Kuhnian Paradigms

Charlotte Blease[1] and Rachel Cooper[2]
[1] *University College Dublin, Ireland and* [2] *Lancaster University, U.K.*

In *The Structure of Scientific Revolutions* (Kuhn, 1970; hereafter referred to as *Structure*) Thomas Kuhn sketched an account of the history of science according to which scientists normally work within *paradigms*. Scientists who work within the same paradigm share certain tacit and explicit knowledge that enables them to conduct their research. For Kuhn, science is a fundamentally social activity: he argued that we can identify scientific paradigms by first identifying communities of like-minded researchers. Although communication among those who share a paradigm is straightforward, communication across paradigms is problematic (Kuhn's *incommensurability* thesis). Kuhn contended that paradigm change was inevitable. He understood science to be cyclical in nature. It is marked by a period of "normal" scientific research, which gives way to uncertainty (anomalies in the extant paradigm) and then revolution, with a new period of normal science emerging with the advent of a new "incommensurable" successor paradigm.

Kuhn on Paradigms and Normal Science

According to Kuhn, prior to a science developing there is a preparadigm period when individuals grapple over how best to understand some aspect of the world. During this period, numerous theories vie for acceptance. These preparadigmatic theories provide different answers to fundamental questions, such as, "What sorts of entities exist?" "How do these entities interact?" and "What techniques can we apply to understanding these entities?" Kuhn argues that, until scientists agree on such points, they cannot embark on what he termed "normal" scientific research. They will be too busy arguing about fundamentals to commence the detailed "puzzle-solving" activity that is the hallmark of mature science.

Once one preparadigmatic theory comes to be accepted by most of those working in a field, it starts to act as a "paradigm" for structuring further research. A sufficient number of individuals now agree as to what counts as excellent research in their field and accept that further research should be in the same mold. These individuals can now comprise a community that constitutes a scientific subdiscipline.

During "normal science," all those working in a subdiscipline work within a specific paradigm. The term "paradigm" deserves some clarification. In the first edition of *Structure* Kuhn neglected explicitly to define the term and spoke of "paradigms" in different senses. In response to critics, in a postscript to the second edition (Kuhn, 1970), Kuhn asserted that the term "paradigm" should be understood in two senses: the first, broader sense of "paradigm" he redefined as the "disciplinary matrix"; the second, narrower sense, he redefined as the interpretation of paradigms as "exemplars." The disciplinary matrix includes all that is shared by the members of a subdiscipline; including explicit and tacit beliefs, values, and the specialized techniques that scientists employ. Let's consider Newtonian mechanics as an example. Once Newton's *Principia* came to be recognized as a great work, Newton's work acted as an exemplary case, or paradigm in the narrow sense, that structured the work of scientists working on problems in mechanics. These scientists agreed that Newton's claims and ways of working were basically correct. The disciplinary matrix consists of the various components of this agreement. The scientists agree on "symbolic generalizations", such as $f = ma$. There is at least implicit scientific consensus regarding the

The Encyclopedia of Clinical Psychology, First Edition. Edited by Robin L. Cautin and Scott O. Lilienfeld.

DOI: 10.1002/9781118625392.wbecp281

nature of reality, for example, "forces exist." When solving problems these scientists follow Newton in the approximations, idealizations, and models that they use—thus, small pendulum bobs might be treated as point masses, and planes assumed to be frictionless. The scientists also agree on the characteristics of successful science. They value equations that can be judged to be simple and elegant in particular ways, they agree about the levels of accuracy that can be expected of experimental results, and so on. Modeling their work on Newton's physics leads to broad agreement as to what qualifies as an appropriate research question, what research methods are legitimate, and on what counts as a solution. Furthermore, all of those working in Newtonian mechanics have certain mathematical techniques at their disposal, and access to laboratories with standard equipment and instruments. As such, those working within the paradigm are able to perform and appreciate similar types of research.

Once established, the broad agreement that characterizes normal science is perpetuated by the somewhat authoritarian nature of scientific education and training. Students are trained to solve set problems as their teachers would. Achieving the "correct" answer, i.e. the answer that experts in the field accept, rather than originality, is rewarded. At the professional level, scientific communities are characterized by active networks of communication: journals, workshops, and conferences. In so far as dissenters tend to be excluded from such networks, consensus is encouraged.

Normal scientific research, guided by the disciplinary matrix and exemplars, is characterized not only by consensus, but also by complacency. Those who work within a paradigm take its basic adequacy for granted and the fundamental theoretical claims of the paradigm are not questioned. Assuming the correctness of the paradigm is generally a good strategy: without consensus on prior assumptions there would be endless philosophical debate of the sort that characterizes the pre-science period. Adherence to the paradigm frees up the individual scientists from having to wrestle with fundamental and intractable questions, and allows them to engage in "puzzle solving." Puzzle solving consists of activities such as extending a paradigm to new areas, or refining the methods and apparatus that a paradigm employs. For those working within the Newtonian paradigm, for example, puzzle-solving activities included showing how Newtonian mechanics could be extended to liquids, figuring out how Newton's laws could account for the observed motions of the planets, and finding ever more accurate figures for G, the gravitational constant. Puzzles come in various grades of difficulty. Some are comparatively easy and might be solved by a competent graduate student; solving others might require great labor and ingenuity.

Failure to solve a puzzle is generally taken to reflect badly on the individual scientist rather than on the paradigm. For the most part such blame allocation is justified, and solutions to most puzzles are eventually found. Still, sometimes the world fails to be as the paradigm dictates—scientists may observe phenomena for which a paradigm cannot account, or a puzzle may resist solution. Kuhn terms such difficulties "anomalies."

The usual initial response to an anomaly is to ignore it. Scientists are rewarded for solving puzzles, not for pointing out problems. Kuhn observes that no progress could be made if scientists sought to resolve every anomaly they observed. Most anomalies will be held in abeyance and resolved by some further articulation of the paradigm. However, some anomalies are particularly resistant to resolution and are simply too striking to ignore: these anomalies may undermine some fundamental aspect of the paradigm. Alternatively, there may be some pressing social or political need to resolve them.

Over time, Kuhn argues, anomalies build up, and eventually a scientific community may come to see itself as being in "crisis." The paradigm that had once commanded allegiance no longer seems viable and there will be little agreement on how to proceed. Some scientists will respond by conservatively defending the paradigm or attempting to modify it in minimal ways. Others become philosophically minded and will start to question what the central features of the paradigm are and how to interpret them. Some will seek a new paradigm—one that can resolve the anomalies that led the prevailing paradigm into crisis. Kuhn sees the diversity of individual responses to crisis as being functional and as ensuring that the best overall resolution will be found. In many cases the actions of conservative scientists will be adequate to resolve a crisis, and normal science within the existing paradigm will resume. In other cases, an embryonic paradigm may win enough recruits to take over the subdiscipline and a new period of normal science under the new paradigm will begin. According to Kuhn, the history of science thus consists of long periods of normal science that are punctured by (inevitable) periods of crisis and revolution.

Kuhn on Paradigm Shifts and Incommensurability

One of Kuhn's most controversial claims is that successive paradigms are "incommensurable." This means that there is no external objective measure by which paradigms can be compared, and as such there is no paradigm-independent means of determining which of two competing paradigms is better. According to Kuhn, incommensurability has three main sources: observations, the meaning of terms, and what counts as "good science" all vary according to each particular paradigm.

Observations are Paradigm-Dependent

Kuhn holds that observation is "theory-laden" —our perceptions of the world are shaped by the theories or beliefs that we hold. He cites a number of psychological experiments to support his claim that people tend to see what they expect. For example, Jerome Bruner and Leo Postman (Kuhn 1970, pp. 62–63) asked subjects to identify playing cards. The cards were briefly presented and some were anomalous, for example, a red five of diamonds. Subjects tended not to notice the anomalies and reported seeing normal cards. Kuhn suggests that expectations also affect scientific observations. He notes that Aristotelian astronomers, who believed that the heavens should be unchanging, and thus had no room for comets in their theory, failed to observe comets, whereas Chinese astronomers, who had no such preconceptions, saw them. Beliefs affect what one notices, and they also affect how one interprets that which is "observed." For example, a Copernican looking at the sunset sees the sun stay still and the horizon rise, whereas a Ptolemaic astronomer sees the horizon stay still while the sun falls behind it. The theory-ladenness of observation ensures that the world looks different to those who work within different paradigms.

Meaning is Paradigm-Dependent

Kuhn held that the meaning of theoretical terms is dependent on the paradigm. This follows from his view that the meaning of theoretical terms is given by their place in a theory. Such a view can be motivated if one considers how one learns the meaning of theoretical terms, such as "atom." In learning such terms one learns a network of claims, such as: molecules are made up of atoms; atoms are made of protons, neutrons, and electrons; and so on. If the meaning of theoretical terms is given by their place in a theory, when scientists have different theories, they will mean different things when they use a term like "atom." With this specific case, we can consider an extreme example to see how problems with communication may result. Some Ancient Greeks held that matter is made of atoms. But although today atoms are thought to have constituents, for the Ancient Greeks atoms were, by definition, indivisible. In an argument between an Ancient Greek and contemporary scientists over whether atoms can be split, the two sides would necessarily be talking at cross purposes.

The Standards for "Successful" Science are Paradigm-Dependent

Kuhn claimed that the importance attached to the various features of a paradigm is itself paradigm-dependent. Proponents of competing paradigms disagree over the nature and priorities of the problems to be solved. An additional issue for Kuhn is how paradigms are evaluated: Kuhn proposed that the following five desiderata are important for evaluating paradigms: accuracy, scope, simplicity, elegance, and fruitfulness. However, he argued that scientists may differ over how to rank these desiderata; and, even if there was agreement over the relative importance of each of these "virtues," scientists might disagree over how well the paradigm fulfilled them. For Kuhn, there is no paradigm-neutral "algorithm" for appraising paradigms.

The incommensurability of paradigms means that one paradigm cannot be proven better than another. Still, during revolutionary science, scientists decide which paradigm they adopt. On what grounds do they make their decision? Kuhn claimed that the reasons can be many and idiosyncratic. One paradigm might win recruits because it seems to offer great promise for explaining pressing problems; a second might attract adherents because of its mathematical tractability; a third might win converts for aesthetic reasons, it might seem simpler or more elegant than competitors. Kuhn even suggested that scientists may endorse a new paradigm for external reasons, such as a paradigm being more consistent with their religious beliefs. Whatever their motivation, if enough scientists convert to working in a new paradigm it can become the dominant way of working within a subdiscipline.

Almost always, even after a scientific revolution, some diehards—often the older members of the scientific community who have devoted their professional life to the paradigm—refuse to switch to the new paradigm. If the new paradigm succeeds in attracting most scientists, those who refuse to make the switch will become increasingly marginalized and the new paradigm will prevail.

Kuhn's thesis that paradigms are incommensurable creates a difficulty when it comes to accounting for scientific progress. If there is no paradigm-neutral standpoint from which one can fairly prove that one paradigm is better than another, paradigm shifts can only be seen as changes rather than progress. Many commentators have detected the seeds of relativism in Kuhn's account of science (see, for example, Barnes, 1982). Throughout *Structure*, Kuhn talks of scientists working in different paradigms as living "in different worlds." However, Kuhn never explicitly committed himself to a full-bloodied relativist reading of this phrase. The radical language tends to be hedged; a typical example is "the historian of science may be tempted to exclaim that when paradigms change, the world itself changes with them" (Kuhn, 1970, p. 111). Although Kuhn sought to distance himself from relativistic readings of his work, other commentators contended that his account was (in fact) committed to epistemic relativism.

On the question of scientific progress, Kuhn spent much time talking around the central issue of whether his account can provide an account of long-term scientific progress despite paradigm changes. Kuhn told us, in detail, how progress can emerge within each paradigm. During normal science, the paradigm under which scientists are working is extended and made more precise, and, judged by the standards of that paradigm, this is progress. Kuhn also explained how much long-term scientific progress may be more apparent than actual. After a scientific revolution, textbooks are rewritten and, as postrevolutionary textbooks are written by the victors, it is no surprise that the new paradigm is presented as if it were much better than its predecessor. On the question of whether there is any sense in which science as a whole progresses, Kuhn is—arguably—less convincing. Kuhn wished to contend that science is progressive but he also understood the growth of science as analogous to biological evolution. He argued that, in the same way that species become more complex without evolution having a goal, science too might evolve without it being necessary to think of it as aiming at producing truer theories. This evolutionary analogy is problematic because most biologists agree that evolution is not a "progressive" process. Many commentators have found Kuhn's account of progress unconvincing. One point is clear, however, by Kuhn's account the traditional image of science as continuous cumulative growth is mistaken—the "stockpiling" of facts can only occur during normal science.

Paradigms in Psychology

Kuhn took his examples mainly from physics or chemistry. He spoke of "science," but it is not clear whether he intended to include the human and social sciences. Although he noted that the question of whether psychology is a science is debated, he did not articulate his view on the question (Kuhn, 1970, p. 160). In the postscript, Kuhn specified, but only in passing, the social grouping that he took to make up the "scientific community" and here he wrote as if all science is natural science (Kuhn, 1970, p. 177). Kuhn himself trained as a physicist, and, plausibly, he spoke little of psychology because he preferred to write about what he knew. Clearly, *Structure* was intended primarily to provide an account of the history of physics and chemistry. Although Kuhn would presumably have been pleased if his model fit other disciplines, in so far as psychology differs from the natural sciences, Kuhn would probably have taken this to show only that his model is limited in its application, not that it is wrong.

Although Kuhn may not have been that interested in the question of whether his account of science applies to psychology, for us the question remains. If we are to apply Kuhn's model of science to psychology, including clinical psychology, one way in which psychology differs from physics and chemistry stands out. Whilst Kuhn held that normal science was marked by consensus, in psychology, and the "psych-sciences" more generally, competing schools of thought coexist. There is no consensus regarding the correct account of the causes of depression or schizophrenia, for example. Different schools seek to explain such conditions by means of evolutionary, developmental, behavioral, cognitive, psychoanalytic, biochemical, and other approaches.

How should such schools be understood? On Kuhn's account there are two options. The first is that multiple schools of thought coexist because psychology is not yet a mature science. On Kuhn's model, only in the mature sciences do all members of a subdiscipline work under one paradigm during normal science. During the earlier stage of "prescience" a number of competing schools coexist, with none having progressed to the stage during which it can convince a majority of practitioners of its correctness.

We suggest that this first option should be largely rejected. In Kuhnian prescience, each scientist must start by arguing about fundamentals and each practitioner has to justify his or her methodology and working assumptions. This sort of fundamental work is not currently necessary for many psychologists (though arguments about fundamental issues do still occur, for example, in psychotherapy). Although different schools of thought differ in their ways of working, a practitioner often has only to locate his or her work within a particular school (either explicitly by stating which approach he or she is adopting, or implicitly by publishing their papers in journals that are known to follow a particular way of working) and they can take much for granted. The sorts of detailed "puzzle-solving" activity that characterizes much current psychology are indicative of a mature science.

The second option is that psychology is a mature science, and the schools mature paradigms, but that a number of paradigms coexist because psychology differs from nineteenth- and early twentieth-century physics and chemistry in some way. We can think of two key differences that might lead psychology to be a multiparadigmatic science.

First, the complexity of the human mind may mean that it can be investigated from different but complementary perspectives. For example, those who adopt biological, social, behaviourist, or cognitive approaches to mental disorder all differ in their fundamental assumptions and ways of working. It might be argued that this proliferation of paradigms reflects the fact that each paradigm addresses different problems but that they are unified by the object under study. For example, in the study of depression, it might be that depression is triggered by specific social factors, such as unemployment. It may also be that, as some developmental psychologists argue, children raised in stressful situations are more prone to depression: the explanation here would be that certain cognitive processes are affected. An evolutionary story explaining how a tendency toward depression may serve some adaptive purpose might also be told. Neuroscientists will offer explanations at a lower level of analysis (perhaps, involving hormones or neurotransmitters). So there may be "explanatory purchase" to be had from multiple paradigms working on the same subject (e.g., depression) because they are concerned with different questions that may yield a unified account of the subject under study.

A second key difference between the sciences that Kuhn studied—nineteenth and early twentieth-century physics and chemistry—and contemporary psychology is the sheer numbers of scientists involved. In the sciences that Kuhn studied, a subdiscipline might be made up of 25 or so scientists (Kuhn, 1970, p. 181). If such a small group became split then there would be too few people committed to one approach for progress to be made. In contrast, thousands of scientists now work in contemporary psychology. This may mean that more than one paradigm can be supported at a time, as the different approaches can all find sufficient numbers of advocates to support their own conferences and journals and carry on in semi-independence from different approaches.

Recall that Kuhn held that those working under different paradigms should have problems communicating with each other. If psychology offers a genuine example of a multiparadigmatic science, are the communication problems that Kuhn would lead us to expect found? Cooper (2007, Chapter 6) addressed this question. Drawing on literature documenting the communication problems that emerge when psychologists who practice within different schools attempt to work together, she concluded that Kuhn was correct that communication across paradigms is often problematic but wrong that problems cannot be overcome.

Looking ahead it is likely that some aspects of clinical psychology will be overthrown and replaced (or enveloped) by other paradigms: it may be that some paradigms will be sloughed off and greater explanatory consilience occur within psychology. For example, many evolutionary and cognitive psychologists have argued that social psychology owes too much to folk psychological assumptions. This claim has clinical consequences: for example, if theories within personality psychology (such as the "big five" personality traits) are eliminated from psychological explanations this would have repercussions for diagnostic classifications within clinical psychology and psychiatry.

The Response to Kuhn's Paradigms

Kuhn himself styled the *Structure* as an "essay," which purported to sketch an account of the history of science, the details of which he promised to fill in at a later date. Although in later essays he returned to rework key ideas from the *Structure*, Kuhn's promise of a full-length book was never fulfilled. As a result, the *Structure* remains a text that is sketchy enough to bear multiple interpretations, and different commentators present us with quite different Kuhns.

Initial criticisms were compiled in *Criticism and the growth of knowledge* (Lakatos & Musgrave, 1970, based on a 1965 conference), which includes serious criticisms of Kuhn's views by the leading philosophers of science of the day. Reading that volume now, one is struck by how much better Kuhn has aged than some of his critics. In the mid 1960s, eminent philosophers of science condemned Kuhn for paying too much attention to the messy empirical details—of history, and of psychology—that play such a large role in the argument of the *Structure*. These critics conceived of the philosophy of science as a largely normative discipline that sought to describe how scientists should weigh evidence and decide among competing theories. On the basis of this conception, if the history of science showed that science proceeded along lines other than those pictured by philosophers, then this could be considered largely irrelevant—the history of science might show how science had progressed, but the job of philosophy was to show how it should continue. Such a response is unthinkable today. In the debate that saw a priori philosophy of science competing with an empirically grounded and historically informed philosophy of science, Kuhn and his followers won. Other criticisms in *Criticism and the growth of knowledge* concern the extent to which Kuhn's picture of science sketched theory change as governed by irrational processes and led to relativism. In response, in 1970 Kuhn published a lengthy postscript to the second edition of *Structure* that provided a clarification and a defense of his views. Here he rejected the charge of relativism and maintained that he was "a convinced believer in scientific progress" and scientific rationality, arguing that successive paradigms solved more problems than their predecessors (Kuhn, 1970, p. 206). This response set up a general pattern that would be followed throughout the rest of his life. Although others detected the seeds of relativism in his work, Kuhn consistently rejected such readings.

Kuhn's legacy was to make the history of science relevant to the philosophy of science, but there is now broad agreement that his picture of the history of science was oversimplified. Most commentators now agree that paradigm change need not be an "all-or-nothing" affair and that a continuum of modifications to the paradigm may be possible. In addition, the inevitability of revolutions in science has also been disputed. Still, despite these complications, Kuhn's basic picture, which sees normal science as working within paradigms, may be maintained.

The notion of incommensurability is more problematic and has been the focus of the most vehement philosophical criticisms of Kuhn's *Structure*. Incommensurability is a contested notion and some philosophers argue that Kuhn overstated the extent to which it is a problem. The term "incommensurability" originates in mathematics where it means "lack of common measure," but Kuhn came under attack for employing the term inconsistently. He variously contended, for example, that scientific revolutions are "those non-cumulative developmental episodes in which an older paradigm is replaced in whole or in part by an incompatible new one" (Kuhn, 1970, p. 92) and that "[the choice] between competing paradigms proves to be a choice between incompatible modes of community life" (Kuhn, 1970, p. 94). Comments such as these have given rise to a number of distinct, and more or less radical, readings of this key Kuhnian notion. Here we only discuss a few such interpretations.

At the radical end, Kuhn's claims about incommensurability between paradigms and his insistence that there is no straightforward algorithm for paradigm choice were influential in shaping the tenets of the Strong Programme in the sociology of science (e.g., Barnes, 1982). The Strong Programme construed scientific rationality as fundamentally historically and socially situated—such that decisions that seem rational to the members of one community seem unjustifiable to members of another. Indeed, Kuhn's views have been influential in giving rise to a tradition of "cultural relativism" within much of the humanities and social sciences (and even counterculture). On this view, sets of beliefs are not only shaped by one's culture but each is as epistemically legitimate as any other set of beliefs. Although passages within *Structure* seem to support such ideas, Kuhn was vociferous in claiming that such interpretations misunderstood his work.

At the conservative end, Wes Sharrock and Rupert Read (2002) claimed that the apparent problems of understanding "incommensurability" dissolve when we pay sufficient attention to how scientists actually work. They suggested that the problems of translating between paradigms run no deeper than those that arise when seeking to translate poetry between different languages. Although word-for-word translations will sometimes miss much, sensitivity to the nuances of what needs to be communicated can enable cross-cultural understanding. Sharrock and Read drew on a Wittgensteinian approach that views "deep" philosophical problems as the artificial constructs of bad philosophy—they only arise when we do not pay sufficient attention to how things actually are, and instead get carried away by insisting on how they ought to be.

From the 1980s, Kuhn himself gave his work a Kantian interpretation (Hoyningen-Huene, 1993, based on interviews with Kuhn). Kant distinguished between the noumenal realm (the so-called "world-in-itself" or "reality") and the phenomenal realm (our fixed categories for interpreting the world). Kuhn sought to use the distinction between the noumenal and the phenomenal to make sense of his claim that incommensurable paradigms could not be said to describe reality "as it is": rather, incommensurable lexicons constituted distinctive ways of viewing the world, none of which could be said to be "more true" than another. However, this Kantian interpretation of Kuhn's work has its own problems: Kant believed that we had fixed (a priori) categories for understanding the world; on Kuhn's view, our lexical categories are changeable.

Questions of incommensurability matter so much because they are tied to the question of whether scientific theory change is rational. The worry is that, if two paradigms are incommensurable, then there can be no possibility for rational disagreement between groups of scientists. Traditionally, it is assumed that to compare the merits of two competing theories, there must be some ahistorical, atheoretical algorithm by which to make such decisions. Kuhn consistently denied that *Structure* cast doubt on scientific rationality. In later work (Kuhn, 1977) he identified five values that formed the basis for choice between two paradigms: paradigms should be (a) empirically accurate; (b) consistent with other paradigms; (c) as simple as possible; (d) wide in scope and able to accommodate facts beyond those it initially explained; and (e) fruitful, thereby providing a successful framework for ongoing research. Kuhn held that there is no decisive choice about how to weigh these values when comparing paradigms. It is this latitude for disagreement that necessitates recourse to "persuasion"; however, he also disputed the claim that either side of the dispute is being "unscientific" (Kuhn, 1970, p. 200). Kuhn thus attempted to overhaul what is meant by rationality in science without thereby eliminating the notion of rationality.

In an interesting line of research, Kuhn's views about the structure of science have been taken up by researchers in the cognitive sciences. Kuhn's idea that much reasoning can be structured by exemplars rather than formal rules has been supported by research that shows that much of our cognition is based on prototypical perception, and perceived deviance from "prototypical cases." Cognitive scientists have further undertaken examination of Kuhn's work using connectionist models of cognition to assess the nature of incommensurability (Nersessian, 2003), as well as Kuhn's claims about the centrality of "paradigms" in scientific reasoning (Churchland, 1989).

SEE ALSO: Epistemology; Kuhn, Thomas S. (1922–96)

References

Barnes, B. (1982). *T. S. Kuhn and social science.* New York: Columbia University Press.

Bird, A. (2000). *Thomas Kuhn*. Princeton, NJ: Princeton University Press.

Churchland, P.M. (1989). *A neurocomputational perspective: The nature of mind and the structure of science*. Cambridge, MA: Bradford/The MIT Press.

Cooper, R. (2007). *Psychiatry and philosophy of science.* London: Acumen.
Hoyningen-Huene, P. (1993). *Reconstructing scientific revolutions: Thomas S. Kuhn's philosophy of science*. Chicago, IL: University of Chicago Press.
Kuhn, T. (1970). *The structure of scientific revolutions* (2nd ed.). Chicago, IL: University of Chicago Press.
Kuhn, T. (1977). *The essential tension*. Chicago, IL: University of Chicago Press.
Lakatos, I., & Musgrave, A. (Eds.). (1970). *Criticism and the growth of knowledge*. Cambridge: Cambridge University Press.
Nersessian, N. (2003). Kuhn, conceptual change, and cognitive science. In T. Nickles (Ed.), *Thomas Kuhn* (pp. 178–211). Cambridge: Cambridge University Press.
Sharrock, W., & Read, R. (2002). *Kuhn: Philosopher of scientific revolution.* Cambridge: Polity.

Further Reading

Thomas Kuhn (2011) *Stanford Encyclopedia of philosophy.* Retrieved from http://plato.stanford.edu/entries/thomas-kuhn/