way of thinking and her way of working. But she cannot work exclusively within an unrevised fluid electricity paradigm and work with dry cells successfully at the same time.

4. THE PROBLEM WITH INCOMMENSURABILITY

The continuous explanation of science that Kuhn rejects has two attractive, connected features: continuity and realism. These features are central to contemporary realist accounts. Since scientists are referring to the same things, their descriptions and explanations have a secure basis for comparison over time. The history of science has a foundation of reference to real, publicly available phenomena. The behavior of scientists can be explained in these terms as well. Scientists make the inferences they do, and construct the theories they do, because their experiments and investigations put them in causal contact with objects and systems with stable properties.[14] The statements of scientific theories are intended as descriptions of those properties, and, when they fall short, they are corrected when the evidence is updated.[15]

The advantages of continuity and realism are palpable when there are rival approaches to the same phenomena. It is straightforward to say that theories compete to be the best descriptions and explanations of interesting phenomena. Moreover, we can explain scientists' behavior by rationally reconstructing[16] how they react to novel evidence, counterexamples, the development of new analytic tools, and the like. Such a reconstruction may be easier if we think of scientists using rival approaches as trying to measure the same things using different yardsticks.

Ever since Kuhn published *Structure*, there have been criticisms of the notion of incommensurability, and of Kuhn's related claim that paradigm shifts are not rationally reconstructible.[17] Fundamentally, one might deny Kuhn's thesis of the "priority of paradigms." We might paraphrase the priority thesis as the assertion that a scientist's way of working with the phenomena, "the set of results provable, puzzles solvable, and propositions cogently formulable" by a scientist, depends on the paradigm under which she is working (Patton 2015b, 57).

Denying the priority thesis has a number of apparently salutary results, related to the advantages of convergent realism. Paradigm shifts become rationally reconstructible, at least in principle, because there is a perspective from outside any given paradigm from which to evaluate competing paradigms. The incommensurability of successive paradigms is undermined as well, for the same reason. It is no longer impossible in principle to find a common measure with which to evaluate competing paradigms. The threat of "Kuhn loss," in which results achieved in one paradigm are not recoverable in a successive paradigm, no longer looms over science.

By these means, we regain an image of science and its practice that preserves a robust continuity consistent with convergent or, as Mizrahi (2013) has defended recently, relative realism.[18] The continuity involved could be continuity of empirical results and practices, or of equations and structural relationships. An essential assertion of many of Kuhn's critics is that there will always be a common measure between any two paradigms, according to which results and assertions of one can be recovered in the next.[19] Scientists need not find themselves blinkered by their training into seeing the world as it is structured by an artificial approach to problems.

Denying the thesis of the priority of paradigms also removes a barrier Kuhn had placed in the way of making the following assertions:

- In principle, there could be an epistemic standard that governs scientific practice and theory, in the past and in the present alike.
- The evidence for scientific claims is publicly available, inferences from such claims are based on fundamental rational or logical principles, and thus scientific research does not require initiation into a scientific élite.
- Scientists work in a common world and with publicly available phenomena.

It is a historical irony that these or similar assertions were characteristic of the Unity of Science movement, in whose *International Encyclopedia of Unified Science Structure* first appeared (Carnap, Morris, and Neurath 1970). As Wray (2016) notes, in a 1963 letter to Kuhn, Marjorie Grene "expresses surprise at where the book is published. 'It seems a bit of a joke that it should appear in the Unity of Science Series, of all places.'"[20] What's funny is that many of Kuhn's conclusions undermine the tenets of the Unity of Science program, and vice versa.

For instance, an account of scientific observation via Otto Neurath's protocol propositions (*Protokollsätze*) that operationalize publicly available observations seems to be ruled out by Kuhn. Kuhn's scientific observation is theory-laden and highly structured.[21] Conversely, Neurath's program of Protokollsätze supports an account of scientific observation that would put the brakes on *Structure* from the beginning. As Massimi (2015) has argued in detail, Kuhn argues at least for the semantic mind-dependence of scientific phenomena.[22]

Thus, we might think that Kuhn is arguing that researchers in rival paradigms are unable to understand each other in principle, not just disinclined to do so. They "work in different worlds," speak different languages, and are unable to cross the gulf of understanding.

5. PARADIGM INADEQUACY AND SUPER-PARADIGMS

In fact, we can say more. The following is an often unacknowledged premise of *Structure*:

> *Paradigm Inadequacy*: Not all phenomena are accessible, and not all scientific results are provable, from any single paradigm.

The careful work of Brorson and Andersen (2001) and of Hoyningen-Huene (1993) provides detail to the account according to which Kuhnian researchers working within a paradigm gain access to phenomena only from within a given paradigm, where that paradigm involves training and an artificial, structured approach to investigation, experiment, and inference.[23] Kuhn draws not only on the analysis of "vademecum" or "textbook" science from Fleck but also on the work of James Bryant Conant and the Harvard science studies curriculum (Wray 2016), on the notion of "tacit knowledge" from Polanyi (Timmins 2013), and on related notions from Toulmin and Foucault.

None of this work provides sufficient evidence for Kuhn's thesis of paradigm inadequacy. It can be true that researchers gain access to phenomena only from within a given paradigm but false that no *single* paradigm provides access to all the phenomena. Without the premise of paradigm inadequacy, though, many of Kuhn's assertions lose their force. Scientific revolutions, and the resulting incommensurability, would be temporary phenomena: mere inconveniences along the way to developing an even more powerful paradigm that dominates the old and the new approaches.

Kuhn might observe that no single paradigm *ever has* provided access to all scientific phenomena or to a way of solving all extant scientific puzzles. Kuhn could respond quite simply that his account is intended as a description and an explanation of the scientific past and present, which makes sense of scientific practice. The history of scientific practice is a history of warring paradigms, not of peaceful agreement.

Technically, paradigm inadequacy is a falsifiable claim. If someone were to develop a semantic and practical magic bullet, a scientific framework from which all scientific results are recoverable and which is perfectly transparent to all forms of scientific research both formal and experimental, that would falsify it. Scientists trained in a super-paradigm would be able to do research in any domain using the paradigm as a guide for their research; they would find that results in that domain immediately apply to phenomena in related domains; and the super-paradigm would show them how to use results in one domain to solve related puzzles elsewhere.[24]

Facts about the practice and development of science require a super-paradigm, a way of solving problems that works for every science, to pursue this way of falsifying the claim of paradigm inadequacy. As David Hilbert has emphasized, questions within physics are suggested by progress in mathematics, and vice versa. It is well-known that approaches to problems in chemistry affect practice in biology, and vice versa. And so on. Without a super-paradigm, there is always the possibility that a paradigm in a single given domain will fail when that domain is extended or drawn differently, to include problems and approaches within another science.

Fortunately for them, Kuhn's critics do not need to achieve a super-paradigm. They can make one of two moves instead:[25]

1. Question the evidence for paradigm inadequacy, even in the history of science.
2. Argue against an assumption behind the premise: that a "single" paradigm must be simple and not composite. If there always will be a bridge between successful paradigms, so that results in one can be assessed and rederived from the perspective of another, that is the practical equivalent of a single super-paradigm.

Many of Kuhn's critics (Lakatos, Friedman, etc.) have taken option 2. Mizrahi (2015a and forthcoming) takes both options, unifying the fronts against

Kuhn. He argues, in a forthcoming paper, that it is not true that the history of science is a "graveyard" of past theories, as Kuhn, Laudan, and others have asserted. While not formulated explicitly in these terms, this is option 1: to deny that the history of science is a history of warring paradigms, and to assert that science displays an underlying continuity.

Stephen Toulmin has argued that Kuhn's history of science is lacking, on different but related grounds:

> with experience, it has become clear to political historians that nothing is achieved by saying "and then there was a revolution," as though that exempted one from the need to give any historical analysis of a more explicit kind. To do only that is not to perform the historian's proper intellectual task, but to shirk it (Toulmin 1967, 84).

Toulmin objects to Kuhn's depiction of the history of science as displaying, not continuity, but radical breaks. Telling the history of science requires an adequate and comprehensive description and explanation of events. Appealing to a "revolution" is a trick. You can explain what happens up to the time of the revolution (at a time t_1, say) and what happens after t_1. But you do not consider yourself responsible for explaining the break at t_1, or the relationships between the "revolution" at t_1 and what happens before and after it. That, says Toulmin, is bad history. Toulmin's objection undermines the evidential base for Kuhn's assertion that the history of science is punctuated with breaks and revolutions. For Toulmin, the "evidence" for this assertion comes from failures of historical rigor: the historian finds a gap she cannot explain, and hypothesizes that this gap is *in the history* instead of *in her explanation* of the history.

Mizrahi's and Toulmin's objections point to a unifying theme in the criticisms of Kuhn, a way to unify (1) and (2). Understanding the history of science requires not just describing events but also explaining why they happened. That requires a standard, a common measure, that spans the history of science.

One popular strategy for providing such a measure is to argue that scientists in successive paradigms are *referring to* the same things, which amounts to a denial of Kuhn's thesis of "taxonomic incommensurability."[26] For instance, Leplin and others have defended "methodological realism," which includes the claim that scientific practice makes sense only if scientists understand themselves to be working with real things that in principle are accessible to other scientists.[27]

I do not believe that there is a global argument for the essential or universal incommensurability of rival scientific theories (see Patton 2015b, 2012). To that extent, I believe that Mizrahi (2015a) and I are in agreement. But, as should be clear by now, I do not agree that a paradigm is restricted to a theory that consists of assertions with truth values that depend on the existence and properties of the referents of their terms. Instead, I think a Kuhnian paradigm is a guide to practice within a scientific community. The interesting facet of Kuhnian incommensurability is thus that it is *practical* and *local* and, as I will conclude, can contribute to the understanding of particular historical events.

6. SCIENTIFIC PRACTICE AND LOCAL EXPLANATION

Kuhn shifted from his earlier emphasis on practical incommensurability, in *Structure*, to an emphasis on taxonomic incommensurability in later work.[28] In *Structure*, however, Kuhn places the main emphasis on practice, and thus on the account that I sketched in the opening sections of this chapter. On page 103, where he introduces the term "incommensurable," Kuhn writes:[29]

> paradigms differ in more than substance, for they are directed not only to nature but also back upon the science that produced them. They are the source of the methods, problem-field, and standards of solution accepted by any mature scientific community at any given time. As a result, the reception of a new paradigm often necessitates a redefinition of the corresponding science. Some old problems may be relegated to another science or declared entirely "unscientific." Others that were previously non-existent or trivial may, with a new paradigm, become the very archetypes of significant scientific achievement. And as the problems change, so, often, does the standard that distinguishes a real scientific solution from a mere metaphysical speculation, word game, or mathematical play (Kuhn 2012/1962, 103).

The last three examples are suggestive. What counts as a scientific problem? What counts as a solution? What is trivial, and what is interesting? What is a clever solution to a merely intellectual puzzle, and what is a substantial contribution, a real scientific achievement?

Kuhn refers to these aspects of local, communal scientific *practice* when he first defines incommensurability. Shifting paradigms can result in shifting community standards: what was an uninteresting problem can become interesting, and what was scientific can be seen as *un*-scientific. Within some traditions of natural philosophy, theology is continuous with physics, because the laws of nature are willed by God. Descartes thought his account of God was a necessary support for his account of the laws of nature, which, in turn, is central to his natural philosophy. He would not have divided the two pursuits, either. But someone in the contemporary context who studies theology is not considered to be pursuing the science of physics as it is done at MIT.[30] That is a sociological fact about the way we organize scientific pursuits, the way disciplines are divided, and the way we divide up problems among researchers. But it is also a practical way that Cartesian natural philosophy and current research paradigms differ.

Contemporary scientists do talk about God or the divine, but arguably they do not count results that are *only* statements about the existence or properties of God as the sole basis for demonstrations of results within physics. There are ways to interpret cosmology, for instance, which appeal to the divine. But it is likely that a contemporary physicist who submitted a proof to *Physical Review Letters* that depended *only* on statements about the nature and attributes of God would have that paper rejected as outside the scope of the journal. Researchers in Cartesian and Newtonian natural philosophy presented such proofs to the scientific community, and they were accepted as proofs within natural philosophy.

Note that Kuhn refers this practical result of paradigm shifts first and foremost to scientific practice, to choices within the scientific community. There is nothing necessary, much less logically necessary, about it. When Laplace was asked about the place of God in his system of physics, he (allegedly) replied, "I have no need of that hypothesis." Laplace developed a scientific achievement, a system, and an approach that broke with the tradition of natural philosophy in turning away from theological concerns. Laplace's system differs from Descartes' system in practice, in its results, and in its standards of explanation. For instance, Laplace's laws of nature are not concerned with or founded on the divine essence or will, while Descartes' are.[31]

We cannot understand Laplace, or Descartes, properly if we understand them to be working with the same entities, problems, and questions. Both are doing physics. But Descartes considers physics to be continuous with theology, and considers problems about God's essence and will to be central to solving problems *for physics*, including problems about the necessity of the laws of nature. Laplace sees himself as having no need of a theology that is continuous with his physics, and so he constructs a physical system that does not appeal to the existence of God or even include any assertions about God. Laplace does not consider problems about God's essence and will to be problems in the domain of physics.

Can we give a reason, a *scientific* reason, why Descartes was *wrong* to include God as a "hypothesis" in his system of physics? We can certainly argue that Laplace's system is simpler, and thus argue on the basis of Ockham's razor.[32] Locally, we can judge Laplace's system from Descartes', and Descartes' from Laplace's. Neither allows for a knockdown argument, that the other must accept, why statements about the existence and attributes of God should or should not figure in physical proofs. On Kuhn's account, an explanation of why a scientist takes certain problems seriously, and others not, may be based on scientific reasons of one kind or another, or it may be based on local facts about the development of a research tradition, including explanations of scientists' behavior that depend on such local facts. Such local, specific explanations of scientific practice are of value for the history and philosophy of science. Above all, Kuhn's emphasis on textbook science and on the initiation of researchers into a specific, local tradition is salutary. It is a mistake simply to assume that scientists who have been trained differently will approach the phenomena in the same way, see the same problems as salient, and so on. We may not be able to understand events in the history of science properly if we do not pay attention to the training, pedagogy, and initiation of scientists.[33]

Researchers are made, and they are made with difficulty. That much is familiar to any working scientist or mathematician. The practical analogue of the paradigm inadequacy thesis is that, as a local and practical matter, no scientist can solve every problem using her current scientific training. Kuhn's practical account thus leads to the injunction that scientists should be aware of the existence of rival approaches, should learn about them, and should be aware of the limitations of their own approaches.

To be sure, Kuhn himself was quite pessimistic about the prospects for enlightened science along these lines, arguing that scientists in his experience were dogmatic and blinkered.[34] That does not, however, license *global* claims about the inability of researchers from rival or distinct paradigms to understand each other, or to work in a common world.

Kuhn's early work was criticized by philosophers who wished to see a more robust role for language and semantics in his view. Kuhn's work was even "Sneedified," as Damböck (2014) details, so that it fit into the formal, semantic tradition associated with Sneed and Stegmüller.[35] Along the way, Kuhn's statements about incommensurability came to be seen—even by Kuhn himself—as broad claims about lexical or taxonomic "speciation" between theories (Marcum 2015b), and as limitations on the ability of scientists even to express their results using rival conceptual frameworks.

Such developments are a shame, in my view. Kuhn's original work did not restrict "paradigm" to "theoretical framework," nor did he restrict the perspective of scientific practice to the content of propositions with a truth-value. And it is mainly because Kuhn's arguments in *Structure* are outside the semantic view, and focus instead on the practice of science, that they are interesting and fresh.

Rather than reading Kuhn through the lens of semantic theorists like Quine, Davidson, and Sneed, I would urge reading Kuhn's project in the lines of recent work on the "context of pedagogy" (Kaiser 2005, Richardson 2012, Woody 2004) and Hasok Chang's emphasis on historical understanding (Chang 2010). It is a long-standing project in the history and philosophy of science to understand, not only what scientists take themselves to be saying, but also "what the devil scientists thought they were up to" (Rudwick 1985, 12).

Understanding local practices in science, the importance of training and education, the salience of which problems researchers take to be compelling, and the shifts that take place as standards change with novel achievements and changes to the context, are all necessary to working out what scientists are doing and—just as importantly, from a historical perspective—what they think they are doing.

NOTES

1 I would like to thank Moti Mizrahi for his kind invitation to contribute to this volume, and for the provocative and compelling questions he has raised. James Collier first invited me to respond to Mizrahi's work in the *SERRC*, and this was a first occasion to think about these questions. My own work has benefited in large measure from the nuanced and well-argued contributions of Vasso Kindi and James Marcum to the exchange with Mizrahi. Barry Lam invited me to think through incommensurability, paradigms, and other central Kuhnian concepts, which has helped me to craft clearer descriptions of them. Alan Richardson made incisive comments on inchoate versions of those descriptions. Some of the research for this chapter was supported by, and done during a visit to, Martin Kusch's ERC project, "The Emergence of Relativism."

2 "Most of the sources cited in *Structure* are sources in the history of science (see Wray 2015). To be precise, 60% of the sources cited in *Structure* are in the history of science" (Wray 2016, 10).

3 By Masterson's count (1970), Kuhn uses "paradigm" in twenty-one ways in *Structure*. And later, Kuhn admits that his thinking changed. But this is the first clear *definition* Kuhn gives in *Structure*. Brorson and Andersen (2001) explain the early and continued influence of Fleck's "textbook science" on Kuhn, the 1950s onward.

4 Those who do put emphasis on practice include Rouse (1998, 2013), Andersen (2000), Brorson and Andersen (2001, including an excellent bibliography of related work), Hoyningen-Huene (1993, 2002), Richardson (2002), and others. As Rouse (2013, 59) says, "Kuhn's challenge to received philosophical views has been domesticated by reading him as offering an alternative conception of scientific knowledge. Kuhn is better understood as rejecting knowledge-centric accounts altogether, in favor of understanding the practice of research."

5 Richardson (2002) has referred to paradigms as giving rules of a game—paradigms guide scientific research. To be sure, Kuhn argued that no comprehensive rules that govern problem solving (and guarantee problems can be solved) could be given ahead of time. But paradigms can give rules for how to approach those problems.

6 Classic criticisms come in the essays in Lakatos and Musgrave (1970). Mayo (1996) is a later example.

7 Perhaps unacknowledged.

8 Timmins (2013) weighs the allegation that Kuhn plagiarized ideas from Polanyi's *Personal Knowledge* and the broader question of Polanyi's influence on *Structure*.

9 See Brorson and Andersen (2001, 110). Wray observes, "Kuhn refers to Fleck's *Genesis and Development of a Scientific Fact* in the preface to *Structure*, noting that it was instrumental in helping him see that his own project was tied to 'the sociology of the scientific community'" (Kuhn 2012/1962 xli; Wray 2016, 4).

10 Hoyningen-Huene (1993, 187), cited in Brorson and Andersen (2001, 110), who link this notion to their analysis of Fleck.

11 See also Hoyningen-Huene (2002), which also discusses Feyerabend in this connection.

12 This is a simplified form of an example Kuhn uses often.

13 Massimi (2015, 84–86) provides a detailed, more technical analysis of one of Kuhn's examples of incommensurability: Galileo's and Aristotle's treatment of falling bodies, before and after Galileo's discovery of the law of free fall.

14 A paraphrase of the commitments of causal descriptivists, including Kripke; for a recent discussion and references to further work, see Patton (2015a).

15 A paraphrase of a classic conception of scientific realism provided by Bas van Fraassen: "Science aims to give us, in its theories, a literally true story of what the world is like: and acceptance of a scientific theory involves the belief that it is true" (1980, 8, emphasis removed).

16 A phrase used by Lakatos, which he ascribes to Carnap. The picture described here is *not* the logical empiricist one, even though some details (like the public availability of phenomena) are found in that tradition.

17 Lakatos argued that paradigm shifts are rationally reconstructible (see, e.g., his chapter in Lakatos and Musgrave 1970). Patton (2012) argues for that claim in a qualified way based on Laudan's "context of pursuit," and provides a bibliography of work on this subject.

18 To be sure, Kuhn's critics are not all realists. I have to simplify a vast literature in this discussion.

19 The work of Friedman (2008, 2001), for instance, analyzes successive paradigms in physics in which one serves as a limiting case of another, which preserves "retrospective rationality." Paradigms are nested like Russian dolls, so that a later, more comprehensive paradigm can explain earlier ones.

20 Marjorie Grene to Kuhn, September 25, 1963; details in references.

21 Of course, it is possible that Kuhn would allow for a ground level of observation that does not depend on theory or on a given practical approach. But his account of *scientific* observation is not at that ground level.

22 Massimi makes clear that Kuhn does not argue for the ontological mind-dependence of the phenomena. Still, as Clark Glymour has recalled, and Norton conveys, "Clark and Hartry Field were having lunch in the cafeteria when Hartry remarked on Kuhn's curious view. When Thomson made his discovery with cathode rays (or however it was done), that's when Kuhn believes electrons popped into existence. Did Kuhn really think that?! At that moment, Kuhn just happened to walk by. Clark stopped him and asked. 'Yeah, of course,' Kuhn replied and he walked away" (Norton 2012). As Norton notes, this exchange does not answer the question of precisely what Kuhn meant. I would note, in particular, he may have thought the specific *semantic kind*, "electron," began to exist when it was experimentally demonstrable,

rather than that the *physical referent* of "electron" did not exist. Massimi provides an argument for a view resembling the former assertion.

23 Moti Mizrahi commented on a draft of this chapter that this point raises the question of how science could ever have started. For instance, when the "first astronomers" looked at the night sky, would they have had access to a paradigm? In *Structure*, Kuhn identifies "pre-paradigm science" as the initial phase of research (this idea is discussed throughout the work, including on pages 20, 48, 61, 162, and 178 of the edition cited). Pre-paradigm scientists are still working within an agreed-upon conceptual and practical framework, however. On the reading discussed here, it is that framework that allows them to have access to structured "phenomena" and not bare sense data, for instance.

24 It is entertaining to think in more detail about what a super-paradigm could be. For instance, robot scientists equipped with supercomputers might be the only earthly beings equipped to carry out scientific research under such a paradigm. But I will leave these speculations aside, reluctantly, for now.

25 There are more possible moves, of course, but these are prominent ones.

26 As Sankey (1993, 1997) makes clear, Kuhn's notions of incommensurability change over time. Marcum (2015b, 153) connects Kuhn's later emphasis on "changes in the lexical taxonomy of a scientific specialty" with Kuhn's "Darwinian" picture of science, so that "scientific progress is analogous to biological speciation, with incommensurability as the isolation mechanism."

27 Leplin (1986, 33) gives the example of Millikan's oil drop experiment: "if we describe what Millikan was doing without mentioning electrons, we seem to impute to him an unaccountable, indeed perverse interest in the amount of electric charge with which X-radiation will endow an oil droplet. What was the experiment for if not to determine the charge of the electron?"

28 As Sankey and Marcum have emphasized, in the works cited in the chapter.

29 I am grateful to Alan Richardson for emphasizing this passage and its significance. (He is, of course, not therefore responsible for my reading or use of the passage.)

30 Intriguingly, in the contemporary context, some realities of funding are pushing in the opposite direction, toward the questions that were considered by natural philosophers.

31 I would like to thank Moti Mizrahi for pressing clarification of this discussion, which has improved the account.

32 Anyone who tries to do this is not terribly familiar with Laplace's system.

33 Alan Richardson pointed out the salience of this question in the context of scientific practice.

34 Steve Fuller (2000) has reproached Kuhn on this score, arguing that Kuhn, in his alliance with Conant, bolstered Cold War science through his conservatism about science (see also Wray 2016). While a full response is beyond the scope of this chapter, I would note the following. Criticizing Kuhn for writing science as conservative and dogmatic is like asking Agatha Christie why she murdered all those people. Kuhn's assertions that science is conservative are descriptive, not normative: Kuhn's descriptions of dogmatic scientists are not flattering. For a more detailed and persuasive argument on this score, see Kindi (2003).

35 Kuhn himself spoke approvingly of Sneed and Stegmüller, but *only* in the context of reconstructing hierarchies of theoretical terms within successive theories, a context in which Kuhn also speaks approvingly of the "often elegant apparatus developed by the logical empiricists" (Kuhn 2000/1987, 14).

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