5 Kuhn's Paradigms

Vasso Kindi

INTRODUCTION

"Paradigm" is a key term in Thomas Kuhn's account of science as it has been articulated in *The Structure of Scientific Revolutions* (1962).[1] It has helped Kuhn advance a picture of science quite different from the one prominent at the time. "Paradigm" shifted the focus of attention from the understanding of science as theory expressed in statements, to the actual practice of science which was modeled upon a paradigm. In this chapter I will first explain how "paradigm" was used in SSR, taking into account the term's reception and Kuhn's later qualifications. I will consider a particular understanding of paradigms that has drawn the attention of contemporary scholars, namely, the one which juxtaposes them to rules. By comparing Kuhn's "paradigm" to Wittgenstein's homonymous term, I will suggest that the contrast between paradigms and rules is misleading. I will argue that paradigms set rules which, when followed, build traditions and form frameworks. In that sense the polysemy of "paradigm" is not as troublesome as has often been thought.

1. PARADIGMS IN SSR

Paradigms are defined in SSR as "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" (10, cf. 23). They are the locus of professional commitment regarding standards and rules (11), they are not reducible to their components (11), they are not to be tested (122), and they are not to be corrected (122). Their function is both cognitive and normative (109). They:

- prepare students for membership in the community (11),
- permit and guide an esoteric type of research (11, 44),

- pick the class of facts that are worth determining with more precision and in a larger variety of situations (25),
- set and define the problems to be solved (27–28),
- are the criteria for choosing problems (37),
- guarantee a stable solution to these problems (28),
- breed problems and solutions (105),
- are the source of the methods, problem field, and standards of solution accepted by any mature scientific community at any given time (103),
- are the prerequisite to the discovery of laws (28),
- induce anticipations regarding phenomena (56),
- account for the observations and experiments easily accessible to the practitioners of science (62),
- lead the way for an exploration of an aspect of nature (92),
- give form to scientific life (109),
- tell the scientist about the entities nature does and does not contain and about the ways these entities behave (109),
- provide a map whose details are elucidated by research and the directions for map making (109),
- define a science (34, cf. 103),
- are constitutive of scientific activity and of nature (109–110),
- are prerequisite to perception (113),
- highlight perceptual features (125),
- provide a box into which nature can be shoved (151–152),
- are adopted largely on faith (158).[2]

As the above list illustrates, paradigms are seen as means that are instrumental in scientific education, as standards shaping scientific practice, and as the explanatory tools which are used to account for what science looks like. They are seen as operating at the level of practice (detected by the analyst in what goes on in scientific activity) but also at the level of analysis (that is, devised by the analyst to account for what goes on).

After the publication of SSR, the concept of paradigm was heavily criticized, at the same time that it was entering every possible field of study.[3] Arnold Thackray said that "it's a brilliant wrong idea" (cited in Coughlin 1982). David Hollinger (1980, 197) called it "[Kuhn's] most celebrated and maligned term" and Shapere (1980) found it mysterious, vague, and ambiguous. J. Wisdom (1974, 832) said that although it is a nice idea "it is not easy to say just what it means" while J. B. Conant wrote to Kuhn that he was afraid that he [Kuhn] will be brushed aside "as the man who grabbed on the word 'paradigm' and used it as a magic verbal wand to explain everything" (letter to Kuhn, June 5, 1961; cited in Cedarbaum 1983, 173).[4] Of course, one should not omit to mention Masterman's (1970) notorious identification of twenty-one different uses of paradigm in SSR, which largely overlap with the uses listed above.

2. KUHN'S RETROSPECTIVE ACCOUNT OF HOW HE UNDERSTOOD PARADIGMS

In his last interview, Kuhn says that the term "was a perfectly good word until [he] messed it up" (2000, 298). What was "paradigm" before he messed it up and how did he mess it up, if, in fact, he did? In the same remark he clarifies his point and says that "[p]aradigms had been traditionally models, particularly grammatical models of the right way to do things" (298). The same claim appears in SSR: "In its established usage, a paradigm is an accepted 'model' or 'pattern'" (23). But he cautions against a particular characteristic of grammatical models: in grammar, paradigms such as "*amo, amas amat*" are imitated mechanically in conjugating other Latin verbs, for example, in producing "*laudo, laudas, laudat.*" In SSR, Kuhn says that in science, unlike grammar, and like common law, "paradigm is rarely an object of replication . . . it is an object for further articulation and specification under new or more stringent conditions" (23). When Kuhn uses "paradigm" to account for the consensus of scientists, he appropriates this aspect of "model," namely, that it is a standard which is being followed rather than imitated[5] in different conditions. The problem with his employment of the term emerges, in his view, by not restricting it to the consensus about new model applications but by extending it to cover consensus about "a hell of a lot of other things [as well] that weren't models." So, he ends up using the term "paradigm" for "the whole bloody tradition" (2000, 299).[6]

It seems that Kuhn is rejecting as problematic one of the two major senses of "paradigm" as it was introduced in SSR. In the narrow sense of the term, a paradigm is a concrete achievement or model and in the wider sense, the framework or tradition built upon it.[7] Kuhn seems to be finding fault with this latter notion but he does not explain why. In his "Postscript," to disambiguate paradigm, Kuhn speaks of "exemplar" (paradigm in the narrow sense) and "disciplinary matrix" (paradigm in the wide sense).

Several issues can be raised in relation to this account of paradigms offered by Kuhn.

• Kuhn says that he does not care for paradigms as used in grammar because, in this context, any new application can fully replace the original example chosen to function as paradigm; every new application can, in principle, obliterate the previous one and stand in its place. Kuhn prefers to treat paradigms as precedents in common law. In legal practice, every new case may differ significantly from, and may add something to, the previous ones; every new application stands next to and does not erase or substitute for older cases. Given this preference for common law rather than grammar, it is puzzling why Kuhn objects to the wide sense of paradigms. He chose to use paradigms as models which are further articulated and not mechanically imitated exactly because he wanted to get a coherent tradition out of them. Drills with grammatical paradigms may teach students to conjugate

verbs (and so give rise to the relevant practice), but the examples used for the instruction of students, being instances of existing grammatical rules, are superfluous and dispensable; students can, in principle, learn to conjugate by using any other example or by invoking directly the relevant explicit rules. The examples are given to make instruction easier. In the case of science, however, as in the case of common law, paradigms are not instances of rules but, rather, concrete individual cases which are irreplaceable and indispensable in the process of instruction and initiation and in building and carrying out the relevant practice. "A paradigm is what you use when a theory [with the explicit axioms and rules] isn't there," says Kuhn, accepting Margaret Masterman's characterization (Kuhn 2000, 300). So, if Kuhn endorses understanding paradigms as building practices, it is not clear why he is reluctant to accept them as traditions. The term may be seen as used synecdochically: a particular item, the paradigm as concrete model, is used to refer to the whole tradition which is built by employing it. In this sense the two are not opposed.

• Another issue with Kuhn's account of what he did right and what he did wrong as regards the concept of paradigm is that it involves an inconsistency. In the same interview Kuhn relates how he wrote SSR:

> I wrote a chapter on revolutions, slowly but not with excessive difficulties and talking about gestalt [switches]. . . . Then I tried to write a chapter on normal science. And I kept finding that I had to—since I was taking a relatively classical, received view approach to what a scientific theory was—I had to attribute all sorts of agreement about this, that, and the other thing, which would have appeared in the axiomatization either as axioms or as definitions. And I was enough of a historian to know that that agreement did not exist among the people who were [concerned]. And that was the crucial point at which the idea of the paradigm as model entered. Once that was in place, and that was quite late in the year, the book sort of wrote itself. (Kuhn 2000, 296)

In this passage Kuhn says that he wanted to write about normal science which, by definition, involves extensive agreement about a number of things. He originally thought that this agreement would have to be about elements of scientific theories (i.e., propositions, axioms, and definitions). But, he couldn't find this kind of agreement. So, he hit upon the idea of paradigms and attributed "all sorts of agreement about this, that, and the other thing" to them. He, thus, solved his problem of writing about normal science. Yet, as was quoted earlier, he also complains that he made the mistake of using "paradigm" for more than one kind of agreement (for "a hell of a lot of other things"). There is some tension here. Kuhn, on the one hand, says that paradigms helped him account for *all sorts* of agreement in normal science and, on the other, that paradigms are responsible for only one sort

of agreement, namely, agreement regarding model applications. Any extension of the use of paradigm to cover agreement about other things is, in his view, illegitimate and results in messing up the use of the term.

• A third problem with Kuhn's account of what he did with paradigms appears when one considers that Kuhn, as noted earlier, proceeded to differentiate between "exemplar" (paradigm in the narrow sense) and "disciplinary matrix" (paradigm in the wide sense) in his "Postscript." If he objects to the wide use of "paradigm," that is, paradigm as tradition, why did he care to give it a different name in the "Postscript"? Why didn't he reject the concept of paradigm-as-tradition altogether? It seems that his only concern was to dispel the criticism of polysemy.

The above problems show that Kuhn's retrospective account of what went wrong with "paradigm" is not very satisfactory. Kuhn maintained that he should not have allowed the term to cover both exemplar and framework. The first remedy he offered already in the "Postscript" was to disambiguate the term, distinguishing between exemplar and disciplinary matrix. But he came to realize that this was not really what was causing trouble and dropped the use of "disciplinary matrix" altogether in his later writings (cf. Hoyningen-Huene 1993, 132).[8] He didn't have much use for "exemplar" either, preferring, instead, terms such as "lexicon," "taxonomy," "language," and "system," which, however, are more easily associated with the wider sense of "paradigm," a sense that he originally said he was interested to remove and which he took to belong to the level of language rather than that of practice. Under the pressure of criticism, which focused on issues of meaning variance and incommensurability of concepts, Kuhn increasingly concentrated on understanding the concept of paradigm linguistically, leaving behind the more practical dimensions which he originally introduced. Lexicons were taken to be wholes, consisting of concepts and terms forming sentences. In reconstructing his account, Kuhn proceeded to disown the concept of paradigm in the wide sense of tradition and acknowledged only the more dignified conception of paradigm as exemplar or model. But that move, which privileged exemplar over tradition, did not really make a difference in his overall scheme and, as we saw above, did not really smooth out tensions and inconsistencies. What is more, the concept of exemplar was not anymore very relevant to what Kuhn did and, consequently, it eventually fell out of his focus.[9] Other scholars, however, maintained that paradigm as exemplar is "Kuhn's most important concept" (Forrester 2007, 783; cf. Crane 1980, 33; Nickles 1998, 2003).[10]

In what follows, I will argue, *pace* later Kuhn, that the two notions (paradigm as exemplar and paradigm as tradition[11]) are very much connected and that he did not really mess up the term, at least in the original formulation in SSR. I will maintain that the use of exemplars sets rules which, when followed, establish a practice which eventually forms a tradition and a framework. In that sense, exemplars should not be contrasted

to rules, as several scholars have lately claimed. Wittgenstein's philosophy, which influenced Kuhn's work, will help me show these connections. I will begin by presenting how Kuhn came to use the term "paradigm" and then proceed to compare Kuhn's understanding of "paradigm" to Wittgenstein's use of the same term.

3. HOW DID KUHN COME TO USE "PARADIGM"?

We, first, need to distinguish between appropriating the term and getting the notion. The term, in its standard sense of prototype or paradigmatic sample, was widely used in every field of inquiry and appears already in Kuhn's *The Copernican Revolution* (1957). He says there that the cases from the history of science discussed at the General Education courses at Harvard College functioned "as paradigms [of science in history] rather than being intrinsically useful bits of information" (ix). Also, discussing telescopic observations of dark spots on the surface of the sun by Galileo, Kuhn says in the same book that "the motion of the spots across the sun's disk indicated that the sun rotated continually on its axis and thus provided a visible paradigm for the axial rotation of the earth" (221–222). In the same period, the term appears, in this standard sense, in most of the books Kuhn mentions in SSR: in Hanson's *Patterns of Discovery*, in Whorf's *Language, Thought and Reality*, in Polanyi's *Personal Knowledge*, and in Fleck's *Entstehung und Entwicklung einer wissenschaftlichen Tatsache*. Given all this, there is nothing special to explain regarding the employment of the term "paradigm" by Kuhn in its standard sense. What about the nonstandard use of the term in SSR?[12]

In his Preface to *The Essential Tension* (1977), Kuhn explains that he first used the term "paradigm" in the sense he eventually adopted in SSR, in his essay "The Essential Tension" (Kuhn 1977a), which was read at a conference at the University of Utah in 1959. The topic of the conference was creativity and scientific talent, and the participants, mostly psychologists, emphasized the importance of imagination, freedom, and open-mindedness. Against this view, which expresses the typical understanding of scientists, that is, as the intrepid, free spirits who conquer the frontiers of knowledge, Kuhn advanced the highly controversial claim that scientific creativity depends on convergent thinking which is achieved by a dogmatic initiation and a rigid education centered on repetitive exercises with concrete problem solutions, the paradigms.[13] It seems that Kuhn formulated his concept of paradigm (adopting also the term), drawing on his experience as a physicist but also as a historian. He had been subject himself to the education he described and he knew from his research in the history of science that "work within a well-defined and deeply ingrained tradition seems more productive of tradition-shattering novelties than work in which no similarly convergent standards are involved" (234).

K. Brad Wray, in his paper about the discovery of the idea of paradigm (Wray 2011), claims that Kuhn offers two different stories regarding the origin of his concept: one that focuses on the differences between natural and social sciences (natural sciences depend on paradigms while social sciences do not[14]) and another that focuses on the consensus necessary for effective research. Wray says that since Kuhn gives these two different stories, Kuhn himself may be mistaken about his own discovery (2). I don't see how this follows (one of the stories may be correct) but, most crucially, I don't see why the stories are different. I take it that the two accounts are complementary. Kuhn was struck by the fact that there is consensus in the practice of the natural sciences, something which is not the case with the social sciences, but he couldn't find evidence of agreement regarding explicit rules or definitions. At some point, he realized that the key was the type of education and he hit upon the word and concept of paradigm. That proved to be the "missing element" necessary to finish the book (Kuhn 1977, xix).

Daniel Cedarbaum, in "Paradigms" (1983), advances the view that Kuhn took "paradigm" from Wittgenstein, who was influenced, in turn, by Georg Lichtenberg. Cedarbaum states explicitly that Kuhn, whom he had interviewed, "does not remember taking the term "paradigm" from Wittgenstein" but insists that "Wittgenstein's treatment of naming in the *Philosophical Investigations* may have had a crucial impact on [Kuhn's] formulation of the paradigm concept in the spring of 1959."[15] Cedarbaum cites as evidence Kuhn's references to Wittgenstein's account of naming in SSR and mentions Stanley Cavell as quite possibly a major influence on Kuhn in that respect.[16] In his last interview, Kuhn denies categorically that he knew of Lichtenberg's or Wittgenstein's use of the paradigm concept: "I certainly was not aware of either of them. Lichtenberg was called to my attention [presumably by Cedarbaum], and I am a little surprised that I haven't had my nose dragged through Wittgenstein's use of it."(2000, 299). Cavell, however, recalls vividly in his autobiography (2010, 354) that Kuhn had told him at Berkeley that he [Kuhn] knew that Wittgenstein uses the idea of paradigm.[17]

It is difficult to settle the historical question of whether Kuhn did in fact get the concept and the term "paradigm" from Wittgenstein. He certainly knew of the relevant material but we cannot tell whether he got the actual term and notion from Wittgenstein. The reason, I think, Wittgenstein's use of the term "paradigm" was not registered by Kuhn is that Wittgenstein's concept, unlike, for instance, his concepts of language game or form of life, was very little discussed in the literature. There were brief and sporadic comments regarding similarities and differences between Kuhn's and Wittgenstein's term, mostly from within philosophy of science,[18] but there wasn't really any sustained discussion of Wittgenstein's "paradigm" in the secondary literature on his philosophy.[19]

One place where Wittgenstein's "paradigm" was used and, in fact, in relation to science, was Stephen Toulmin's book *Foresight and Understanding*,

which was published in 1961, just before SSR's publication. The term "paradigm" appears in its pages repeatedly. Toulmin, clearly, borrows the term from Wittgenstein and takes it to mean "object of comparison," again a Wittgensteinian term. Actually, Toulmin uses Wittgensteinian ideas throughout the book, and applies them to science. For instance, he argues that science cannot have a single aim and purpose that can be captured by a definition, echoing Wittgenstein's opposition to essentialism. Particularly telling is the comparison he makes between science and sport, which is very much reminiscent of Wittgenstein's discussion of games, as is Toulmin's claim that explanations reach rock bottom (1961, 42), a phrase very similar to Wittgenstein's idea that justifications reach bedrock where spades are turned (PI, 217).[20]

Apart from "objects of comparison" (Toulmin 1961, 52), paradigms for Toulmin are "ideals of natural order" (38), models and ideals as well as principles of regularity (42–43), fundamental patterns of expectation (47, 56), explanatory conceptions (52), "standards of rationality and intelligibility" (56), standard cases (57), intellectual patterns which define the range of things we can accept (81), preconceived notions (100). They stand to reason (42), are self-explanatory (42), set the regular order of things and what departs from it and needs explanation (54, 79), are not true or false (57), "take us further (or less far)" and are more or less "fruitful" (57). They change and develop and are broadly empirical, but one cannot confront them directly with the results of observation and experiment (100). Our commitments to them blind us to other possibilities (101). Those who accept different ideals have no common theoretical terms (57) while cross-type comparisons are not fair (62).

Kuhn does not compare his use of "paradigm" to Toulmin's in any of his writings. He says that he deliberately did not read *Foresight and Understanding* while he was trying to write SSR,[21] but he also admits, not referring to "paradigm" in particular, that he understands "why Toulmin might have been sore at me for stealing his ideas" (2000, 297). Toulmin, however, does not complain of any such thing.[22] In fact, he says that Wittgenstein's "theory of paradigms," which he also advocated, is very different from Kuhn's since Wittgenstein's, and his own, theory does not imply discontinuous change (Toulmin 1972, 106–107).[23]

In what follows I will not try to establish a detailed line of influence from Wittgenstein to Kuhn,[24] but, rather, I will argue that Kuhn's concept of paradigm has indeed much in common with and draws upon Wittgensteinian considerations regarding rule following, which also involve a discussion of paradigms and examples. More than simply establishing the affinities, I will argue that seeing Kuhn's paradigm from a Wittgensteinian perspective lends support to the original conception of paradigm by Kuhn, which combines both the narrow (exemplar) and the wide (framework) sense of the term. Accordingly, I will maintain that the contrast between exemplar and rule, which appears in Kuhn's work and features prominently in recent

secondary literature on Kuhn, is misleading and rests on a very particular understanding of the concept of rule.[25]

4. THE PRIORITY OF PARADIGMS IN SSR—THE CONNECTION WITH WITTGENSTEIN

In SSR Kuhn devotes a chapter to what he calls the priority of paradigms [over rules] in which he compares his discussion of paradigms to Wittgenstein's discussion of applying terms "unequivocally and without provoking argument" (45). His main concern in this chapter is to show that scientific activity, and the cohesion of scientific tradition, does not depend on the operation of rules but can rather be attributed to (and is, in fact, dependent upon) the operation of paradigms regardless of the existence of rules. He identifies paradigms with the standard illustrations of theories in their various applications which recur in textbooks, lectures, and laboratory exercises, while he understands rules to be isolable elements abstracted from paradigms and articulated retrospectively by scientists, that is, after they have been initiated into the profession and usually only if they are asked to rationalize or interpret what they do.

Kuhn gives four arguments in favor of the view that paradigms not only could function without the presence of rules, but do, in fact, function that way. The first argument notes the difficulty of discovering rules governing scientific practice. Just as in the case of games that Wittgenstein discusses, it is difficult to find their essential characteristics—what all games have in common; in a similar manner, Kuhn says, it is difficult to find the rules which can account for the correct practice of science.[26] Kuhn wants to say that just as we do not need the common, essential, characteristics of things to use the words referring to them correctly, in the same way we do not need rules to practice science properly. The second argument says that scientists do not actually come across rules in their scientific education and so, he implies, they do not need them. Scientists, Kuhn says, do not learn theories in the abstract, that is, by following abstract rules; rather, they study particular examples of theory applications (the paradigms) and do finger exercises with them. So, contrary to what is commonly believed about examples and paradigms in general, that is, that they are only needed to either document general propositions or illustrate rules, Kuhn maintains that paradigms are anything but dispensable. "They are not there merely as embroidery or even as documentation" (46–47). They have a vital role to play and, according to Kuhn, it is rules that are superfluous. The third argument focuses on the fact that rules become important only during periods of crisis. When scientists proceed securely in their practice following the paradigms of their discipline, they have no need for rules. This means that paradigms are prior to rules in the sense that paradigms need to be presupposed in order even for rules to become important in the rare occasion of a

crisis. The fourth and last argument aims to show that paradigms, unlike rules, are necessary to account for the diversity of scientific fields. "Explicit rules, when they exist, are usually common to a very broad scientific group, but paradigms need not be" (49). In the process of specialization scientists practice with and follow different paradigms even if they may presuppose common rules.[27]

In the course of this discussion, Kuhn invokes Wittgenstein to answer two questions: (1) what restricts scientists to a particular normal-scientific tradition in the absence of rules and (2) what can the phrase "direct inspection of a paradigm" mean? (44). In the case of Wittgenstein, as presented by Kuhn, the use of words is not restricted by some set of characteristics common to all members of the class referred to by the particular terms, but rather by a network of crisscross similarities which form natural families.[28] In the same way, the research problems addressed by the scientists in a certain tradition need not have anything in common (for instance, falling under some general rule). They may, instead, relate to each other by crisscross similarities and by having been modeled upon the accepted paradigms. By appealing to Wittgenstein, Kuhn solved his problem of accounting for the cohesion of a tradition in view of the difficulty he had in discovering any rules commonly adopted by scientists. So, the answer to the first question is "paradigms"; that's what restricts scientists to a particular course of practice. The answer to the second question is "identifying similarities and modeling a problem to a paradigm"; that's what "direct inspection of a paradigm" means.

So, Kuhn's preference for paradigms over rules in SSR was instigated by the fact that he could not discover rules which scientists would cite and follow to produce scientific knowledge. What kind of rules could these be? Methodological rules, for instance, how to experiment, how to make inferences, how to test laws, how to collect data, how to improve accuracy, how to expand a series, etc.; explicit definitions of scientific terms which would guide their implementation, but also elements of theories, such as laws or generalizations, which would cover all particular instances of application. For instance, a law of the form "all As are Bs" can be interpreted as a rule which dictates that all As ought to be also Bs. Having found no consensus upon these among scientists, Kuhn turned to paradigms.[29] It's not that scientists could cite paradigms instead of rules. Kuhn discovered paradigms looking into textbooks and scientific education. Instead of concentrating on what the philosophers described and prescribed as the scientific method, he concentrated on scientific practice and found particular problem solutions repeatedly used in textbooks and in training. These exemplary problems and solutions, which embody all the elements assigned to the disciplinary matrix later,[30] that is, both methodological and theoretical items, he called "paradigms." Kuhn did not claim, as Feyerabend did (1979, 18), that scientists do not follow rules because they are "unscrupulous opportunists," applying whatever methodology happens to suit the occasion.[31] Rather,

Kuhn turned his attention from an intellectualist, epistemological understanding of science, in terms of rigid formal rules, to a practical one which highlights the concrete idiosyncrasies of a practice. Feyerabend, a liberal pluralist, rejected the uniformity of method enjoined by the epistemologists and allowed more options; Kuhn gave up this picture of choosing from epistemological alternatives altogether and attributed agreement in practice to the type of training.

Subsequently in the literature, the privileging of exemplars over rules was taken out of the practical context of scientific education into a theoretical cognitive one and was understood as promoting a certain way of learning and reasoning, for instance, case-based over rule-based. Alexander Bird credits Kuhn with the thought that "not all human psychology can be explained in terms of rules and expert systems, and that in particular certain kinds of learning through concrete examples need not be seen in such terms (Bird 2000, 74). Thomas Nickles contended that "for Kuhn scientific methodology (insofar as that enterprise can be defended at all) is a case-based rather than a rule-based system" (Nickles 2000, 244),[32] although he admits that Kuhn says little about the representation of exemplars in human cognition (ibid., 249).

Kuhn did not say much about the role of exemplars in human cognition because this would mean that he would be engaged in doing psychology rather than philosophy. He experimented in this direction in the early period after SSR, but he eventually returned to the philosophical issues that mostly concerned him, namely, the problems of incommensurability and meaning change. Also, Kuhn's description of scientific methodology as case-based rather than rule-based is ambiguous. If the reference is to case-based, rather than rule-based, *reasoning*, then we credit Kuhn with an interest that he did not really have, namely, an interest in modeling, for instance, human reasoning, much like current work in the fields of cognitive science and artificial intelligence. If, on the other hand, a comparison is made between Kuhn's description of scientific methodology as case-based and the methodology followed in case-based disciplines, such as law, medicine, or psychoanalysis, then certain differences become salient. For one, the particular cases in the case-based disciplines are themselves the focus of attention; they are not treated as instances of some general rule. In both law and medicine, for instance, one is interested in describing and solving problems that pertain to the particular cases under consideration. Even precedents, which are appealed to in order to provide guidance for future decisions, are studied in detail so that, in later cases, specific similarities and differences between them are identified and assessed. In scientific methodology, on the other hand, as described by Kuhn, paradigmatic cases, the paradigms, are not studied for what they are in themselves, but as teaching tools for future action. Wittgenstein put the difference as follows: "Teaching which is not meant to apply to anything but the examples given is different from that which '*points beyond*' them." (PI 208).[33] In case-based

disciplines, the cases used as precedents (exemplars) need to be studied in detail in order to be able to tell whether a new case is similar to the precedent and can be ruled or treated similarly. In science training as described by Kuhn, however, exemplars are used to point beyond them. That is, students who learn projectile motion, for instance, do not need to concentrate on the particulars of the examples used in teaching it; for example, they do not need to remember the specific values of the variables in the original setting of the problem situation. They are asked to practice with exercises which are mapped onto, and are, therefore, *already* taken to be similar to, the exemplary one used in teaching. By solving these, students develop the skill to map future problems onto the original one and treat them accordingly. In case-based disciplines, we don't take it for granted but are trying to discover (or establish) whether the later case is similar to precedent. We know the specific characteristics of precedent and then check whether the case under consideration is similar to the original one. In science, always as described by Kuhn, the similarity between precedent and later case is not discovered post hoc but already given or imposed by training (cf. Lipton 2005, 1264). What one acquires by being exposed to Kuhnian paradigms is the skill to transcend the particular exemplary case and move on to future cases guided by it. This is an idea Kuhn found in Wittgenstein.

5. WITTGENSTEIN'S PARADIGMS AND RULES

Wittgenstein uses the term "Paradigma" in nearly all his published writings in the sense of sample (BB, 128; WL, 143), prototype (RFM III, 7), model (RFM III, 31, 41), standard (RFM VI, 22), object of comparison (BB, 166; PI, §50; PR, 57), rule (PG, 419; RFM III 28; LFM, 55).[34] A paradigm can be something special, like the standard meter kept in Paris, or just any example of a specific practice (for instance, an example of language usage, of mathematical proof, or of behavior) that is chosen as a means of instruction in order to be followed on future occasions. It is something particular, a concrete case which has, however, a general import. It lays down what is correct to do by being itself the measure of correctness. The idea is that a sample of something is used to show how one is to go on in accordance with it. Instead of having a general formula, or an injunction expressed in words, which tells us what to do, we have a concrete instantiation of, say, some color, or of an arithmetical calculation which we learn to follow. Several problems immediately arise: (1) What is the relation between the samples used in instruction and the rules they set? (2) How do the rules compel us in a certain direction? (3) How are we to know which of the properties not only possessed by the sample used for instruction, but also exemplified by it,[35] are to be concentrated on and followed? When a child, for instance, is shown a swatch of blue cloth and told "blue," how is she to know whether the term refers to the color, the texture, the shape, or any other quality the

swatch exemplifies and use the term accordingly in the future? (4) How is one guided to move from the original sample, or paradigm, to all the other cases that are supposed to be treated as similar to it? How does one read off the instruction and how is generality achieved? (5) Is the similarity between paradigm and future cases something waiting to be recognized or is the similarity imposed and established in practice?

All these issues are taken up by Wittgenstein in his discussion of rule following and by scholars in the vast secondary literature on the topic. I will not review the discussion and controversies here; what I would like to do is to sketch briefly how I see Wittgenstein's understanding of rule following in order to illuminate Kuhn's understanding of paradigms. I would like to appropriate Wittgenstein's work on the issue to make three points: (1) discussion of paradigms is not to be seen as dealing with an empirical account of how human beings follow rules nor with modeling human reasoning, but rather with issues that pertain to how rules relate to practice; (2) paradigms should not be contrasted to rules; and (3) it is not a mistake to understand paradigms both in a narrow and in a wide sense.

Wittgenstein understands prototypes or paradigms as models which articulate a particular way of conceiving things: "the prototype must be presented for what it is; as characterizing the whole examination and determining its form. In this way it stands at the head & is generally valid by virtue of determining the form of examination, not by virtue of a claim that everything which is true only of it holds for all the objects to which the examination is applied." (Manuscript 111, 119–120; referred to in Kuusela 2008, 124). That is, paradigms set the stage, open up a space in which things are supposed to be done in the way exemplified by the paradigm. This does not mean that one must copy or reproduce exactly what the paradigm says or looks like; rather one is supposed to move forward by assimilating further cases to the exemplary ones used in instruction. As Wittgenstein put it, paradigms are "centres of variation" (Manuscript 152, 16–17; referred to in Kuusela 2008, 173). Once the example of a prototype is followed, a rule is (or rules are) set and a particular practice emerges. For instance, being exposed to samples of color one learns what color is and how to use the words for color. By following the rules derived from the samples (on what occasions one uses the relevant words), one learns to participate in the practice of using color words (referring to color, identifying color, distinguishing colors, etc.), which involves also extending the practice. Thus, the original paradigm (the particular exemplar), is responsible for the development of a whole practice. It's like a point which expands into a whole open-ended area.

Wittgenstein's discussion of rules and paradigms is not empirical. He did not seek to uncover facts regarding the way human beings learn and follow rules. Nor did he try to model human reasoning. His effort aimed at sketching the grammar of the relevant notions. Kuhn should be seen as doing something similar. He was not involved in a psychological or sociological

investigation but rather in a logical one informed by history, which offered an account of the conditions that make science possible (cf. Kindi 2005). If we see Kuhn's paradigms under these Wittgensteinian lights, then we can also see how it is possible to understand paradigms both in the narrow (exemplar) and the wide (practice-tradition) sense. As regards the relation of paradigms to rules we saw that Kuhn, and later commentators, contrasted the two notions. However, Wittgenstein's analysis may help us clarify what Kuhn meant and allow for a more complex understanding.

Traditionally, it was supposed that one learns what a word or a formula means by being given a definition which functions as a rule. For instance, "horse" means a large four-legged animal with a mane. Given this definition, one knows how to proceed, that is, how to use the word "horse" and how to identify horses. Understanding the definition, or any kind of instruction, meant understanding the words comprising it. But we cannot go on to explain words by words for ever. As Schlick put it,

> In order to arrive at the meaning of a sentence or a proposition we must go beyond propositions. For we cannot hope to explain the meaning of a proposition merely by presenting another proposition. . . . I could always go on asking 'But what does this proposition mean?' You see there would never be any end to this kind of enquiry, the meaning would never be clarified if there were no other way of defining it than by a series of propositions. . . . The discovery of the meaning of any proposition must ultimately be achieved by some act, some immediate procedure, for instance, as the showing of yellow; it cannot be given in a proposition. (cited in Hanfling 1981, 19)

Connecting words to samples was considered to be one way of linking language to the world. But given the problems of exemplification noted by Goodman (which of the properties possessed by the sample are exemplified?), samples cannot unproblematically play the role of anchoring language. Just pointing to a sample does not tell us what features to concentrate on and what to do to follow it. Wittgenstein took samples to be already symbols, that is, already connected to the world through a particular linguistic, or other, practice. The way a sample is copied and followed cannot just be dictated by the sample *qua* physical object but has to be learned in training and picked up in practice.

Samples, or exemplars, set rules but also, through their use in a particular practice, anchor rules to reality and restrict their application. Rules cannot determine by themselves how they ought to be followed. We would be involved in an infinite regress if we were to add rules to rules to learn how to employ them. We need examples of application to break the impasse and learn how to go on. If practical training with examples was missing, then any course of action could be shown to be consistent with the expression of the rule, which means that "there would be neither accord nor conflict

here" (PI, §198–202). We would be trapped in an infinite regress of interpretations, substituting one expression of the rule for another. What makes the following of a rule possible, in a specific and concrete manner, is training with actual cases.

Not only rules, but also examples are needed for establishing a practice. Our rules leave loopholes open, and the practice has to speak for itself (OC, 139).[36] So, rules need examples/exemplars in order to be properly followed; practice with exemplars sets rules which, as they are followed, form traditions.[37]

6. CONCLUSION

The complex understanding of the relation between exemplar and rule offered by Wittgenstein fits very well, I think, Kuhn's use of the relevant notions. The contrast between exemplar and rule which we find in Kuhn's work should not be seen as a general opposition but, rather, as a contrast between following particular examples of application and following abstract expressions of general methodological rules dictated by a particular philosophical understanding of science. Kuhn maintained that scientists do not follow the abstract rules that epistemologists used to prescribe. They are rigorously trained to emulate concrete examples of scientific achievement. This practice gives rise to a rule-governed behavior which eventually builds a tradition. The rules followed are not general prescriptions of the form "confirm your hypotheses" but, rather, patterns of behavior, closely modeled on exemplars, which have been mastered practically in education and carried over in research. The mutual dependence of exemplar and rule and their common connection to tradition show that the notorious polysemy of "paradigm" is not to be eradicated as pernicious. The different senses of the term (for instance, exemplar and framework or tradition) reflect the different functions performed by paradigms.

NOTES

1. Abbreviated as SSR. All references to this book are given directly by page numbers.
2. In the above list I kept (with occasional editing) Kuhn's phraseology.
3. See, for instance, the indicative study of Cohen (1999).
4. Cf.: "Unfortunately that word [paradigm] has been used in so many senses, not least in Kuhn's (1970) classic discussion itself, that its meaning has been almost blurred out of existence" (Lipton 2005, 1264); "Kuhn's portmanteau notion of the 'paradigm'" (Daston 2010, 217).
5. Cf. the distinction between imitation and following made by Kant in §32 and §49 of his Third Critique (Kant 2000). Martin Gammon (1997) extensively discusses the differences Kant notes between imitation (*Nachahmung*), following (*Nachfolge*), mechanical replication (*Nachmachung*), and aping

(*Nachäffung*). According to Gammon (1997, 586), Kant also distinguishes between two senses of the exemplary: as an archetype (*Urbild*) for emulation and as a pattern (*Muster*) for imitation. No copy is adequate to the archetype; it is the original measure of things. Kant sees the work of genius in fine art "as an archetype (*Urbild*) for the emulation (*Nachfolge*) of future geniuses, as a pattern (*Muster*) for the imitation (*Nachahmung*) of future artists, as a model (*Modell*) for the replication (*Nachmachung*) by schools and as an expression of peculiarity (*Eigenthumlichkeit*) which may serve for the aping (*Nachäffung*) of counterfeits, plagiarists, and 'tyros'" (ibid., 588). The work of a genius sets a new rule not for future geniuses but for aesthetic instruction in schools for future artists. In science, Kant says, we can learn the work of great inventors, who differ from an imitator and apprentice only in degree, "but we cannot learn to write in truly poetic vein" (ibid., 590).

6. Kuhn does not specify where this sleight of hand took place, whether in SSR or later. Judging from what he says in the Preface of *The Essential Tension* (1977, xviii), it seems that the expansion of the concept took place even before the publication of SSR. "That concept [paradigm] had come to me only a few months before the paper ["The Essential Tension"] was read [in June 1959], and by the time I employed it again in 1961 and 1962 [in the first draft of SSR and in "The Function of Dogma in Scientific Research"] its content had expanded to global proportions, disguising my original intent." "[P]aradigms took on a life of their own" (ibid., xix).
7. Toulmin (1963, 384) understands differently the distinction between a narrower and a wider sense of paradigm. Paradigm in the narrow sense refers to a particular set of basic concepts while paradigm in the wider sense refers to a whole masterpiece of science.
8. In a letter to Donald Gillies (2 April 1990, Kuhn Archive, Box 20) Kuhn expresses his preference for the term "research programme" as substitute for the wider sense of paradigms: "I've often wished that I'd thought of the term 'research programme' for the more popular of the two different senses in which I used 'paradigm.'"
9. Wray claimed that, although theory change became taxonomic change in Kuhn's later writings, exemplars did not become obsolete or irrelevant to our understanding of science since they continued to be important to how theories are learned (Wray 2011, 392). Wray, however, neglects to mention that Kuhn was not as much interested in scientific education as he had been when he wrote SSR.
10. It's not clear, however, whether their claim relates to the significance of exemplar in general or as regards Kuhn's philosophy only.
11. Disciplinary matrix and tradition do not exactly mean the same thing. I take disciplinary matrix to be a logical notion and tradition to be an epistemological and sociological one. Disciplinary matrix is introduced to provide the context in which specific elements, such as generalizations, models, values, and exemplars, are orderly arranged. The concept of tradition refers to how knowledge is transmitted and put to use in the practice of a scientific community.
12. On June 29, 1961, in his letter to J. B. Conant, where he responds to the latter's criticism of paradigm, Kuhn says that his usage, "though certainly not normal, . . . does not strain the dictionary definition as much as [Conant] impl[ies]" (Kuhn Archive, Box 25; cf. Kuhn 1977, xix).
13. Kuhn presents the same ideas in "The Function of Dogma in Scientific Research" (1963; 1963a), read at Oxford in July of 1961.
14. Read and Sharrock (this volume) claim that the key to understanding Kuhn's work is to reflect on the contrast between the social and the natural sciences.

15. At that time, Kuhn found himself at an impasse trying to account for the consensus in the period between scientific revolutions.
16. Cf. Kindi (2010) for a slightly different account of the relation between Kuhn and Cavell.
17. It is worth quoting the passage in full: " . . . Kuhn, perhaps after a department meeting, accompanied me home for a drink, and, talking past midnight Tom was becoming agitated in a way I had not seen. He suddenly lurched forward in his chair with a somewhat tortured look that I had begun to be familiar with. 'I know Wittgenstein uses the idea of "paradigm". But I do not see its implications in his work. How do I answer the objection that this destroys the truth of science? I deplore the idea'" (Cavell 2010, 354–355).
18. Toulmin (1973, 284n12) and in a certain sense Stegmüller (1976, 170ff.).
19. The only exceptions that I am aware of are Toulmin (1961), where the term is used rather than analyzed, Luckhardt (1978), Kindi (1995), and Kindi and Zika (2005).
20. None of the reviews of *Foresight and Understanding*, however, mentions the connection to Wittgenstein (see Achinstein 1963; Cooper 1963; Kyburg 1963). Toulmin acknowledges his "special debt to the late Professor Ludwig Wittgenstein" in his Preface to his earlier book *The Philosophy of Science: An Introduction* (1953).
21. In the same place he says that he hadn't read Polanyi's book either, but he cites the book in SSR, pointing to two particular chapters. In general, I remember also from a personal communication that he avoided reading books relevant to what he did from fear, as he said, that he wouldn't be able (he wouldn't have the time) to formulate and express his own thoughts.
22. For instance, Toulmin does not connect his and Kuhn's use of "paradigm" in his commentary on Kuhn's work at Bedford College in 1965 (Toulmin 1970), while in *Human Understanding* (1972, 100–101) he explicitly compares Kuhn's paradigms to Collingwood's absolute presuppositions.
23. Cf. "The use of this term [paradigm] by Wittgenstein himself . . . differs significantly from that made familiar recently by T. S. Kuhn in his much discussed book, *The Structure of Scientific Revolutions*" (Janik and Toulmin 1973, 284n12).
24. Wray (2011) offers a history of Kuhn's discovery of paradigms.
25. Nickles (this volume) also discusses Kuhn's understanding of exemplars and their role in establishing the practice of normal science. Nickles' account is less exegetical in comparison to mine and aims at showing that normal science is not a static phase, but that it evolves, has a structure, and merges with extraordinary science. According to Nickles, exemplars themselves also evolve.
26. Here, it should be noted that mention of difficulty does not imply that these common characteristics or the rules exist, but are only difficult to find.
27. Here Kuhn gives the example of the physical scientists who all learn the laws of quantum mechanics by being given, however, different paradigm applications in their various special fields. It seems that in this example Kuhn identifies rules and laws as being both abstract.
28. It is noteworthy that Kuhn expresses here for the first time a concern that will come to obsess him later in his work. He worries about how the world features in and channels our practices. "Wittgenstein . . . says almost nothing about the sort of world necessary to support the naming procedure he outlines" (45n2). Although I agree with Read and Sharrock (this volume) that Kuhn is not in general interested in metaphysical issues, Hanne Andersen (this volume) is right in suggesting that, in the case of family resemblance concepts, Kuhn moves beyond Wittgenstein when he formulates a particular

condition on the world in order to account for our success in identifying the objects and activities corresponding to the family resemblance concepts.

29. Peter Lipton described the situation as follows: Kuhn, having turned from physics to the history of science, was struck by the consensus he observed among scientists during periods of normal science. "It is as if all the scientists in the group had the same secret rulebook for doing good science in their specialty. The nonexistence of the rulebook gave Kuhn his question: how does one explain the rule-like behaviour of a scientific community in the absence of rules? Kuhn's answer: by exemplars" (2005, 1264).
30. As noted earlier, Kuhn in his "Postscript" (182–187) distinguishes between exemplar and disciplinary matrix but includes exemplars as elements of the matrix. The other elements are symbolic generalizations, models, and values.
31. Feyerabend (1979, 18) quotes Einstein, who said that the scientist "must appear to the systematic epistemologist as a type of unscrupulous opportunist."
32. To be sure, Nickles also considers Kuhn a schema theorist, that is, as adopting the view that past experience shapes future experience via schemata, that is, large organizing structures, such as the large paradigms or disciplinary matrices, which are created by this past experience (2000, 246, 247).
33. Cf. "[In] an ostensive definition I do not state anything about the paradigm (sample), but only use it to make statements, that it belongs to the symbolism and is not one of the objects to which I apply the symbolism" (BT, 408).
34. The particular references to these different senses of "paradigm" in Wittgenstein's philosophy are merely indicative since similar uses can be found in many other places in his published work. The term "exemplar" is rarely employed. In one place, PI §272, the German term *Exemplar* is better translated as specimen (see the new translation by Hacker and Schulte), while in Z 444 the English 'exemplar' is used by Anscombe to translate *Urbild*.
35. Cf. Nelson Goodman's discussion of exemplification (1976, 52–57).
36. Kant had already seen this. In the *Critique of Pure Reason* (A133–134/B172–173), he claims that we cannot add rules to rules to learn how to employ them. That would involve us in an infinite regress. We need adequate training with particular examples to learn how to go on.
37. One might say that a particular example is as mute as the expression of a rule as regards the way it is to be followed. But an example (or a sample) which is made part of such a process of instruction is not a mere object. It is an object (be it a spoken or a written word, a sample, an experimental apparatus, an expression of a scientific law) put to a specific use which concretely illustrates the way it is to be further applied. Students pick it up as teachers muster a number of techniques to beat students into line (PI 208). The course of development in subsequent applications of the examples learned may not be smooth and may branch out in different directions which may involve the introduction of new examples of use.

REFERENCES

Achinstein, Peter. 1963. Review of *Foresight and Understanding*, by Stephen Toulmin. *Isis* 54: 408–410.

Andersen, Hanne. This volume. "Scientific Concepts and Conceptual Change."

Bird, Alexander. 2000. *Thomas Kuhn*. Chesham: Acumen

Cavell, S. 2010. *Little Did I Know: Excerpts from Memory*. Stanford, CA: Stanford University Press.

Cedarbaum, D. G. 1983. "Paradigms." *Studies in the History and Philosophy of Science* 14(3): 173–213.
Cohen, Jon. 1999. "The March of Paradigms." *Science*, New Series, 283(5410): 1998–1999.
Cooper, Neil. 1963. Review of *Foresight and Understanding*, by Stephen Toulmin. *The Philosophical Quarterly* 13(51): 180–181.
Coughlin, Ellen K. 1982. "Thomas Kuhn's Ideas about Science. 20 Years after the Revolution." *The Chronicle of Higher Education*, Sept. 22, 1982: 21–23.
Crane, Diana. 1980. "An Exploratory Study of Kuhnian Paradigms in Theoretical High Energy Physics." *Social Studies of Science* 10(1): 23–54.
Daston, Lorraine. 2010. "Human Nature Is a Garden." *Interdisciplinary Science Reviews* 35(3–4): 215–230.
Feyerabend, Paul. 1979. *Against Method*. London: Verso.
Forrester, John. 2007. "On Kuhn's Case: Psychoanalysis and the Paradigm." *Critical Inquiry* 33: 782–819.
Gammon, Martin. 1997. "Exemplary Originality." *Journal of the History of Philosophy* 35(4): 563–592.
Goodman, Nelson. 1976. *Languages of Art*. Indianapolis, IN: Hackett.
Hanfling, Oswald. 1981. *Logical Positivism*. Oxford: Blackwell.
Hanson, N. R. 1958. *Patterns of Discovery*. Cambridge: Cambridge University Press.
Hollinger, David. 1980. "T. S. Kuhn's Theory of Science and Its Implications for History." In G. Gutting (ed.), *Paradigms and Revolutions*. Notre Dame: University of Notre Dame Press, 195–222.
Janik, Alan, and Stephen Toulmin. 1973. *Wittgenstein's Vienna*. New York: Touchstone Book.
Kant, I. 1990. *The Critique of Pure Reason*. Trans. Norman Kemp Smith. New York: Macmillan.
Kant, I. 2000. *Critique of the Power of Judgment*. Ed. Paul Gauyer. Trans. Paul Guyer and Eric Matthews. Cambridge: Cambridge University Press.
Kindi, V. 1995. *Kuhn and Wittgenstein: Philosophical Investigation of The Structure of Scientific Revolutions*. Athens, Greece: Smili Editions (in Greek).
Kindi, V., and F. Zika. 2005. "Paradigms and Examples in Wittgenstein's Philosophy." In K. Ierodiakonou (ed.), *The Use of Examples in Philosophy*. Athens, Greece: Ekkremes Editions, 75–103 (in Greek).
Kindi, V. 2010. "Novelty and Revolution in Art and Science: The Connection between Kuhn and Cavell." *Perspectives on Science* 18(3): 284–310.
Kuhn, Thomas S. 1957. *The Copernican Revolution*. Cambridge, MA: Harvard University Press.
Kuhn, Thomas S. 1962, 1970. *The Structure of Scientific Revolutions*. With added "Postscript" in the second edition. Chicago: The University of Chicago Press.
Kuhn, Thomas S. 1963. "The Function of Dogma in Scientific Research." In A. Crombie (ed.), *Scientific Change Symposium on the History of Science, University of Oxford, 9–15 July 1961*. New York: Basic Books, 347–369.
Kuhn, Thomas S. 1963a. "T. S. Kuhn's Intervention in the Discussion Following 'The Function of Dogma in Scientific Research.'" In A. Crombie (ed.), *Scientific Change Symposium on the History of Science, University of Oxford, 9–15 July 1961*. New York: Basic Books, 386–395.
Kuhn, Thomas S. 1977. *The Essential Tension: Selected Studies in Scientific Tradition and Change*. Chicago: The University of Chicago Press.
Kuhn, Thomas S. 1959, 1977a. "The Essential Tension: Tradition and Innovation in Scientific Research." In *The Essential Tension: Selected Studies in Scientific Tradition and Change*. Chicago: The University of Chicago Press, 225–239.

Kuhn, Thomas S. 2000. "A Discussion with Thomas Kuhn." In *The Road Since Structure*. Edited by James Conant and John Haugeland. Chicago: The University of Chicago Press, 255–323.
Kuusela, Oskari. 2008. *The Structure against Dogmatism: Wittgenstein and the Concept of Philosophy*. Cambridge, MA: Harvard University Press.
Kyburg, Henry E. Jr. 1963. Review of *Foresight and Understanding*, by Stephen Toulmin. *The Philosophical Review* 72(1): 115–116.
Lipton, Peter. 2005. "The Medawar Lecture 2004. The Truth about Science." *Philosophical Transactions of the Royal Society* 360: 1259–1269.
Luckhardt, C. G. 1978. "Beyond Knowledge: Paradigms in Wittgenstein's Later Philosophy." *Philosophy and Phenomenological Research* 39(2): 240–252.
Masterman, M. 1970. "The Nature of a Paradigm." In Imre Lakatos and Alan Musgrave (eds.), *Criticism and the Growth of Knowledge*. Cambridge: Cambridge University Press, 59–89.
Nickles, Thomas. 1998. "Kuhn, Historical Philosophy of Science, and Case-Based Reasoning." *Configurations* 6: 51–85.
Nickles, Thomas. 2000. "Kuhnian Puzzle-Solving and Schema Theory." *Philosophy of Science* 67, Supplement. Proceedings of the 1998 Biennial Meetings of the Philosophy of Science Association. Part II Symposia Papers, 242–255.
Nickles, Thomas. 2003. "Normal Science: From Logic to Case-Based and Model-Based Reasoning." In T. Nickles (ed.), *Thomas Kuhn*. Cambridge: Cambridge University Press, 142–177.
Nickles, Thomas. This volume. "Some Puzzles about Kuhn's Exemplars."
Pickering, Andy. 1980. "Exemplars and Analogies: A Comment on Crane's Study of Kuhnian Paradigms in High Energy Physics." *Social Studies of Science* 10: 497–502.
Polanyi, Michael. 1958, 1974. *Personal Knowledge: Towards a Post-Critical Philosophy*. Chicago: University of Chicago Press.
Read, R., and W. Sharrock. This volume. "Kuhn's Fundamental Insight—Reflection on the 'Social Sciences,' as a Pedagogical and Philosophical Tool for Thinking Adequately about the Natural Sciences."
Shapere, D. 1964, 1980. "The Structure of Scientific Revolutions." Reprinted in G. Gutting (ed.), *Paradigms and Revolutions*. Notre Dame: University of Notre Dame Press, 27–38.
Toulmin, Stephen. 1953. *The Philosophy of Science: An Introduction*. London: Hautchinson's University Library.
Toulmin, Stephen. 1961. *Foresight and Understanding: An Enquiry into the Aims of Science*. Bloomington, Indiana: Indiana University Press.
Toulmin, Stephen. 1963. "S. E. Toulmin's Intervention in the Discussion Following T. S. Kuhn's 'The Function of Dogma in Scientific Research.'" In A. Crombie (ed.), *Scientific Change Symposium on the History of Science, University of Oxford, 9–15 July 1961*. New York: Basic Books, 382–384.
Toulmin, Stephen. 1970. "Does the Distinction between Normal and Revolutionary Science Hold Water?" In Imre Lakatos and Alan Musgrave (eds.), *Criticism and the Growth of Knowledge*. Cambridge: Cambridge University Press, 39–47.
Toulmin, Stephen. 1972. *Human Understanding*. Princeton, NJ: Princeton University Press.
Watling, John. 1964. Review of *Foresight and Understanding*, by Stephen Toulmin. *The British Journal for the Philosophy of Science* 15(58): 164–166.
Wisdom, J. O. 1974. "The Nature of 'Normal Science.'" In P. A. Schilpp (ed.), *The Philosophy of Karl Popper*, *The Library of Living Philosophers*. Vol. 14. LaSalle, IL: Open Court, 820–842.

Wittgenstein, Ludwig. 1951, 2009. *Philosophical Investigations*. Trans. G. E. M. Anscombe, M. S. Hacker, and J. Schulte. Oxford: Blackwell. (Abbreviated as PI)
Wittgenstein, L. 1958. *The Blue and Brown Books*. Ed. Rush Rhees. Oxford: Basil Blackwell. (Abbreviated as BB)
Wittgenstein, L. 1975. *Wittgenstein's Lectures on the Foundation of Mathematics*. Ed. Cora Diamond. Chicago: The University of Chicago Press. (Abbreviated as LFM)
Wittgenstein, Ludwig. 1978. *Philosophical Grammar*. Ed. R. Rhees. Berkeley: University of California Press. (Abbreviated as PG)
Wittgenstein, L. 1978. *Remarks on the Foundations of Mathematics*. Ed. G.H. von Wright, R. Rhees, and G. E. M. Anscombe. 3rd ed. Oxford: Basil Blackwell. (Abbreviated as RFM)
Wittgenstein, L. 1979. *Wittgenstein Lectures. Cambridge 1932–1935*. From the Notes of Alice Ambrose and Margaret Macdonald. Ed. Alice Ambrose. Amherst, NY: Prometheus Books.
Wittgenstein, Ludwig. 2005. *The Big Typescript*. Ed. and trans. C. Grant Luckhardt and Maximilian A. E. Aue. Oxford: Blackwell. (Abbreviated as BT)
Wray, Brad K. 2011. "Kuhn and the Discovery of Paradigms." *Philosophy of the Social Sciences* 41(3): 380–397.

ARCHIVAL MATERIAL

Letter from Thomas S. Kuhn to James B. Conant, 29 June 1961. Thomas S. Kuhn Papers, MC 240, box 25. Massachusetts Institute of Technology, Institute Archives and Special Collections, Cambridge, Massachusetts.
Letter from Thomas S. Kuhn to Donald Gillies, 2 April 1990. Thomas S. Kuhn Papers, MC 240, box 20. Massachusetts Institute of Technology, Institute Archives and Special Collections, Cambridge, Massachusetts.