**Influences on Kuhn and the Relationship between History and Philosophy of Science**

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**The Preface**

In the Preface to *The Structure of Scientific Revolutions* Kuhn (1922-1996) provides some useful background to understanding what he sought to accomplish in the book. Kuhn begins by explaining his own starting point for writing the book. He explains that his exposure to the history of science “radically undermined some of [his] basic conceptions about the nature of science” (Kuhn 1962/2012, xxxix). In fact, *The Structure of Scientific Revolutions* is a presentation of the new conception of science that he developed in light of this experience. Ultimately, as Kuhn makes clear, he is interested in developing a philosophy of science. In his words, in *Structure* he is addressing “the more philosophical concerns that had initially led [him] to history” (Kuhn 1962/2012, xxxix-xl). Indeed, his principal concerns are epistemological: understanding the relationship between data and theory, understanding how scientific knowledge grows, and understanding the nature of progress in science. The history of science functions as a source of data about science.

Kuhn explains that his approach to the history of science was influenced by the work of the French school, and especially Alexandre Koyré (1892-1964). From Koyré and others, Kuhn learned “to think scientifically in a period when the canons of scientific thought were very different from those current today” (Kuhn 1962/2012, xl). That is, Kuhn learned to see that earlier scientific practices were no less scientific than contemporary science. But Kuhn acknowledges that scientists of earlier times were engaging in radically different approaches to science than contemporary scientists. In order to see this, Kuhn believes that we must understand the scientific practices of previous times in their own terms. For example, we might fail to appreciate Aristotle’s physics and the depth of his understanding of the natural world if we do not recognize that he is concerned with a subject matter much broader than contemporary physics. Aristotle’s concern in his writings on physics was with change in general, which includes, for example, the change of an acorn into an oak tree, something that is no part of contemporary physics. To appreciate much of Aristotle’s work, we must be attuned to this and other significant differences.

Kuhn also remarks that in developing a philosophy of science he draws on research in psychology, specifically Jean Piaget’s (1896-1980) work on child development and gestalt psychology. From Piaget Kuhn gained insight into how a working scientist must learn to see the world differently when they confront anomalous experiences, experiences that do not fit their expectations, just as the young child must do. One must alter one’s beliefs, sometimes quite significantly, in order to make sense of the anomalous experience. And Kuhn appealed to popular gestalt images, like the duck/rabbit image, in order to illustrate how people can see different things from the same sense experience. This suggests to Kuhn that there is more to seeing than merely visually attending to the world. Rather, our expectations, theoretical expectations as well as unconscious expectations that may be a consequence of our cultural background, shape what we see. Perception does not give us an unmediated access to the world. Kuhn also mentions the influence of the linguist Benjamin Whorf (1897-1941), specifically Whorf’s “speculations about the effect of language on world view” (xl-xli). Kuhn believed that scientists were constrained, to some degree, in their thinking about the world by the language they bring to their experiences.

Finally, Kuhn acknowledges that his project has a sociological dimension, which he claims owes something to his reading of Ludwik Fleck’s (1896-1961) book, *Entstehung und Entwicklung einer wissenschaftlichen Tatsache*. Published in German in the 1930s, Fleck’s book was largely neglected until Kuhn drew attention to it. Kuhn was especially inspired by the suggestive remark that scientific facts *develop*. Regarding the sociological dimension of his project, Kuhn insists that in order to understand how science works so effectively we must understand “the sociology of the scientific community” (Kuhn 1962/2012, xli). Significantly, Kuhn takes the success of science for granted. That is what he seeks to explain. What is novel is his explanation. Unlike other philosophical accounts of science, Kuhn does not think that the success of science is a consequence of the methods employed in the sciences. The social structure of scientific communities provides important insights into how scientists have achieved the detailed and complex understanding of the natural world that they have developed.

Kuhn explains that a key turning point in the development of his views occurred when he spent a year at The Center for Advanced Studies in the Behavioral Sciences, at Stanford University. There, working along-side social scientists, he was struck by the fact that social scientists seemed to be in constant disagreement about the fundamentals of their field. There is no widely accepted theory in sociology, political science, or anthropology. This was in marked contrast to the natural sciences, where researchers generally agree about the fundamentals. Young physicists-in-training, whether in Germany, Poland, China, or Argentina, learn the same basic theories. Kuhn initially invoked the notion of a “paradigm” to explain this difference. According to Kuhn, whereas natural scientists are guided by paradigms, “universally recognized scientific achievements that … provide model problems and solutions to a community of practitioners” (Kuhn 1962/2012, xlii), social scientists lack paradigms in this sense.

The notion of a paradigm is a central concept in the book, and as the book progresses Kuhn uses the term in a variety of different ways and to refer to a variety of different things. Ultimately, this would be a focus of criticism of Kuhn’s view. Critics suggested that the notion of paradigm seemed to be doing a lot of conceptual work, and yet it was not clearly defined. Readers are forewarned that it is worth distinguishing between two principal senses in which the term is used. A paradigm is, as Kuhn says in the Preface, a concrete scientific accomplishment that guides scientists in their research, and that holds a scientific research community together. An example is Johannes Kepler’s (1571-1630) mathematical model of the orbit of Mars. According to Kepler, Mars moves in an elliptical orbit around the Sun, sweeping out equal areas in equal time, with the Sun located at one of the foci of the ellipse. This model was subsequently used to model the orbits of (i) other planets, (ii) the Moon, (ii) satellites of other planets, and even (iv) comets. Significantly, the solution to each of these problems cannot be solved by merely mechanically applying Kepler’s solution for the orbit of Mars. Rather, ingenuity is required to find solutions specific to each application. That is the primary notion of paradigm.

The second way in which Kuhn uses the term “paradigm” is equivalent to theory. Thus, when a scientific community replaces one theory by another, Kuhn describes it as a paradigm change. These are rather disruptive events in the development of science, as they involve a change in the basic ontology. For example, the physics associated with Galileo and Descartes was essentially a contact physics. Material objects move because they are moved by other material objects that are in direct contact with them. There is no action at a distance in such a worldview. In contrast, the physics of Newton also invokes attractive forces. Gravity is an attractive force between every piece of matter in the universe, and it acts across vast distances. For example, the Earth, via the gravitation attraction between it and the Moon, affects the motion of the Moon even though they are not in direct contact. The Newtonian worldview is, in some important sense, fundamentally different from the mechanistic world picture that informed the research of Galileo, Descartes, and their contemporaries. Every paradigm change involves a similar fundamental shift of this sort.

Kuhn remarks that most of the examples that he discusses in the book are drawn from the physical sciences, specifically, physics, chemistry, and astronomy. These are the sciences with which he was most familiar, as Kuhn’s own formal education was in physics, both at the bachelors level, and the Ph.D. level. But he does believe his theory applies to the other natural sciences as well, including the biological sciences and geology. Kuhn also notes some other limitations of the account he develops in *Structure*. For example, he does not discuss the influence of technology on the development of science. Nor does he discuss the influence of social factors that are generally regarded as external to science. He does not deny their importance, but his focus in *Structure* is on the internal dynamics of science. Indeed, Kuhn presents a theory of science according to which science progresses by its own internal dynamic. He claims that one of the key distinguishing features between the natural sciences, on the one hand, and the social sciences and humanities, on the other hand, is the fact that the former develop according to their own inner dynamic, shielded, in many respects, from the influence of broader social factors external to science.

Kuhn also mentions a number of people who influenced him as he wrote the book. It is worth briefly mentioning three of these people. The first is James B. Conant (1893-1978). Conant was the President of Harvard University, and the person who got Kuhn involved in teaching the history of science in the first place, when Kuhn was completing his Ph.D. in Physics. Conant was a chemist by training, and played a crucial role in the Manhattan Project, which led to the development of the atom bomb. After World War II, Conant was spearheading a program at Harvard to teach non-science students about science through a study of the history of science. Conant believed that one of the key lessons learned during World War II was that many of the social problems we face in the world today, that is, in the late 1940s, can only be solved with the help of science. Thus, Conant thought it was imperative for all citizens, and politicians especially, to have a basic understanding of how science works. Conant would later become the first American ambassador to West Germany (see Reisch 2019).

Kuhn also thanks Leonard Nash (1918-2013). Nash was also a chemist by training, and he co-taught the history of science course with Kuhn at Harvard as Kuhn was working on his book. Finally, he thanks Stanley Cavell (1926-2018). Cavell was a peer of Kuhn’s, and they worked together at the University of California, Berkeley. Cavell introduced Kuhn to Ludwig Wittgenstein’s (1889-1951) philosophy. Though Wittgenstein’s philosophy had little influence on Kuhn as he wrote *Structure*, after the publication of *Structure* Kuhn would explain his own views on concept application in Wittgensteinian terms. Further, Kuhn thought of the various applications of a paradigm to a set of scientific problems as having a family resemblance of the sort that Wittgenstein highlights in *Philosophical Investigations* in his discussion of games. Cavell is also responsible for Kuhn’s passing remarks about the context of discovery and context of justification at the end of the first chapter of *Structure* (see Hoyningen-Huene 2015, § 13.2.2).

**Chapter I: Introduction: A Role for History**

In Chapter I Kuhn describes the relevance of the history of science for his project. Further, he explains some recent developments in the historiography of science that have shaped his project.

Traditionally, Kuhn notes, histories of science focused on the finished products of scientific investigations. This has the effect of leading to a certain conception of science. A reader of such histories can get the sense that the great scientific discoveries of the past were inevitable. But Kuhn thinks that such histories are an impediment to understanding how scientific knowledge develops. There is a lot more uncertainty for the working scientist undertaking scientific research at the research frontier than these traditional histories would lead us to expect. Scientists at the research frontier do not know exactly where their research will lead them.

Kuhn compares the image of science one gets from these traditional histories of science to the image one gets of a foreign country from tourist brochures. Of course, the tourist brochures capture something about the culture and people of a country, but it is inevitably a very partial and somewhat misleading image. A tourist brochure of Germany, for example, might emphasize the castles along the Rhine, or the beerhalls in Bavaria. Though clearly a part of German culture, these are far removed from the everyday experience of most Germans.

The old history of science also leaves one with the impression that science grows by the gradual and constant addition of new discoveries. Kuhn, though, objects to this “development-by-accumulation” account of science (Kuhn 1962/2012, 2). That picture of science leads one to think that as science develops, we are leaving behind the “errors, superstitions, and myths” of the past (Kuhn 1962/2012, 2).

The new historiography of science aims to correct this image. It is more concerned with understanding the development of science as scientists experience it. The new historiography teaches us to study past scientific theories and practices with an eye to seeing the integrity of those practices. As Kuhn explains, historians have come to realize that “Aristotelian dynamics, phlogistic chemistry, or caloric thermodynamics … were, as a whole, neither less scientific nor more the product of human idiosyncrasy than those current today” (Kuhn 1962/2012, 2-3).

The key challenge is to understand these practices on their own terms, and see that Aristotle’s concerns, for example, were not the same as Galileo’s or Einstein’s, though we assume they were all concerned with the same subject, physics. Kuhn, though, insists that Aristotle’s dynamics is not a failed attempt to accomplish what Galileo accomplished. Rather, it is a fundamentally different approach to the study of nature. Aristotle and his contemporaries were occupied with very different sorts of physical problems than the problems that concerned Galileo and his contemporaries. In this respect, it is no surprise that he never reached Galileo’s conclusions.

Kuhn suggests that if one reflects on the history of science, one faces a dilemma. On the one hand, if one regarded these outdated theories as myths, and thus not fully scientific, then one must admit that “myths can be produced by the same sorts of methods and held for the same sorts of reasons that now lead to scientific knowledge” (Kuhn 1962/2012, 3). On the other hand, if one recognize these older theories which we no longer accept today, as scientific, then one must recognize that “science has included bodies of belief quite incompatible with the ones we hold today” (Kuhn 1962/2012, 3).

Kuhn suggests that the correct response to this dilemma is the latter choice. We must recognize that those older theories are scientific, even though the beliefs held then and the way scientific research was conducted then, are fundamentally different from the scientific beliefs and practices prevalent today. A consequence of this admission is that we must realize that science does not grow “as a process of accretion” (Kuhn 1962/2012, 3). That is, it is a mistake to see the growth of scientific knowledge as resulting from a strictly additive process, like the building of a great cathedral, brick by brick. Rather, the development of a science, Kuhn insists, involves significant disruptions and discontinuities. Recognizing this is the only way to make sense of the radically different ways in which science has been conducted in the past.

Kuhn suggests that this new way of thinking about science that has emerged from the history of science will lead us to ask new questions about science. In reflecting on these new questions, Kuhn notes that historians of science have begun to “trace different, and often less than cumulative, developmental lines for the sciences” (Kuhn 1962/2012, 3). Indeed, this is a key insight that Kuhn wants to emphasize throughout the book. In fact, it was this realization that led him to want to write the book in the first place. He wanted to make sense of the periodic revolutionary paradigm changes in the development of science. He wanted to get at the structure of scientific revolutions.

Kuhn also notes that resolving the dilemma posed by the new historiography in the manner that he suggests has led historians to “attempt to display the historical integrity of … [older] science in its own terms” (Kuhn 1962/2012, 3). Historians are now more concerned than ever, for example, with making sense of what Joseph Priestley and his contemporaries thought about the world when phlogiston was a central concept to their understanding of chemical processes. The fact that we no longer believe that there is such a substance as phlogiston does not mean that their understanding of the world was irrational. Historians aim to make sense of their laboratory experiments, even those that seem, initially, quite bizarre or fruitless, given our modern understanding of the chemical world.

Kuhn then notes a number of important philosophical insights that emerge when we embrace this new perspective on science.

First, he insists that we will realize that the methodological directives that guide scientists are insufficient “to dictate a unique substantive conclusion to many sorts of scientific questions” (Kuhn 1962/2012, 4). In fact, Kuhn also claims, “observation and experience … cannot alone determine a particular body of [scientific] belief” (Kuhn 1962/2012, 4). As a consequence, confronted with a choice between competing theories, and (i) relying on the typical methodological directives that scientists learn in their training, and (ii) attending to the same body of evidence, there are times when a unique answer is not dictated. This is referred to as “Kuhn-underdetermination” (see Carrier 2011, 202-203, Ftnt. 5). In situations such as these, scientists will often appeal to other considerations in making a choice between the competing theories, for example, their relative simplicity, or their relative scope.

Importantly, because of Kuhn-underdetermination, rational scientists can disagree. So we should not regard disagreements between scientists about competing hypotheses or theories as grounds for concern. Such disagreements do not suggest (i) that science is irrational, (ii) that any theory is as good as any other, or (iii) that scientists can never determine which of two competing theories is superior. Kuhn is not a relativist in this sense.

One reason Kuhn emphasizes this point is that he wants us to see that scientific research is more complex and messier than the traditional logical analyses of science make it seem. Kuhn suggests that scientists who accept competing theories in a field have “incommensurable ways of seeing the world and of practicing science in it” (Kuhn 1962/2012, 4). That is, they have fundamentally different ways of doing science, and are guided by fundamentally different pictures of the world. And these differences can be barriers to resolving disputes in science.

Significantly, though, Kuhn insists that scientists are constrained in their theorizing by observations and evidence. In his words, “observation and experience can and must drastically restrict the range of admissible scientific beliefs, else there would be no science” (Kuhn 1962/2012, 4). Science is, after all, about a world that is in important respects independent of us and our thoughts about it (see Wray 2011, Chapter 8). This insistence on accounting for our observations and experiences underscores the claim that Kuhn’s view is not a form of relativism according to which any belief system or theory is as good as any other.

Kuhn also notes that what beliefs and assumptions a scientist brings to inquiry is somewhat arbitrary, and a result of “personal and historical accident” (Kuhn 1962/2012, 4). This may seem to threaten the objectivity of science, but that is not Kuhn’s point here. He is attempting to make sense of the fact that people can and have conducted science in very different ways throughout history. One of the key things that distinguishes Aristotle’s physics from Galileo’s are the assumptions each brings to their inquiry. And some of these assumptions are a consequence of the time in which each lived and worked. But Kuhn also rightly notes that science has to start somewhere. One needs to bring some sorts of assumptions to bear on their research, if they are to make any advances. So the fact that scientists begin with assumptions and beliefs that are, to some degree, a result of personal and historical accident, does not undermine the integrity of science.

These assumptions that scientists bring to inquiry play a crucial role in the activity that Kuhn characterizes as normal science. As Kuhn explains, normal science involves a “strenuous and devoted attempt to force nature into the conceptual boxes supplied by professional education” (Kuhn 1962/2012, 5). Kuhn is emphasizing the fact that, generally, the working scientist assumes that the theory she is working with is more or less correct, and her job is to resolve an unsolved research problem using the conceptual resources of the accepted theories, the theories that she was taught during her education.

So firm is the scientist’s commitment to the accepted theory that when one encounters phenomena that do not fit with the accepted theory, that is, anomalies, they are often initially suppressed or set aside (Kuhn 1962/2012, 5). Ultimately, though, Kuhn believes that anomalies will threaten to “subvert the existing tradition of scientific practice” (Kuhn 1962/2012, 6). When this occurs, then a field will undergo a revolutionary change of theory. Kuhn is here emphasizing the internal dynamics of scientific change.

Kuhn calls this phase of transition between paradigms extraordinary science, in contrast to normal science, the sort of science for which scientific education prepares a student. Whereas normal science is a tradition-bound activity, extraordinary science is tradition shattering. Significantly, Kuhn wants us to see scientific practices as like other cultural practices; they are largely tradition guided. This is part of the sociological dimension of his account of science (see Wray 2011, Chapter 10). To understand scientific change, we will need to understand (i) how a scientific tradition is sustained over time, (ii) how such a tradition is undermined, and (iii) how it is ultimately replaced by a new scientific tradition.

In an effort to elucidate the notion of a paradigm change, Kuhn cites a number of familiar examples of scientific revolutions:

1. the Copernican Revolution in astronomy that occurred in the 16th and 17th Centuries,
2. the Newtonian Revolution in physics in the 17th Century,
3. the revolution in Chemistry in the late-18th Century associated with Lavoisier, and
4. the revolution in early 20th Century physics associated with Einstein.

Kuhn claims that each of these episodes involved the replacement of a long-accepted theory with a new theory, one that is incompatible with the theory it replaces. This aspect of the development of science is very important, according to Kuhn. It underscores his claim that the development of science is not cumulative. Rather, there are significant disruptive periods, where scientists develop radically new conceptual frameworks. That is, they come to develop a new understanding of science and the world radically at odds with the understanding that was previously widely held.

In fact, it is not only the theory that changes when there is a revolution in science. Kuhn claims that the problems that concern scientists also change. Further, he claims that the standards change. He even claims that there is a transformation of the “scientific imagination” (Kuhn 1962/2012, 6). Significantly, Kuhn believes that scientific research is a very creative activity, requiring imagination on the part of the scientist. Given the many facets of these changes in science, it is no wonder that Kuhn describes them as revolutions.

Kuhn also argues that in addition to these scientific revolutions that most people are familiar with, there are many smaller revolutions in science, revolutions that affect only a relatively small group of scientists (Kuhn 1962/2012, 7). Despite the restricted scope of the group affected by these smaller revolutions, sometimes affecting only 100 or so scientists, Kuhn insists that these revolutions are experienced as very disruptive to those affected by them. A scientific revolution is not characterized by the size of the group affected. Rather, it is characterized by the nature of the changes involved.

Kuhn notes that scientific revolutions are “seldom completed by a single man and never overnight” (Kuhn 1962/2012, 7). This is an important remark to keep in mind when reading the book. Kuhn wants readers to realize that scientific revolutions are often quite drawn-out and take time to run their course. In fact, the Copernican Revolution in astronomy, which began with the publication of Copernicus’ book in 1543, was not consummated until the 1620s or 1630s. Because they are extended across time, scientific revolutions can also be challenging to date precisely (Kuhn 1962/2012, 7).

Another theme Kuhn highlights is the intimate connection between theory and fact. According to traditional philosophies of science, fact and theory are quite distinct. In principle, the facts can be established prior to theorizing. They are objective and are not subject to change over time. On this view, facts form the foundation for our scientific knowledge. And the purpose of theory is to bring order to the facts. Kuhn rejects this view of the relationship between fact and theory. According to Kuhn, the theory a scientist accepts affects what she regards as a fact. Consequently, facts cannot provide a theory-neutral or theory-independent check on the theories we develop or accept. This is often referred to as “the theory-ladenness of observation”. Sometimes it is suggested that our theories contaminate the facts. But this way of expressing the point is overly skeptical, and misses a key point Kuhn aims to establish. He is not saying that the theories we accept *distort* our perception, as the notion of contamination implies. Rather, Kuhn is claiming that in order to effectively perceive facts we need to bring to the experience various theoretical assumptions. There is no theory-neutral ground on which to begin inquiry.

The importance of the theory-ladenness of observation, for Kuhn, is that it helps explain why scientists face many challenges when they are in the process of making a novel discovery, a discovery that is wholly unanticipated by the accepted theories. Because scientists’ expectations, and thus also their observations, are shaped by the theory or theories they accept, they can sometimes either completely overlook or disregard phenomena that are anomalous.

Still, Kuhn insists that the theory-ladenness of observation does not prevent scientists from changing their beliefs, even in fundamental ways. Kuhn believes that if anomalies are persistent, ultimately scientists rethink their theoretical commitments.

Kuhn then explains the tasks of later chapters. Chapter XI examines the role textbooks play in shaping the way scientists see and experience the world. It is through textbooks that young scientists-in-training are introduced to the accepted theories. He also explains why scientific revolutions are often unnoticed, at least, after the fact. They are, he claims, in some sense, systematically covered over, but not for any sort of devious reasons. Chapter XII is concerned with explaining the processes by which one theory comes to replace another. In Chapter XIII Kuhn examines the nature of scientific progress. Motivating Kuhn is the realization that scientific progress must be something very different than many assume, if he is correct that the development of science is not a strictly cumulative process.

Kuhn ends the first Chapter noting some of the apparently paradoxical aspects of his approach, aspects that are apt to cause resistance to his view. For example, he notes that there is likely to be some resistance to his constructing a philosophy of science based on a study of the history of science. Philosophy of science and epistemology are normative disciplines, aiming to tell us how scientists should behave or how scientific inquiry ought to be conducted. Yet Kuhn claims to be building his philosophical theory of science on a study of the history of science, and sociological and psychological studies, which are descriptive disciplines. One might think that drawing normative conclusions from descriptive data would be unwarranted. But Kuhn insists that “at least a few of my conclusions belong traditionally to logic or epistemology” (Kuhn 1962/2012, 8).

Kuhn acknowledges that the distinction between the descriptive and the normative, as well as the related distinction between the context of discovery and the context of justification, has played a central role in philosophy of science. Here he has in mind the philosophy of science as developed by the Vienna Circle Logical Positivists and Karl Popper. Kuhn insists that because these distinctions are tied to a particular theory of science, specifically, these logical analyses of science, they should be scrutinized empirically. In this respect, Kuhn is suggesting that our evaluation of our philosophies of science should proceed in a manner similar to the way scientists proceed in their evaluation of competing scientific theories. None of our assumptions, no matter how fundamental they seem, are immune from critical scrutiny. In our efforts to develop a compelling philosophy of science, we may even have to rethink our understanding of the relationship between descriptive claims about science and normative claims about science.

Kuhn ends Chapter I with a rhetorical question: “How could history of science fail to be a source of phenomena to which theories about knowledge may legitimately be asked to apply?” (Kuhn 1962/2012, 9) Kuhn’s point here is that we should expect our philosophy of science to apply to science as it is really practiced by scientists. And if it does not, he argues, we have grounds for doubting that our theory is correct. In this way, he feels he has addressed the methodological concern regarding his novel approach to developing a philosophy of science.

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