Chapter 5

Kuhn's Revolution

5.1 *"The Paradigm Has Shifted"*

In this chapter we encounter the most famous book about science written during the last hundred years: *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 art 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, according to 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 exaggerated. Kuhn spent a good deal 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. It may also come as a surprise 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. But there is no denying that this was something of a "Trojan horse" situation. Logical empiricism was widely perceived to have been greatly 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 novel *A Man in Full*. Charlie Croker, a real-estate developer with debt problems, is talking with his financial adviser, Wismer ("the 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?" (1998, 71)

This sort of talk about "paradigm shifts" derives from Kuhn's book. 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 did not believe that. Instead, Kuhn had a rather complicated and subtle view about the roles of observation and logic in scientific change.

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

Kuhn did not invent the word "paradigm." It was an established term that meant (roughly) an illustrative example on which other cases can be modeled. (Kuhn discusses this original meaning in *Structure*, on page 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 just one sense in which Kuhn used the term. In *Structure*, "paradigm" is used in a number of different ways. I will recognize two different senses of the term. 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 how some part of the world works, and a way of doing science. When I say "paradigm" in this book without adding "broad" or "narrow," I mean this broad sense. According to Kuhn, one 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" for a specific achievement of this kind. I will call these paradigms in the *narrow* sense. 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, the broad sense is intended.

Kuhn used the phrase "normal science" for scientific work that occurs within the framework provided by a paradigm. A central 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. Kuhn 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 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 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. Because revolutions are essential to science, the task of describing rationality and progress in science as a whole becomes complicated.

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). But it's often hard to tell when he is just saying how things are and when he is making claims about what is good or bad. My own interpretation of Kuhn stresses the normative element in his work. I think Kuhn had a definite picture of how science should work and of what can cause harm to it. 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 tenacity with which ideas are held by scientists and the "conceptual boxes" that nature gets forced into . . . , 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.

How can features that look like failings and flaws 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 research 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 (1938). Along with these principles, the Skinnerian paradigm included a set of experimental tools, such as an apparatus 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: (1) genes are made of DNA (in all organisms except some viruses, which have RNA genes), (2) genes have their effects mostly by producing protein molecules, and (3) nucleic acids (DNA and RNA) specify the structure of proteins by determining the order of the units that make them up, and not vice versa. This last principle is often called "the central dogma" (Crick 1958). 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. Kuhn does allow that occasionally a field can be governed by several related paradigms, but this is rare. In general, Kuhn's picture has it that there is *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. For Kuhn, a key feature that distinguishes normal science from other kinds 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. Once biologists agree that genes are made of DNA, they can 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 the “consensus-forging” role of paradigms. He argues that without it, there is no chance for scientists to achieve a 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 Kuhn does think this (see Kuhn 1996, 24–25, 65), but the 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, Popper’s view does hold that a good scientist is permanently open-minded with respect to all issues in the field in which they are working, even the very basic issues. Popper criticized Kuhn explicitly on this point (1970); he said that although “normal science” of Kuhn’s kind does sometimes occur, it is a bad thing that it does.

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 that 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 that turn out eventually to have huge importance. And although the normal scientist is not trying to find phenomena that lead to paradigm change, 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. 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 expected results, 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.

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 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. 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 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 phrase "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. That 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, a paradigm does not break down easily. But when the right stimulus comes, the paradigm will disintegrate. 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 an important theme in Kuhn's work. All paradigms constantly encounter anomalies. For a Popperian, and many forms of empiricism, these anomalies should count as "refutations" of the theory. Kuhn thinks that science does not treat these ubiquitous anomalies as refutations, and it also 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 balance it manages to strike between being too resistant to change 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 were completely unresponsive to empirical failures, conceptual advance would grind to a halt. For Kuhn, science seems to achieve a delicate balance. This 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.

This kind of balance, if it is real, involves interesting relationships between the properties of individuals and communities. We had a quick first look at this theme in the previous chapter. Popper wants to see open-mindedness, and an ongoing process of conjecture and refutation. I asked: might an open-minded *community* be built out of rather closed-minded *individuals*? If scientists are wedded to their own conjectures until refutations arrive, but each is wedded to a different conjecture and would like to prove the others wrong, shouldn't the process of conjecture and refutation work? What is wrong with the situation where *B*'s role is to critically test *A*'s ideas, without *A* being critical about their own ideas? In Kuhn we see a different sort of combination. Normal science is full of rather closed-minded individuals, usually with no one trying to knock over a paradigm. But by their intensely focused work and the exploration of anomalies, they produce the paradigm's collapse.

From this point in the book onward, these relations between features of individuals and communities will be an important theme. To help with this, I will introduce a three-way distinction between different perspectives on science. We can think of this as three scales or levels of analysis, from fine-grained to coarse-grained (see figure 5.1). There are not sharp boundaries between the levels, though, and it is also helpful to think of the situation using an analogy with a zoom lens on a camera —we can zoom in and out continuously.

First, there is a very fine-grained or zoomed-in perspective on science, where we are looking at the activities of individual people. Observation, reasoning, and belief are treated as features of the individual scientist. I will call this level 1.

If we zoom out a bit, we will find communities of scientists and their social networks. This is level 2. Here we see relationships where scientists

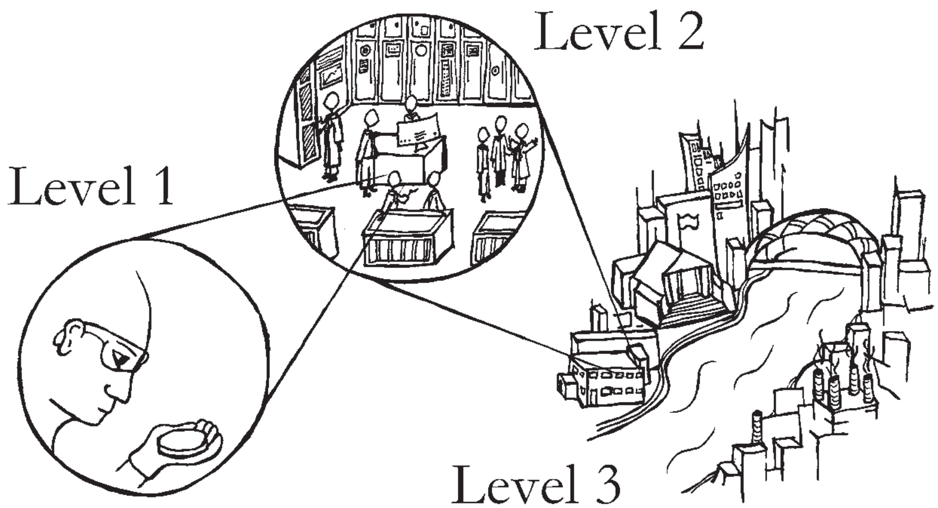

Figure 5.1. Three levels of analysis that can be applied to science, from the level of the individual (level 1), through the level of the scientific community (level 2), to the level of the whole society in which science is embedded (level 3)

collaborate and compete with each other. They use one another's work, criticize that work, and train newcomers.

As the camera zooms out further, we see the embedding of a scientific community within a larger society. Scientific communities have effects on technology, medicine, and education. Those communities are also affected by the economy and markets, by government policies, and attitudes to science in the culture as a whole.

Many interesting questions concern the relations between the three levels. We asked Popper, might an open-minded community be made up of closed-minded individuals? That is question about relations between levels 1 and 2. Much of the subtlety and interest in Kuhn's view is also about level 1 to level 2 relationships. Kuhn was not so interested in level 3; he was mostly concerned with scientific communities themselves. We will get to level 3 later.

Turning back to Kuhn, we have not yet reached the most controversial part of his theory, but are there any problems with what we have so far?

Kuhn probably 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 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 may have weakened the faith of some 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 whether this strong commitment is a good thing. For Kuhn, the great virtue of normal science is its organized, coordinated structure. 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 pre-paradigm 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.

5.5 *Revolutions and Their Aftermath*

> "Look," Thomas Kuhn said. The word was weighted with weariness, as if Kuhn was resigned to the fact that I would misinterpret him, but he was still going to try—no doubt in vain—to make his point. "Look," he said again. He leaned his gangly frame and long face forward, and his big lower lip, which ordinarily curled up amiably at the corners, sagged. "For Christ's sake, if I had my choice of having written the book or not having written it, I would choose to have written it. But there have certainly been aspects involving considerable upset about the response to it." **John Horgan, *The End of Science***

The most controversial parts of Kuhn's book were his discussions of scientific revolutions. Kuhn argued that some periods of scientific change involve a fundamentally different kind of process from what we find in normal science. Revolutionary periods see a breakdown of order and a questioning of the rules of the game, and they are followed by a process of rebuilding that creates new concepts, methods, and practices. Revolutions involve a breakdown, but they are essential to science as we know it. They have a "function," Kuhn often said, within the totality of science. The special features we associate with science arise from the combination and interaction of two different kinds of activity—the orderly, organized, disciplined process of normal science, and the periodic breakdowns of order found in revolutions. These two processes happen in sequence, within each scientific field. Science as a whole is a result of their interaction, and of nothing less.

Kuhn thought that looking *within* a period of normal science, you can easily distinguish good work from bad, rational moves from irrational, big problems from small problems, and so on. Progress is evident as time goes by. In a scientific revolution, as in a political one, rules break down and have to be rebuilt afresh. If you look at two pieces of scientific work across a revolutionary divide, it will not be clear whether there has been progress from earlier to later. It might not even be clear how to compare the theories or pieces of work at all—they may look like very different kinds of intellectual activity. The people on different sides of the divide will be "speaking different languages." In the climax of his book, Kuhn suggests that workers in different paradigms are living in different worlds.

How do revolutions occur? Above I described the transition from normal science to crisis. In Kuhn's story, large-scale scientific change usually requires both a crisis *and* the appearance of a new candidate paradigm. A crisis alone will not induce scientists to regard a large-scale theory or paradigm as "falsified." We do not find pure falsifications, rejections of one paradigm without simultaneous acceptance of a new one. Rather, the rejection of one paradigm accompanies the acceptance of another. But also, the switch to a new paradigm does not occur just because a new idea appears that looks better than the old one. Without a crisis, scientists will not have any motivation to consider radical change.

Suppose we do have a crisis, a period full of confusion and strange guests in the philosophy department. Then a new candidate paradigm appears, precipitating a revolution. What initially appears is a new paradigm in the narrow sense, an achievement that begins to inspire people and seems to point the way forward. More specifically, the new work appears to solve one or more of the problems that prompted the crisis in the old paradigm. The sudden appearance of problem-solving of this kind is the spark to the revolution. Kuhn did not think these processes could be described by an explicit philosophical theory of evidence and testing. Instead, we should think of the shift to a new paradigm as a something like a "conversion" phenomenon, or a gestalt switch. Kuhn also argued that revolutions are capricious, disorderly events. They are affected by idiosyncratic personal factors and accidents of history.

One reason for the disorderly character of revolutions is that some of the principles by which scientific evidence is assessed are themselves liable to be destabilized by a crisis, and they can change with a revolution. Kuhn did not argue that traditional philosophical ideas about how theories should relate to evidence are completely misguided. He made it clear in his later work that there are some core ways of assessing theories that are common to all paradigms (1977c, 321–22). Theories should be predictively accurate, consistent with well-established theories in neighboring fields, able to unify disparate phenomena, and fruitful of new ideas and discoveries. These principles, along with other similar ones, "provide *the* shared basis for theory choice" (322). (I should note that some commentators think these later essays change, rather than clarify, the views presented in *Structure*.) But Kuhn thought that when these principles are expressed in a broad enough way to be common across all of science, they will be so vague that they will be powerless to settle hard cases. Also, these goals must often be traded off against each other; emphasizing one often requires downplaying another.

Within a single paradigm, more precise ways of assessing hypotheses will operate. These will include sharper versions of the common principles listed above, but these sharper versions will not be explicit "principles." Instead, they will be more like habits and values, aspects of the shared mindset of normal scientists. Those are liable to change in the course of a revolution.

So we have two kinds of scientific change in Kuhn's picture, neither of which is what empiricist philosophies of science might have led us to expect. Change within normal science is orderly and responsive to evidence—but normal science works via a closing of debate about fundamental ideas. Revolutionary change does involve challenges to fundamentals, but these are episodes in which the orderly assessment of ideas breaks down. Displays of problem-solving power have an important role in these transitions between paradigms, but the shifts also involve gestalt switches and leaps of faith.

You might wonder at this point about the power of observational data to impose some order on these revolutionary changes. Kuhn, along with some others around the same time, argued that we cannot think of observation as a neutral source of information for choosing between theories, because what people see is influenced by their paradigm. This "theory-ladenness of observation" is an important topic in its own right, and it will be discussed in chapter 9.

In Kuhn's treatment of revolutionary change, the distinction between descriptive and normative issues is again important. Kuhn uses language that suggests that revolutions are not only bound to happen, but have a positive role in science. They are part of what makes science so powerful as a means for exploring the world (a "supremely efficient instrument"; 1996, 169). Some interpreters regard this as colorful talk and not essential to Kuhn's general message. I have the opposite view; I think this is central to Kuhn's picture. Science for Kuhn is a social mechanism that combines two capacities. One is the capacity for sustained, cooperative work. The other is science's capacity to partially break down and reconstitute itself from time to time. When a paradigm runs out of steam, there is nothing within the community that could reliably give science a set of directions for orderly movement toward a new paradigm. Instead, the goals of science are best served at these special times by a disorderly process, in which even basic ideas are put back on the table for discussion, and a new direction eventually emerges from the chaos. This sounds strange, but I think it was Kuhn's picture.

Here is another way of expressing these relationships. Without the tenacious commitment to a paradigm seen in normal science, investigation tends to be shallow. But without a descent into a crisis, there is no motivation to consider radically new ideas. Significant innovation requires both normal and revolutionary modes of change.

All of Kuhn's claims about what follows what in scientific change tend to be qualified; he is describing the central and characteristic patterns of change, not every case without exception. But the idea that revolutions generally require crises raised some serious historical issues. Was there a crisis in the state of astronomy before Copernicus, or in biology before Darwin? Was there a state of disorder following an earlier period of confident work? Maybe. But taking another biological example, if the appearance of genetics as a science around 1900 was a revolution, it is very hard to find a crisis in the work on inheritance that preceded it. Maybe Kuhn would regard this as a transition from pre-paradigm science to normal science, though that could not be said about most of biology around that time.

In the "Postscript" written for the second edition of *Structure*, Kuhn qualified his claims about the role of crisis (1970a, 181). He still maintained that crises are the "usual prelude" to revolutions. But even that claim is controversial. Kuhn's emphasis on crises sometimes seems driven more by the demands of his hypothesized mechanism for scientific change than by the historical data; Kuhn's story demands crises because only a crisis can loosen the grip of a paradigm and make people receptive to alternatives.

Another way there might be a revolution without a crisis comes from interactions between neighboring fields. Kuhn sometimes writes in a way that treats each scientific field as self-contained, but this is surely not so. Might there be a situation where there is a Kuhnian revolution in one field—with a crisis and all the rest—and the new paradigm that appears in that field also inspires a radical change in another field? This would be quite "un-Kuhnian" if things in the second field were previously going fairly well, but a revolution happens anyway. I don't have a clear example of this, but here is an approximate one. In the late 1950s there was a revolution in linguistics, owing to the work of Noam Chomsky (1957 and 1959). His "generative linguistics" introduced the idea that all humans have an innate knowledge of a grammatical "deep structure" that is common to all languages. For Chomsky, learning has a surprisingly minor role in how language develops in each of us. This might also be another case where you might wonder whether there was a crisis before the revolution, but in any case, once the new paradigm was established by Chomsky in linguistics, it had massive effects on a neighboring field, psychology. Earlier in this chapter I used as an example of normal science the behaviorist paradigm of Skinner in mid-twentieth-century psychology. This approach was replaced during and around the 1960s by "cognitive" psychology, using ideas of information processing, computation, and symbol manipulation. Several things fed into this shift (Greenwood 2015). Behaviorist views had problems, and new ideas surrounding the invention of computers seemed important for psychology. But the new approach that Chomsky introduced in the study of language was also inspiring, and a significant contributor to the revolution in psychology.

We might describe this case instead by saying that Chomsky was as much a psychologist as a linguist, and had effects on both fields. We might also wonder whether any of this fits Kuhn's model, in which there are paradigms that dominate a field until they die. But this case is a partial illustration of a revolution induced by a change in a neighboring field, and this does make sense as a possibility: a Kuhnian revolution in one field might prompt a no-crisis revolution in a neighboring field, simply by being an impressive and relevant breakthrough.

5.6 *Incommensurability, Relativism, and Progress*

Kuhn said that revolutions have a "non-cumulative" nature. There is no steady buildup of some useful outcome, like *true beliefs*, as science goes along. Instead, according to Kuhn, in a revolution you always gain some things and lose some things. Questions that the old paradigm answered now might become puzzling again, or cease to be coherent questions. So we might want to ask, do we usually gain more than we lose? In at least the middle chapters of his book, Kuhn seems to think there is no way to answer this question in an unbiased way (1996, 109, 110). Of course, it will *feel* like we have gained more than we've lost, or we would not have had the revolution at all. But that does not mean that there is some unbiased way of comparing what we had before with what we have after.

This question connects us to one of the most famous topics in Kuhn's work, the idea that different paradigms in a field are *incommensurable* with each other.

What does "incommensurable" mean here? Most literally, it means not comparable by use of a common standard or measure. This idea needs to be carefully expressed, however. Two rival paradigms can be compared well enough for it to be clear that they are incompatible—that they are rivals. And people working within any one paradigm will have no problem saying why their paradigm is superior to others, by citing key differences in what can be explained and what cannot. But these comparisons will be compelling only to those inside the paradigm from which the claim of superiority is being made. If we look "from above" at two people who are arguing during a revolutionary period, defending different approaches to their field, it will often appear that the two people are talking past each other.

There are two reasons for this—there are (roughly speaking) two aspects of the problem of incommensurability. First, people debating fundamental ideas will not be able to fully communicate with each other; they will use terms in different ways and in a sense will be speaking slightly different languages. Second, even when communication is possible, people in this situation will use different standards of evidence and argument. They will not agree on what a good theory is supposed to do.

Let us look first at the issues involving language. Here Kuhn's claims depend on a holistic view about the meaning of scientific language. Each term in a theory derives its meaning from its place in the whole theoretical structure. Two people operating within different paradigms might seem to use the same word—"mass" or "species"—but the meanings of these terms will be slightly different because of their different roles in the two rival theories.

For Kuhn, it will not usually happen that two people within different paradigms interact directly. As we saw, paradigms dominate and then are overthrown. But there are situations that are relevantly similar. The main one will involve one person defending an old paradigm and another person advocating a new one.

If incommensurability of meanings is real, then it should be visible in the history of science. Those who study the history of science should be able to find many examples of the usual signs of failed communication—confusion, correction, a perceived inability to make contact. Although I am not a historian of science, my impression is that historians have not found many examples of failed communication in crucial debates. Scientists are often adept at "scientific bilingualism," switching from one framework to another. And they are often able to improvise ways of bridging linguistic gaps, much as traders from different cultures are able to, by improvising "pidgin" languages (Galison 1997). Scientists often deliberately misrepresent each other's claims, in the service of rhetorical points, but that is not a case of failed comprehension or communication.

The other form of incommensurability is more important. This is incommensurability of *standards*. Kuhn argued that paradigms tend to bring with them their own standards for what counts as a good argument or good evidence, and these standards can change across a revolution.

One of Kuhn's best examples here involves the role of *causal explanation*. Should a scientific theory be required to make causal sense of why things happen? Should we always hope to understand the mechanisms underlying events? Or can a theory be acceptable if it gives a mathematical formalism that describes phenomena without making causal sense of them? An example of this problem concerns Newton's theory of gravity. Newton gave a mathematical description of gravity—his famous inverse-square law—but did not give a mechanism for how gravitational attraction works. Indeed, Newton's view that gravity acts instantaneously and at a distance seemed to be extremely hard to supplement with a mechanistic explanation. Was this a problem with Newton's theory, or should we drop the demand for a causal mechanism and be content with the mathematics? Would it be scientifically acceptable to regard gravity as just an "innate" power of matter that follows a mathematical law? People argued about this a good deal in the early eighteenth century. Kuhn's view is that there is no general answer to the question of whether scientific theories should give causal mechanisms for phenomena; this is the kind of goal that will be present in one paradigm and absent from another.

During the early twentieth century, there was a similar, although smaller-scale, debate within English biology. In the latter part of the nineteenth century, a group of biologists called the "Biometricians" had formulated a mathematical law that they thought described the inheritance of biological traits across generations (Provine 1971). They had no mechanism for how inheritance works, and their law did not lend itself to supplementation with such a mechanism. In 1900 the pioneering work done by Mendel in the mid-nineteenth century was brought to light, and the science of genetics was launched. For about six years, though, the Biometricians and the Mendelians conducted an intense debate about which approach to understanding inheritance was superior. One of the issues at stake was what kind of theory of inheritance should be the goal. The Biometricians thought that a mathematically formulated law was the right goal, while William Bateson, in the Mendelian camp, argued that understanding the mechanisms of inheritance was the goal. In the short term, the Mendelians won the battle. Eventually the two approaches were married; modern biology now has both math and mechanisms. But during the battle there was intense argument about what a good scientific theory should do (MacKenzie 1981). I agree with Kuhn that incommensurability of standards is a real and interesting issue.

Kuhn's discussion of incommensurability is the main reason his view of science is often referred to as "relativist." Kuhn's book is often considered one of the first steps in a tradition of work in the second half of the twentieth century that embraced relativism about science and knowledge. Kuhn himself was shocked to be interpreted this way. But what is relativism? This is a chaotic area of discussion. Roughly speaking, relativist views hold that the truth or justification of a claim, or the applicability of a rule or standard, depends on one's situation or point of view. Such a claim might be made generally ("all truth is relative") or in a more restricted way, about art, morality, good manners, or some other particular domain. The "point of view" might be that of an individual, a society, or some other group.

If people differ about the facts or the proper standards in some area, that itself does not imply that relativism applies in that case; some of the people might just be wrong. It is also important that if someone holds that moral rightness or good reasoning "depends on context," that need not be a form of relativism, although it might be. This is because a single set of moral rules (or rules of reasoning) might have built into them some sensitivity to circumstances. A set of moral rules might say, "If you are in circumstances *X*, you should do *Y*." That is not relativism, even though not everyone might be in circumstances *X*.

In this discussion we are mostly concerned with relativism applied to standards governing reasoning, evidence, and the justification of beliefs. And the "point of view" here is that of the users of a paradigm.

Is Kuhn a relativist with regard to these matters? Kuhn had a subtle view that is hard to categorize, and I doubt that everything Kuhn said can be fitted together consistently. The issue of relativism in Kuhn is also bound up with the question of how to understand scientific progress, something Kuhn struggled with in the final pages of his book.

As we have seen, Kuhn argued that different paradigms often carry with them different standards for good and bad scientific work. So far, this does not tell us whether Kuhn was a relativist—there might be an advance in standards as well in theories, where better ones replace worse ones as time passes. But Kuhn also argued that the paradigms we have in science now are not closer than earlier paradigms to an "ideal" or "perfect" paradigm. Scientific fields do not head steadily toward a final paradigm that is superior to all others.

Claims like these seem to be taking us close to a relativist view about the standards and ideas that are not shared across paradigms. But Kuhn said some rather different things in the final pages of *Structure*. There he said that our present paradigms have more problem-solving power than earlier paradigms did. This claim was made when Kuhn confronted the question of how to understand progress in science.

Kuhn gave two quite different kinds of explanation for the apparent large-scale progress we see in science. His first was a kind of eye-of-the-beholder explanation. Science will inevitably appear to exhibit progress because each field has one paradigm at a time, the victors after each revolution will naturally view their victory as progressive, and science is insulated from outside criticism. Celebrations of progress on the part of the victors will not be met with any serious objection. This first explanation of the appearance of progress is consistent with a relativist view of the changes between paradigms.

Kuhn also developed a different account of the appearance of progress in science, especially in the final pages of *Structure*, and this account seems to conflict with a relativist reading. Here Kuhn argued that science has a special kind of efficiency, and this efficiency results in a genuine form of progress across revolutions: the number and precision of solutions to problems in a scientific field tend to grow over time (1996, 170). It is quite difficult to reconcile this claim with some of his discussions of incommensurability in earlier chapters. There he said that revolutions always involve losses as well as gains, and he also said that the standards that might be used to classify some problems as important and others as unimportant tend to change as a result of revolutions. It is not clear whether the ideas about progress that Kuhn introduces in the last pages of *Structure* are compatible with the rest of the book.

5.7 *The X-Rated "Chapter X"*

> I have argued so far only that paradigms are constitutive of science. Now I wish to display a sense in which they are constitutive of nature as well.
> **Thomas Kuhn**, *Structure*

Kuhn's book starts out with his analysis of normal science. The middle chapters become more adventurous, and the book climaxes with chapter X. Here Kuhn puts forward his most radical claims. Not only do ideas, standards, and ways of seeing change when paradigms change; in some sense the *world* changes as well. Reality itself is paradigm-relative or paradigm-dependent. After a revolution, "scientists work in a different world" (1996, 135).

Philosophers and other commentators tend to split between two different attitudes toward this part of Kuhn's work. One group thinks that Kuhn exposes the fact that any notion of a single, stable world persisting through our various attempts to conceptualize it is an idea dependent on a failed view of science and outdated psychological theories. Kuhn, on this interpretation, shows that changing our view of science requires us to change our metaphysics too—our most basic views about reality and our relationship to it. Holding onto the idea of a single fixed world that science strives to describe is holding onto the last element of a conservative view of conceptual change.

Others think that this side of Kuhn's work is a mess. When paradigms change, ideas change. Standards change also, and maybe the way we experience the world changes as well. But that is very different from claiming that the world itself depends on paradigms. The way we see things changes, but the world itself does not change.

I am in the second camp; the X-rated chapter X is the worst material in Kuhn's great book. It would have been better if he had left this chapter in a taxi, in one of those famous mistakes that authors are prone to.

I should say immediately that it is not always clear how radical Kuhn wants to be. Sometimes it seems that he is just saying that our ideas and experience change. Also, there are some entirely reasonable claims we can make about changes to the world that result from paradigm changes. As paradigms change, scientists change their behavior and experimental practices as well as their ideas. Scientific revolutions result in new technologies that have far-reaching effects on the world. Probably many or most of the objects around you right now would not have existed at all if a lot of particular scientific theories had not been developed. Changes of this kind can be far-reaching, but they are still restricted by the causal powers of human action. We can change plants and animals by controlled breeding and genetic engineering. We can dam rivers and pollute them. We can create computers. But our reach is restricted, not indefinite. Kuhn discussed some cases in chapter X that make it clear that he did not have ordinary causal influences in mind. He discussed cases where changes in ideas about stars, planets, and comets led to astronomers "living in a different world," for example (1996, 117).

Perhaps the main problem with these discussions is that Kuhn seems to think that the view that we all inhabit a single world, existing independently of paradigms, also commits us to a naive set of ideas about perception and belief. This is an error. We might decide that perception is radically affected by beliefs and expectations, while still holding that perception is something that connects us to a single real world that we all inhabit.

Did Kuhn really make a mistake of this kind? Here is an especially relevant quote from the chapter:

> At the very least, as a result of discovering oxygen, Lavoisier saw nature differently. And in the absence of some recourse to that hypothetical fixed nature that he "saw differently," the principle of economy will urge us to say that after discovering oxygen Lavoisier worked in a different world. (1996, 118)

"Principle of economy"? Would it be economical for us to give up the idea that Lavoisier was living in the same world as the rest of us and acquiring new ideas about it? Appeals to economy are always suspicious in philosophy. They are usually weak arguments. This one also seems to have the accounting wrong.

From the point of view of a kind of skeptical philosophical discussion, it can be considered hypothetical that there is a world beyond our momentary sensory experiences and ideas. But this is a rather special sense of "hypothetical." If we are trying to understand science as a social activity, as Kuhn is, there is nothing hypothetical about the idea that science takes place in a single, structured world that includes the community of scientists, their instruments and laboratories, and various other objects, including the ones they try to study.

Kuhn hesitated after that quote above—maybe, he said, we should look into ways of "avoiding this strange locution." But he decided that we "must learn to make sense of statements that at least resemble these" (p. 121). I think Kuhn was not entirely sure what he wanted to say, but he was sure what he did *not* want to say. He did not want to say that although we might see the world differently and interpret it differently, we are all dealing with the same world, a world that might eventually change materially after a change of paradigm, as a result of new technologies and actions, but not just via the change in worldview itself. And that, I think, is what he should have said.

These issues connect to one raised in the previous section. Kuhn opposed the idea that the large-scale history of science involves an accumulation of more and more knowledge about how the world really works. He was willing, on occasion, to recognize some kinds of accumulation of useful results: there is an accumulation of problem-solving power. But we cannot see, in science, an ongoing growth of knowledge about the structure of the world.

When Kuhn wrote about this issue, he often came back to cases in the history of physics. Like Popper and others, Kuhn seems to have been hugely influenced by the fall of the Newtonian picture of the world at the start of the twentieth century. But Kuhn, along with some others, was too focused on the case of physics; he seems to have thought that we can only see science as achieving a growth of knowledge about the structure of the world if we can see this kind of progress in the parts of science that deal with the most low-level and fundamental entities and processes. If we look at other parts of science—at chemistry and molecular biology, for example—it is much more reasonable to see a continuing growth (with some hiccups) in knowledge about how the world works. We see a steady growth in knowledge about the structures of sugars, fats, proteins, and other important molecules, for example. There is no evidence that these kinds of results will come to be replaced, as opposed to extended, as science moves along. This type of work does not concern the most basic features of the universe, but it is undoubtedly science. I think that when we try to work out how to describe the growth of knowledge over time in science, we should probably treat theoretical physics as a special case and not as a model for all science (McMullin 1984). Kuhn's pessimism about the accumulation of knowledge in science appears overstated.

Although Kuhn's most famous discussions of realism are his notorious claims about how the world changes when paradigms change, at other times he seems more like a kind of pessimistic or skeptical realist. These are passages where Kuhn seems to think that the world is so complicated that our theories will always run into trouble in the end—and this is a fact about the world that is independent of paradigms. We try to "force" nature into "boxes," but nature resists. All paradigms are doomed to fail eventually, because nature is complex and science must simplify. This view is more coherent and more interesting than Kuhn's changing-worlds position.

5.8 *Final Thoughts about Kuhn*

Kuhn changed the philosophy of science by describing an extraordinarily vivid picture of scientific change. He attributed the success and the power of science to a delicate balance between factors in a complex and fragile mechanism; science owes its strength to an interaction between the ordered cooperation and single-mindedness of normal science, together with the ability of these behavioral patterns to break down and reconstitute themselves in revolutions. Quite quickly, critics were able to find problems with this mechanism when interpreted as a description of how science actually works—paradigms need not exert the kind of psychological dominance that Kuhn describes, and large-scale changes can occur without crises, for example. Many parts of Kuhn's mechanism are especially hard to apply to the history of biology. Kuhn's account of the mechanisms behind scientific change is in several ways too tightly structured, too specific. The real story is more mixed. But Kuhn's work was also an attempt at a new approach to the philosophy of science, a new kind of theory. These are theories that approach questions in the philosophy of science by looking at the social structure of science and the mechanisms underlying scientific change. This approach has flourished.

Back in the first chapter, I distinguished views that construe science broadly from those that construe it narrowly. Those that construe it broadly see the differences between science and everyday problem-solving as matters of detail and degree. Kuhn's theory is nothing like this. His theory of science emphasizes the differences between science and various other kinds of learning and investigation. Science is a form of organized behavior with a specific social structure. As a consequence, science appears in this story as a rather fragile cultural achievement; subtle changes in the education, incentive structure, and political situation of scientists could result in the loss of the special mechanisms of change that Kuhn described.

Before moving on, as a kind of appendix I will mention some connections between Kuhn's theory of science and a few other famous mechanisms for change. First, in some ways Kuhn's view of science has an "invisible hand" structure. The Scottish political and economic theorist

Adam Smith argued in *The Wealth of Nations* ([1776] 1976) that individual selfishness in economic behavior leads to good outcomes for society as a whole. The market is an efficient distributor of goods to everyone, even though the people involved are each just out for themselves. Here we have an apparent mismatch between individual-level characteristics and the characteristics of the group; selfishness at one level leads to the general benefit. The mismatch disappears when we look at the consequences of having a large number of individuals interacting together. (Smith's theory is another with interesting relations between individual-level and community-level facts.) Something similar is seen in Kuhn's theory of science: narrow-mindedness and dogmatism at the level of the individual lead to intellectual openness in science as a whole. Anomaly and crisis produce such stresses in the normal scientist that a wholesale openness to novelty is found in revolutions. In the next chapter we will look at a critic who was suspicious of Kuhn on exactly this point; he thought Kuhn was trying to excuse and encourage the most narrow-minded and unimaginative trends in modern science.

Another comparison requires a bit more background knowledge. In the chapter on Popper, I briefly compared his conjecture-and-refutation mechanism with a Darwinian mutation-and-selection mechanism in biology. A biological analogy can also be found in the case of Kuhn. During the 1970s the biologists Stephen Jay Gould, Niles Eldredge, and others argued that a large-scale pattern seen in much biological evolution is one of "punctuated equilibrium" (Eldredge and Gould 1972). A lineage of organisms in evolutionary time will usually exhibit long periods of stasis, during which we see low-level tinkering but little change to fundamental structures. These periods of stasis or equilibrium are punctuated by occasional periods of much more rapid change in which new fundamental structures arise. (Note that "rapid" here means taking place over thousands of years rather than millions.) The rapid periods of change are disorderly and unpredictable when compared to the simplest kind of natural selection in large populations. The periods of stasis also feature a kind of "homeostasis," in which the genetic system in the population tends to resist substantial change.

The analogy with Kuhn's theory of science is striking. We have the same long periods of stability and resistance to change, punctuated by unpredictable, rapid change to fundamentals.

The theory of punctuated equilibrium in biology was controversial for some time, especially because it was sometimes presented by Gould in rather radical forms (Gould 1980). The idea of a kind of homeostatic resistance to change brought about by the genetic system is a tendentious one, for example. And the idea that ordinary processes of natural selection do not operate normally during the periods of rapid change, but are replaced by other kinds of processes, is also very unorthodox. But as the years have passed, the idea of punctuated equilibrium has been moderated and has passed, in its more moderate form, into mainstream biology's description of some (not all) patterns in evolution.

Gould also wrote a paper called "Eternal Metaphors in Paleontology" (1977) in which he argued that the history of theorizing about the history of life sees the same basic kinds of ideas about change come up again and again, often mixed and matched into new combinations. The analogy between Kuhn's theory and the biological theory of punctuated equilibrium shows a similar kind of convergence in stories about processes of change. I say "convergence" here, but in some ways it's more than that. Gould (2002, 967) acknowledged the influence of Kuhn's picture of science on him when he was working out his biological ideas in the 1960s and 1970s.

Further Reading and Notes

Lakatos and Musgrave's *Criticism and the Growth of Knowledge* (1970) is an excellent set of essays on Kuhn. Another edited collection is Horwich's *World Changes* (1993). The fiftieth anniversary of Kuhn's book, in 2012, produced much reflection. See, for example, Devlin and Bokulich (2015).

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

For a detailed discussion of Kuhn's philosophy, see Hoyningen-Huene

(1993). Kitcher (1993) contains discussions of Kuhn's arguments about revolutions and progress. Doppelt (1978) is a clear discussion of incommensurability of standards.

The general question of whether there might be a revolution without a crisis, owing to another revolution in a neighboring field, was raised to me in discussion by Ramesh Ghelichi. Two important early works on the psychological side are Newell, Shaw, and Simon (1958) and Miller, Galanter, and Pribram (1960). Miller later said that encountering Chomsky at a seminar persuaded him to abandon behaviorism. According to Greenwood (2015), which I draw on here, many people involved in this episode in the 1960s saw it self-consciously in Kuhnian terms—they thought they were engaged in a Kuhnian revolution, and "everyone toted around their little copy of Kuhn's *The Structure of Scientific Revolutions*" (James Jenkins, quoted in Greenwood 2015, 454).