From: P. Godfrey-Smith, Theory and Reality.
U of C Press, Chicago, 2003.

5

Kuhn and Normal Science

5.1 "The Paradigm Has Shifted"

In this chapter we encounter the most famous book about science written during the twentieth century—*The Structure of Scientific Revolutions,* by Thomas Kuhn. Kuhn's book was first published in 1962, and its impact was enormous. Just about everything written about science by philosophers, historians, and sociologists since then has been influenced by it. The book has also been hotly debated by scientists themselves. But *Structure* (as the book is known) has not only influenced these academic disciplines; many of Kuhn's ideas and terms have made their way into areas like politics and business as well.

A common way of describing the importance of Kuhn's book is to say that he shattered traditional myths about science, especially empiricist myths. Kuhn showed, on this view, that actual scientific behavior has little to do with traditional philosophical theories of rationality and knowledge.

There is some truth in this interpretation, but it is often greatly exaggerated. Kuhn spent much of his time after *Structure* trying to distance himself from some of the radical views of science that came after him, even though he was revered by the radicals. And the connection between Kuhn's views and logical empiricism is actually quite complicated. For example, it comes as a surprise to many to learn that Kuhn's book was published in a series organized and edited *by* the logical empiricists; *Structure* was published as part of their International Encyclopedia of Unified Science series. As a matter of historical fact, though, there is no denying that this was something of a "Trojan horse" situation. Logical empiricism was widely perceived as being seriously damaged by Kuhn.

I said above that some of Kuhn's ideas and terms have made their way into areas far from the philosophy of science. The best example is Kuhn's use of the term "paradigm." Here is a passage from Tom Wolfe's 1998 novel, *A Man in Full.* Charlie Croker, a real estate developer who has debt problems, is talking with his financial adviser, Wismer ("Wiz") Stroock.

> "I'm afraid that's a sunk cost, Charlie," said Wismer Stroock. "At this point the whole paradigm has shifted."
>
> Charlie started to remonstrate. Most of the Wiz's lingo he could put up with, even a "sunk cost." But this word "paradigm" absolutely drove him up the wall, so much so that he had complained to the Wiz about it. The damned word meant nothing at all, near as he could make out, and yet it was always "shifting," whatever it was. In fact, that was the only thing the "paradigm" ever seemed to do. It only shifted. But he didn't have enough energy for another discussion with Wismer Stroock about technogeekspeak. So all he said was:
>
> "OK, the paradigm has shifted. Which means what?" (71)

This sort of talk derives completely from Kuhn. But what *is* a paradigm? The short answer is that a paradigm, in Kuhn's theory, is a whole way of doing science, in some particular field. It is a package of claims about the world, methods for gathering and analyzing data, and habits of scientific thought and action. In Kuhn's theory of science, the big changes in how scientists see the world—the "revolutions" that science undergoes every now and then—occur when one paradigm replaces another. Kuhn argued that observational data and logic alone cannot force scientists to move from one paradigm to another, because different paradigms often include within them different rules for treating data and assessing theories. Some people have interpreted Kuhn as claiming that changes between paradigms are completely irrational, but Kuhn definitely did not believe that. Instead, Kuhn had a complicated and subtle view about the roles of observation and logic in scientific change.

In a passage like the Tom Wolfe one above, "paradigm" is used in a looser way derived from its role in Kuhn's theory of science. A paradigm in this sense means something like a way of seeing the world and interacting with it.

Kuhn did not invent the word "paradigm." It was an established term, which meant (roughly) an illustrative example of something, on which other cases can be modeled. Kuhn discusses this original meaning in *Structure* (1996, 23). And although Kuhn's theory is the inspiration for all the talk about paradigm shifts that one hears, Kuhn only occasionally used the phrase "paradigm shift." More often he talked about paradigms changing or being replaced. Whichever term one uses, though, Kuhn's theory was itself something like a paradigm change in the history and philosophy of science. Nothing has been the same since.

5.2 Paradigms: A Closer Look

A moment ago I said that a paradigm in Kuhn's theory is a package of claims about the world, methods for gathering and analyzing data, and habits of scientific thought and action. However, it is more accurate to say that this is *one* sense in which Kuhn used the term "paradigm." In *Structure,* the term is used in several different ways; one critic counted as many as twenty-one different senses (Masterman 1970). Kuhn later agreed that he had used the word ambiguously, and throughout his career he kept fine-tuning this and other key concepts. To keep things simple, though, in this book I will recognize two different senses of the term "paradigm."

The first sense, which I will call the *broad* sense, is the one I described above. Here, a paradigm is a package of ideas and methods, which, when combined, make up both a view of the world and a way of doing science. When I say "paradigm" in this book without adding "broad" or "narrow," I mean this broad sense. But there is also a narrower sense. According to Kuhn, one key *part* of a paradigm in the broad sense is a specific *achievement,* or an *exemplar.* This achievement might be a strikingly successful experiment, such as Mendel's experiments with peas, which eventually became the basis of modern genetics. It might be the formulation of a set of equations or laws, such as Newton's laws of motion or Maxwell's equations describing electromagnetism. Whatever it is, this achievement is a source of inspiration to others; it suggests a way to investigate the world. Kuhn often used the term "paradigm" just for a specific achievement of this kind. I will call these achievements paradigms in the *narrow* sense. So paradigms in the broad sense (whole ways of doing science) include within them paradigms in the narrow sense (examples that serve as models, inspiring and directing further work). Kuhn himself did not use this "narrow/broad" terminology, but it is helpful. When Kuhn first introduced the term "paradigm" in *Structure,* he defined it in the narrower sense. But in much of his writing, and in most of the work written after *Structure* using the term, the broad sense is intended.

Kuhn used the phrase "normal science" for scientific work that occurs within the framework provided by a paradigm. A key feature of normal science is that it is *well organized.* Scientists doing normal science tend to agree on which problems are important, on how to approach these problems, and on how to assess possible solutions. They also agree on what the world is like, at least in broad outlines. A scientific revolution occurs when one paradigm breaks down and is replaced by another.

This initial sketch is enough for us to go straight to some central points about the message of Kuhn's book.

The first point can be approached via a contrast with Popper. For Popper, science is characterized by *permanent openness*, a permanent and all-encompassing *critical stance*, even with respect to the fundamental ideas in a field. Other empiricist views will differ on the details here, but the idea of science as featuring permanent openness to criticism and testing is common to many versions of empiricism. Kuhn disagreed. He argued that it is *false* that science exhibits a permanent openness to the testing of fundamental ideas. Not only that, but science would be *worse off* if it had the kind of openness that philosophers have treasured.

The second point concerns scientific change. Here again a contrast with Popper is convenient. For Popper, all science proceeds via a single process, the process of conjecture and refutation. There can still be episodes called "revolutions" in such a view, but revolutions are just different in degree from what goes on the rest of the time; they involve bigger conjectures and more dramatic refutations. For Kuhn, there are two distinct kinds of scientific change: change within normal science, and revolutionary science. (These are bridged by "crisis science," a period of unstable stasis.) These two kinds of change have very different epistemological features; when we try to apply concepts such as *justification, rationality,* and *progress* to science, according to Kuhn we find that normal and revolutionary science have to be described very differently. Within normal science, there are clear and agreed-upon standards for the justification of arguments; within revolutionary science there are not. Within normal science there is clear progress; within revolutionary science it is very hard to tell (and it is hard to even interpret the question). Because revolutions are essential to science, the task of describing rationality and progress in science *as a whole* becomes very complicated.

So Kuhn is first making some claims about how science actually operates, and then drawing philosophical conclusions from those claims. Even if we leave aside the details of Kuhn's claims, this strategy of argument was controversial and influential. Kuhn addressed philosophical questions about reason and evidence via an examination of history. As we saw in chapter 2, the logical empiricists made a sharp distinction between questions about the history and psychology of science, on the one hand, and questions about evidence and justification, on the other. Kuhn was deliberately mixing together things that the logical empiricists had insisted should be kept apart. One of the reasons that Kuhn was interpreted as a "destroyer" of logical empiricism was that Kuhn's work seemed to show how interesting it is to connect philosophical questions about science with questions about the history of science. Kuhn seemed to open up an exciting new way of approaching a set of problems that the logical empiricists

were approaching in a very abstract manner. And although I emphasized in earlier chapters some appealing parts of the logical empiricist approach, I agree with Kuhn about the useful role of history in addressing philosophical questions about science.

Before we go deeper into the details of Kuhn's view, there is one other preliminary point to make. This has to do with a question that one should always ask when thinking about Kuhn's theory and other theories like it. The question is, Which parts of the theory are just *descriptive*, and which are *normative*? That is, when is Kuhn just making a claim about how things *are*, and when is he making a value judgment, saying how they *should* be? Kuhn certainly accepted that he was making some normative claims (1996, 8). Some commentators were critical of Kuhn, however, because it's often hard to tell when he is just "saying how things are" and when he is making claims about good and bad science. My own interpretation of Kuhn stresses the normative element in his work. I think Kuhn had a very definite picture of how science should work and of what can cause harm to science. In fact, it is here that we find what I regard as the most fascinating feature of *The Structure of Scientific Revolutions*. This is the relationship between

1. Kuhn's constant emphasis on the arbitrary, personal nature of factors often influencing scientific decisions, the rigidity of scientific indoctrination of students, the "conceptual boxes" that nature gets forced into by scientists . . . , and
2. Kuhn's suggestion that these features are actually the *key to science's success*—without them, there is no way for scientific research to proceed as effectively as it does.

Kuhn is saying that without the factors referred to in (1), we would not have the most valuable and impressive features of science. But how can this be? How can features that look like failings and flaws actually *help* science? How can it *help* science for decisions to be made on the basis of anything other than what the data say? To answer these questions, we need to look more closely at the details of Kuhn's story about scientific change.

5.3 Normal Science

Normal science is work inspired by a striking achievement that provides a basis for further work (a paradigm in the narrow sense). Kuhn does not think that all science needs a paradigm. Each scientific field starts out in a state of "pre-paradigm science." During this pre-paradigm state, scientific work can go on, but it is not well organized and usually not very effective.

At some point, however, some striking piece of work appears. This achievement is taken to provide insight into the workings of some part of the world, and it supplies a model for further investigation. This achievement is so impressive that a tradition of further work starts to grow up around it. The field has its first paradigm.

What are some examples of paradigms? Kuhn gave examples from physics and chemistry, such as Newton's and Einstein's paradigms. Here I will mention two cases from other fields. Within psychology around the middle of the twentieth century, a great deal of work was based upon the behaviorist approach of B. F. Skinner. Two basic principles of Skinnerian behaviorism are (1) that learning is basically the same in humans, rats, pigeons, and other animals and (2) that learning proceeds by reinforcement—behaviors followed by good consequences tend to be repeated, while behaviors followed by bad consequences tend not to be repeated. Along with these principles, the Skinnerian paradigm included a set of experimental tools, such as standardized boxes in which pigeons made choices in response to stimuli by pecking lighted keys. It also included statistical techniques used to analyze data and various habits and skills for working out relevant and interesting experiments.

Here is an example from biology. Modern molecular genetics is based on a set of principles such as the following: (1) genes are made of DNA (in all organisms except some viruses, which have RNA genes), (2) genes have their effects by producing protein molecules and regulating other genes, and (3) nucleic acids (DNA and RNA) specify the structure of proteins but not vice versa. This last principle is often called "the central dogma." Along with these theoretical claims, molecular genetics includes a set of techniques for sequencing genes, for producing and studying mutations, for analyzing the similarity of different genes, and so on.

For Kuhn, a scientific field usually has only one paradigm guiding it at any particular time. Sometimes Kuhn wrote as if this were true by definition—a *field* being defined as an area of scientific investigation unified by a single paradigm. This led him to divide some scientific fields up more finely than is usual. Kuhn does allow that occasionally a field can be governed by several related paradigms, but this is rare. In general, a key part of Kuhn's theory is the principle *one paradigm per field per time.*

A paradigm's role is to *organize* scientific work; the paradigm coordinates the work of individuals into an efficient collective enterprise. A key feature that distinguishes normal science from other kinds of science for Kuhn is the absence of debate about fundamentals. Because scientists doing normal science agree on these fundamentals, they do not waste their time arguing about the most basic issues that arise in their field. Once biologists

agree that genes are made of DNA, they can focus and coordinate their work on how specific genes affect the characteristics of plants and animals. Once chemists agree that understanding chemical bonding is understanding the interactions between the outer layers of electrons within different atoms, they can work together to investigate when and how particular reactions will occur. Kuhn places great emphasis on this "consensus-forging" role of paradigms. He argues that without it, there is no chance for scientists to achieve a really detailed and deep understanding of phenomena. Detailed work and revealing discoveries require cooperation and consensus. Cooperation and consensus require *closing off debate about fundamentals.*

As usual, we should be careful to distinguish between the descriptive and the normative here. Kuhn certainly claims that normal science does close off debate about fundamentals. But does he go beyond that and claim this is something that normal science *should* do? I think it is clear that he does (see Kuhn 1996, 24–25, 65), but these issues are controversial.

If Kuhn does make a normative claim here, then we see an important contrast with Popper. Although Popper can certainly allow that not everything can be criticized at once within science, Popper's view does hold that a good scientist is permanently open-minded with respect to *all* issues in the field in which he or she is working, even the very basic issues. Any "closing off" of debate is bad news according to Popper. Popper criticized Kuhn explicitly on this point; Popper said that although "normal science" of Kuhn's kind does occur, it is a bad thing that it does (1970).

What is the work of a good normal scientist like? Kuhn describes much of the work done in normal science as "puzzle-solving." The normal scientist tries to use the tools and concepts provided by the paradigm to describe, model, or create new phenomena. The "puzzle" is trying to get a new case to fit smoothly into the framework provided by the paradigm. Kuhn used the term "puzzle" rather than "problem" for a reason. A puzzle is something we have not yet solved but which we think does have a solution. A problem might, for all we know, have no solution. Normal science tries to apply the concepts provided by a paradigm to issues that the paradigm suggests *should* be soluble. Part of the guidance provided by a paradigm is guiding the selection of good puzzles.

The term "puzzle" also seems to suggest that the work is in some way insignificant or trivial. Here again, Kuhn intends to convey a precise message with the term. A normal scientist *does,* Kuhn thinks, spend a lot of time on topics that look insignificant from the outside. (He even uses the term "minuscule" [1996, 24].) But it is this close attention to detail—which only the well-organized machine of normal science makes possible—that is able to reveal *deep* new facts about the world. I think Kuhn felt a kind of awe at the ability of normal science to home in on topics and phenomena that look insignificant from outside but which turn out eventually to have huge importance. And although the normal scientist is not *trying* to find phenomena that lead to paradigm change—far from it!—these detailed discoveries often contain the seeds of large-scale change and the destruction of the paradigm that produced them.

5.4 Anomaly and Crisis

I said that a central feature of normal science, for Kuhn, is that the fundamental ideas associated with a paradigm are not debated. Fundamental principles are insulated from refutation. Normal scientists spend their time trying to extend the paradigm, theoretically and experimentally, to deal with new cases. When there is a failure to get the results expected, the good normal scientist reacts by trying to work out what mistake she or he has made. The proverb "only a poor workman blames his tools" applies. The normal scientist should take failure as a challenge.

Kuhn accepts that theories are sometimes refuted by observation; within normal science, hypotheses are refuted (and confirmed) all the time. The paradigm supplies principles for making these decisions. But throwing out an entire paradigm is much more difficult. According to Kuhn, the rejection of a paradigm happens only when (1) a critical mass of anomalies has arisen and (2) a rival paradigm has appeared. For now we will look just at the first of these—the accumulation of a critical mass of anomalies.

An "anomaly" for Kuhn is a puzzle that has resisted solution. Kuhn holds that all paradigms face some anomalies at any given time. As long as there are not too many of them, normal science proceeds as usual and scientists regard them as a challenge. But the anomalies tend to accumulate. Sometimes a single one becomes particularly prominent, by resisting the efforts of the best workers in the field. Eventually, according to Kuhn, the scientists start to lose faith in their paradigm. The result is a *crisis*.

Crisis science, for Kuhn, is a special period when an existing paradigm has lost the ability to inspire and guide scientists, but when no new paradigm has emerged to get the field back on track. The transition to a crisis is almost like a phase transition, like the change of a substance from solid to liquid during melting. For whatever reason, the scientists in a field lose their confidence in the paradigm. As a consequence, the most fundamental issues are back on the table for debate. Amusingly, Kuhn even suggests that during crises scientists tend to suddenly become interested in philosophy, a field that he sees as quite useless for normal science.

I used the term "critical mass" of anomalies to describe the trigger for

a crisis. This atomic-age metaphor is appropriate in several ways. In particular, I use it here to suggest that Kuhn sees the breakdown of a paradigm as something that is part of the "proper functioning" of science, though it does not feel that way to the scientists involved. Normal science is structured in a way that makes its own destruction inevitable, but only in response to the right stimulus. The "right stimulus" is the appearance of problems that are deep rather than superficial, problems that reveal a real inadequacy in the paradigm. Because normal scientists will tolerate a good deal of temporary trouble without abandoning normal science—they will blame the failure on themselves, at least for a while—a paradigm does not break down easily. But when the right stimulus comes, the paradigm will break down. In this way, a paradigm is like a well-shielded and well-designed bomb. A bomb is supposed to blow up; that is its function. But a bomb is not supposed to blow up at any old time; it's supposed to blow up in very specific circumstances. A well-designed bomb will be shielded from minor buffets. Only a very specific stimulus will trigger the explosion.

Some might find this militaristic analogy unpleasant, but I think it captures a lot of what Kuhn says. Kuhn's story is guided by his claim that all paradigms constantly encounter anomalies. For a Popperian view, or for other simpler forms of empiricism, these anomalies should count as "refutations" of the theory. But Kuhn thinks that science *does* not treat these constantly arising anomalies as refutations, and also that it *should* not. If scientists dropped their paradigms every time a problem arose, they would never get anything done.

Much of the secret of science, for Kuhn, is the remarkable balance it manages to strike between being *too* resistant to change in basic ideas, and not being resistant enough. If the simplest form of empiricist thinking prevailed, people would throw ideas away too quickly when unexpected observations appeared, and chaos would result. Ideas need some protection, or they can never be properly developed. But if science was completely unresponsive to empirical failures, conceptual advance would grind to a halt. For Kuhn, science seems to get the balance just right. And this delicate balance is not something we can describe in terms of a set of explicit rules. It exists implicitly in the social structures and transmitted traditions of scientific behavior, and in the quirks of the scientific mind.

These ideas about the balance that makes science work are an important challenge to empiricism, at least in its simpler forms. The idea that a willingness to revise ideas in response to observation *can go too far* is unexpected from the point of view of empiricist philosophy. And Kuhn supported this claim with a mass of evidence from the history of science.

So far we have gone from pre-paradigm science, through normal science, to crisis. The next stage in Kuhn's story is revolution. But before we get there, I will make some summary remarks about Kuhn's theory of normal science.

5.5 Wrap-up of Normal Science

Let's sum up what we have so far. Paradigms function to organize scientific work. Normal science is work aimed at extending and refining the paradigm. A good normal scientist is committed to the paradigm and does not question it. Normal scientists extend their paradigm both theoretically and experimentally. Anomalies inevitably arise, however, and eventually these reach a kind of critical mass, at which point scientists lose faith in the paradigm and the field plunges into a state of crisis.

We have not yet reached the most controversial part of Kuhn's theory, but are there any problems with what we have so far? One problem comes from Kuhn's insistence that, except in unusual cases, a scientific field has one paradigm per field per time. Kuhn held that, in general, a single paradigm will dominate its field. He did not think that two or three separate and competing paradigms could normally coexist. Many critics have thought Kuhn was wrong about this, both in the cases of physics and chemistry, which he discussed extensively, and, even more so, for areas like biology and psychology, which he did not often discuss. We will come back to this issue in chapter 7.

Secondly, Kuhn exaggerates the degree of commitment that a normal scientist does and should have to a paradigm. Kuhn describes the attitude of a normal scientist in very strong terms. Scientific education is a kind of "indoctrination," which results in scientists having a deep "faith" in their paradigm. As a description of how science actually works, this seems exaggerated. Sometimes there is a faithlike commitment, but sometimes there is not. Many scientists are able to say that they always *work* within a paradigm, for practical reasons, while being very aware of the possibility of error and the eventual replacement of their framework. One of the ironies of Kuhn's influence is that his book might have weakened the faith of some normal scientists, even though Kuhn thought that normal scientists should have a deep faith in their paradigms!

Leaving aside the factual issue of whether a tenacious commitment to a paradigm is what we generally find, we should also ask about Kuhn's belief that this strong commitment is a good thing. For Kuhn, the great virtue of normal science is its organized, coordinated structure, a structure that results in precision and efficiency. Unless debate about fundamentals is closed off, this precision and efficiency will be reduced. A key contrast here is with Popper, who insists on permanent open-mindedness. For Kuhn, a constant questioning and criticism of basic beliefs is liable to result in chaos—in the partially "random" fact-gathering and speculation that we see in preparadigm science. But here again, Kuhn probably goes too far. He does not take seriously the possibility that scientists could *agree* to work together in a coordinated way, not wasting time on constant discussion of fundamental issues, while retaining a *cautious* attitude toward their paradigm. Surely this is possible.

To close this chapter, I will describe an example that is very far from Kuhn's usual cases—an example that is on the periphery of science, but which I think illustrates Kuhn's insights well. During the 1980s and 1990s, there was a lot of excitement about a new field known as "Artificial Life." The aim was to use computers to model the most basic features of living systems, in such a way that it might eventually be reasonable to say that the artificial systems *were* alive. I went to several "Alife" conferences during this time and watched the field develop. At the time that I write this, the field seems to have ground to a halt. Maybe it will revitalize. But the failure of the field in recent years seems to me to involve some very "Kuhnian" reasons.

During the heyday of the movement, two or three pieces of work appeared that were strikingly successful. Perhaps the most impressive of all was Tom Ray's Tierra project, in which Ray was able to create open-ended evolution among self-replicating programs in a computer (Ray 1992). Work by Chris Langton and Steven Wolfram on the mathematical analysis of "cellular automata," simple systems in which local interactions between elements give rise to global, self-sustaining patterns, might be another case. And closer to the borderline with mainstream biology, there was Stuart Kaufmann's work on "the origins of order" in complex systems (Kauffman 1993).

All of this was impressive work, and it pointed the way forward to a consolidation of what these imaginative individuals had done. But the consolidation never happened. At each conference I went to, the larger group of people involved all seemed to want to do things from scratch, in their own way. Each had his or her own way of setting up the issues. There was not nearly enough work that *built on* the promising beginnings of Ray and others. The field never made a transition into anything resembling normal science. And it has now ground to a halt.

Another reason for the breakdown also relates to Kuhn. The field of Alife suffered from a kind of "premature commercialization." It was realized early on that some of the work had great potential for animation and other kinds of commercial art. At some Alife events, the climax of each talk seemed to be not some new theoretical idea, but a dramatic video. (I even heard speakers half-apologize before the video, as if they knew this was somehow not the right emphasis for their work.) For Kuhn, science depends on the good normal scientist's keen interest in puzzle-solving for its own sake. Looking outside the paradigm too often to applications and external rewards is not good for normal science.

Further Reading

Lakatos and Musgrave's collection *Criticism and the Growth of Knowledge* (1970) contains an excellent set of essays on Kuhn. A more recent edited collection is Horwich, *World Changes* (1993).

Kuhn's collection of essays *The Essential Tension* (1977b) is an important extra source. Kuhn also wrote two historical books (1957, 1978). His later essays have been collected in *The Road since Structure* (2000).

Levy 1992 is a readable survey of Alife work. Many of the best Alife papers may be found in the collection *Artificial Life II* (Langton et al. 1992).