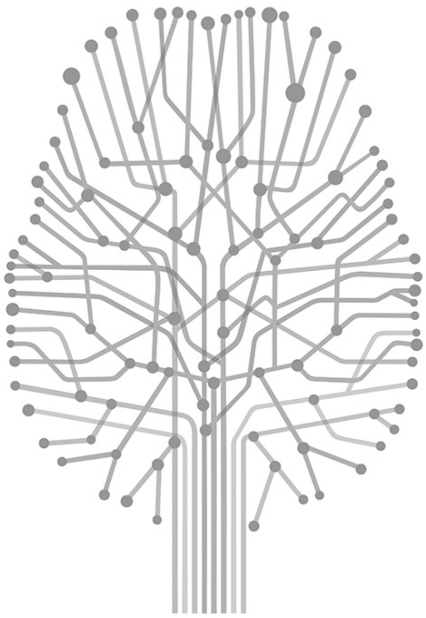
THE MEANING OF SCIENCE

An Introduction to the Philosophy of Science

Tim Lewens



BASIC BOOKS
A Member of the Perseus Books Group
New York

Published in Great Britain by Allen Lane, The Penguin Press

Published in the United States by Basic Books,

A Member of the Perseus Books Group

Designed by Jeff Williams

Library of Congress Cataloging-in-Publication Data
Names: Lewens, Tim.
Title: The meaning of science : an introduction to the philosophy of science / Tim Lewens.
Description: New York : Basic Books, [2015] | Includes bibliographical references and index.
Identifiers: LCCN 2015039234 | ISBN 9780465097487 (hardcover) | ISBN 9780465097494 (e-book)
Subjects: LCSH: Science—Philosophy.
Classification: LCC Q175 .L477 2015 | DDC 501--dc23 LC record available at http://lccn.loc.gov/2015039234

10 9 8 7 6 5 4 3 2 1

Chapter Three

The "Paradigm" Paradigm

Popper Versus Kuhn

Students who approach the philosophy of science for the first time usually begin by meeting, and then dismembering, the views of Karl Popper. We did the same in Chapter 1. They then move on to acquaint themselves with the philosophical image of science put forward by Thomas Kuhn. The two thinkers are often cast as great rivals who offer markedly contrasting accounts of scientific achievements and the nature of change in the sciences. Popper takes the role of the champion of scientific rationalism, and of scientific progress. We have already seen how keenly scientists, glad to find a philosopher who massages their collective scientific ego so effectively, have embraced Popper's views.

Kuhn, on the other hand, deals in ideas that seem far more threatening to cherished notions of the advancement of science. It is commonplace to read that Kuhn denies that changes in scientific thinking are rational, and it is even more common to read that Kuhn denies that science makes progress. He is sometimes accused of reducing changes in accepted scientific wisdom to an irrational form of herding behavior, or "mob psychology." It is maybe not surprising, then, that he has been treated with suspicion from many within science.

These efforts to set Popper and Kuhn against each other rely on significant distortions of their writings. It is worth being clear about this at the outset: Kuhn does believe that science makes progress; Kuhn does believe that changes in scientific theory are rational. Indeed, a proper understanding of Kuhn's work shows that his views are far less exotic, and far more persuasive, than a superficial reading suggests. Meanwhile, Popper, who (as we saw in Chapter 1) ultimately grounds the foundations of scientific thought in collective convention, is perhaps more vulnerable to accusations of irrationality and mob psychology.

Thomas Kuhn (1922–1996)

Thomas Kuhn entered Harvard University in 1940 as an undergraduate specializing in physics. In 1945 he began doctoral research—still in physics, still at Harvard—but his interests extended well beyond his thesis topics of quantum mechanics and magnetism. At the time he started his PhD, he simultaneously undertook work in philosophy. He served as editor of the Harvard newspaper *The Crimson,* and he was president of the literary Signet Society.[1] From the late 1940s up until 1956, Kuhn taught a course at Harvard that was intended to familiarize undergraduates in the humanities with work in the sciences. This was when he first became engaged with the history of science, because his teaching method focused on historical case studies going back to Aristotle. In 1956 Kuhn moved to a position in the philosophy department at Berkeley, California, albeit a position in the history of science rather than the philosophy of science.

It was here that Kuhn began to grapple with philosophical work by the likes of Ludwig Wittgenstein and Paul Feyerabend.

Kuhn's best-known work by far is *The Structure of Scientific Revolutions* (henceforth *Structure*), a book that is short, engaging, and important. It was first written in 1962, for incorporation in a series called The International Encyclopedia of Unified Science. This venue for *Structure*'s first publication is ironic, for Kuhn's views are usually thought antithetical to the notion that science as a whole constitutes a unified edifice. Kuhn left Berkeley for Princeton in 1964, and then moved again to MIT in 1983. Much of his later work was devoted to clarifying, modifying, and applying the ideas initially presented in *Structure:* at the time of his death in 1996, for example, he was working on a book exploring an evolutionary conception of the growth of scientific knowledge, an idea that he had first defended in *Structure* itself.

The Structure of Scientific Revolutions

Structure's central thesis is that scientific change is cyclical. Long periods of "normal science," when communities of investigators are more or less united in a vision of what good research looks like, are punctuated by occasional violent conceptual "revolutions." Kuhn contends that examples of these revolutions include acceptance of the idea, following work by Nicolaus Copernicus in the sixteenth century, that the Sun (rather than the Earth) was at the center of the universe and acceptance of Einstein's introduction of the relativistic view of space and time at the beginning of the twentieth century.

Revolutions, says Kuhn, are preceded by a buildup of "anomalies"—problematic phenomena that the anointed scientific approach is unable to account for, no matter how hard scientists try to shoehorn them into accepted explanatory frameworks. After a revolution, scientists embrace a new approach that is able to account for the anomalies that provoked the crisis. Kuhn suggests that scientific communities may need to change their membership for this to occur: sometimes the only way a new approach can gain hold is when the old guard retire from their posts, or when they die.[2] A new period of "normal science" begins, until eventually there is another accumulation of anomalies, another crisis, another revolution. That, in rough terms, is Kuhn's image of science. But what does it involve in detail?

In what Kuhn calls the "pre-paradigm" phase, scientific disciplines are characterized by considerable disunity among their practitioners, often coupled with explicit theoretical debate about the proper foundations of their enterprise. There is little agreement about the requirements of proper scientific training, and little agreement about what sort of thing counts as a significant achievement on the part of earlier thinkers. My own discipline of philosophy is, and most likely always will be, in a state rather like this: there is plenty of valuable activity in the world's philosophy departments, but academic philosophers are not sure about whether their discipline should be directed at examining the history of great philosophical works, exposing the meanings of various problematic concepts, unearthing fundamental facts about the nature of the universe, offering a critical synthesis of the significance of scientific research, or something else altogether. There is also profound disagreement about what counts as good philosophical work. For some, Wittgenstein is a pernicious anti-philosopher who has wrought great damage on the discipline; for others, Wittgenstein is the only thinker to have diagnosed the mistakes of the Western philosophical tradition. Some think of Jacques Derrida's work as groundbreaking, others consider him a charlatan.

When fields of scientific knowledge first got going, Kuhn says, they all had this pre-paradigmatic character, symptomatic of philosophy today. This may be no coincidence, for many—perhaps all—of today's scientific disciplines started out life as speculative branches of philosophy itself. Eventually, says Kuhn, fields of inquiry settle into phases of what he calls "normal science," guided by a paradigm.

This word, *paradigm,* has been used so widely in the management-speak of recent years that we must be careful not to let it wash over us. Instead we must attend to precisely what Kuhn means by it. In the important "Postscript" that he wrote seven years after the first publication of *Structure,* Kuhn acknowledged that he had perhaps used the word in as many as twenty-two different senses.[3] I follow Kuhn himself (and also my former colleague Peter Lipton) in thinking that it is particularly important to think of a paradigm in the specific sense of an *exemplar*—that is, an agreed-upon instance of important scientific achievement.[4]

A paradigm, understood as an exemplar, is not a style of thinking, a worldview, or a form of training. An exemplar is instead a particular example of a solution to a scientific problem. It is something that everyone, or more or less everyone, in a scientific community acknowledges as a piece of work to be admired and emulated. For example, Gregor Mendel's work on inheritance in peas was eventually accorded that status by twentieth-century geneticists. Isaac Newton's work in his 1687 book *Principia* was thought of as an exemplar for centuries. And it seems likely that Charles Darwin structured the *Origin of Species* according to Victorian recommendations for how to formulate and defend scientific hypotheses. Those recommendations, in turn, were based on the efforts of Victorian men of science to pinpoint exactly what had made Newton's work so good.[5]

Kuhn's notion of "normal science" is meant to bring out the idea that this type of science is business as usual: scientists within a given discipline know what sort of work they are supposed to be doing because they agree on which past achievements are exemplary. I do not mean to suggest that all scientists in a community work in precisely the same ways: indeed, this is one of Kuhn's key messages when he tells us that science is guided by exemplars rather than by rules.

It is easiest to see the difference between the notion that science is guided by exemplars and the notion that science is guided by rules if we begin by focusing on activities that are quite distinct from science. A group of expert chefs might agree that Ferran Adrià's work in the 2000s at his restaurant El Bulli in Catalonia is an exemplar for elite cooking, while disagreeing about exactly what made his cuisine so good. Hence they might unite in the notion that Adrià's work should be emulated, while diverging considerably in what they think it means to work "just like him." The cooking styles of these disciples will not be uniform. Contrast this with a rule-based approach, which aims to codify in a far more explicit way what is involved in good cooking. Many amateur cooks in Britain try slavishly to reproduce the recipes of Delia Smith by following her every instruction in detail, even down to using the same cookware. Kuhn's point is that while scientists might be united in their admiration of Newton's achievements, this leaves open the question of exactly how a given investigator will understand what it means to work in the manner of Newton's *Principia.* Scientists are guided by exemplars, but they are not shackled by a detailed recipe book telling them how to investigate the world.

This brings us to a second important point about Kuhn's notion of normal science. The first complete sequencing of the human genome—or rather, the draft sequencing of a supposedly representative genome for our species—was a monumental achievement when it was first announced in 2001.[6] Since that time, we have been treated to more detailed data regarding how the human genome varies, and we have also been given whole genome sequences for many other species, including the genome of the dog, the genome of rice, and the genome of the pigeon.[7] For people with the right equipment and training, genome sequencing is no longer a challenge. It would be tempting, then, to think of the initial human genome project as an exemplar and these other projects as instances of normal genomic science. This could give the misleading implication that, for Kuhn, normal science is just "more of the same"—the mechanical application of methods that have been shown to work by earlier scientists of greater stature.

But Kuhn does not mean to imply that normal science—the work most scientists do, most of the time—is uncreative, or algorithmic, or boring, or trivial. Kuhn's view is that scientific creativity often consists in understanding how a new problem posed to us by nature can be seen as similar to a different problem that we already know how to solve. Galileo began by discovering what happens when a ball is rolled down a slope. When it travels back up another slope it returns to very nearly the same height as that from which it was released, regardless of how steep that second slope might be. He then learned to see the swinging motion of a pendulum as similar to the return of a rolling ball to its release height. A real pendulum has a large weight at its bottom end, but the rod or string that the large weight is attached to also swings, and it, too, has mass. The Dutch natural philosopher Christiaan Huygens later saw that it would be possible to understand the detailed motion of the whole pendulum as if it were composed of a series of connected pendulums, arranged along the line of the string, or rod. In other words, he learned to see a single real pendulum as a collection of simpler Galilean pendulums. Huygens treated Galileo as an exemplar, and Kuhn thinks of Huygens's work as a piece of normal science because of that.[8] But Kuhn also regards Huygens's work as creative, insightful, and important. Normal science is the artful adaptation of that which we already understand to that which we do not.

After a time, normal science may enter what Kuhn calls a "crisis" phase. In a crisis, problematic phenomena begin to accumulate, which no amount of creative work in the style of the agreed exemplars seems able to account for. Science enters a phase of self-doubt. Since scientists are no longer confident that recognizable styles of work will suffice to account for these troubling phenomena, they stop working in carefree emulation of their exemplars and begin to speculate about what proper scientific method should be like, and whether their exemplars have been correctly interpreted. In other words, they spend less time doing science and more time doing philosophy. Eventually, a new theory emerges, often fashioned by younger scholars who are not so enamored of the established exemplars. If this new theory can account for the anomalies left unexplained by previous theorizing, then eventually those old exemplars are cast off and new ones are anointed. A new phase of normal science begins. A scientific revolution has occurred.

What sort of episode does Kuhn have in mind when he describes the general pattern of a scientific revolution? Isaac Newton thought that space was a kind of substance—an infinitely large container in which events might take place. His contemporary Gottfried Leibniz argued against this conception: on Leibniz's view there are physical things—a table, a chair—and we can say how they are related to each other spatially—the chair is one yard to the left of the table—but there is no need to think of space itself as a containing substance.

The Newtonian image of space as a substance seemed to receive a significant boost as later nineteenth-century physicists increasingly accepted the idea that light consists of waves. Sound waves travel through vibrations in air molecules: that is why sound cannot be transmitted through a vacuum. Waves in the sea travel via the up-and-down motion of water molecules. What material medium vibrates when light waves move from one place to another? Not air, for light can travel through a vacuum. It seemed to these physicists that light must travel through oscillations in the substance of space itself, a material without mass that they called the *luminiferous aether.*[9]

The problem was that numerous experiments of increasing ingenuity, designed to detect the luminiferous aether as the Earth moved through it, all failed, or at least they did not yield a decisive verdict in the aether's favor.[10] The aether had become an anomaly; it was something that dominant theories seemed committed to, and yet it could not be detected in any way. And in 1905, with the publication of Einstein's special theory of relativity, physicists converted very quickly to a view of light and space that did not require the aether, and which, more generally, did not require a Newtonian conception of space as an infinitely large container in which physical events are situated. Einstein had effected what Kuhn would call a scientific revolution.

Incommensurability

Kuhn's language of scientific revolutions evokes images in the reader of religious conversion. Perhaps for that reason, Kuhn is often characterized as someone who thinks that the seismic changes in theory that accompany scientific revolutions are irrational: the scientist, it seems, must take a leap of faith, from the old worldview to the new. That impression is further encouraged by one of Kuhn's most notorious assertions—namely, that theories within different paradigms are *incommensurable.* Kuhn himself flatly denies that scientific theory change is irrational, but we cannot understand why he does so until we see what Kuhn means by this notion of incommensurability.

What are the marks of a good piece of scientific theorizing? And how are we to decide when one theory is better than another? As we have seen, Kuhn takes it that normal science is guided by shared exemplars. In endorsing a given piece of scientific work as exemplary, a community of scientists holds up *that* publication—Newton's *Principia,* Darwin's *Origin of Species,* Mendel's work on peas—as setting the standard for quality. If Kuhn is right that exemplars set standards in this way, and if he is right that exemplars change during scientific revolutions, it immediately follows that the very question of what counts as a good piece of science will change after a revolution. This is what Kuhn means when he says that changes in theory across revolution are incommensurable: they have no common measure by which to assess their merits, because standards are informed by exemplars, and exemplars are not constant.

Kuhn thinks that exemplars determine scientific standards in a variety of ways. He is quite emphatic that some very general criteria for assessment persist across scientific revolutions: scientists across all times prefer theories that predict phenomena with accuracy, they prefer theories that are simple, they prefer theories that are plausible in the light of what is already part of established scientific knowledge, they prefer theories that are consistent. Even so, let us focus on just one of these general criteria for quality. What do we mean when we say that a theory is simple? Do we mean it is easy to work with? Do we mean it asserts the existence of very few new theoretical entities? Do we mean the relationships the entities stand in can be modeled using equations of an elegant form?

What is more, the virtues that persist across revolutions will rarely all pull in the same direction. Suppose we must choose between two theories. One is mathematically elegant but seems highly implausible in the light of existing knowledge. Another fits well with what is already known but can be stated only with ugly equations. Which theory should we prefer? Does simplicity trump plausibility, or vice versa? Kuhn's idea is that a scientific community's commitment to following one particular set of exemplars will inform these issues of interpreting the meaning of individual standards and deciding how to balance competing standards against each other. It seems, then, that there is no neutral way to assess, for example, the standing of quantum theory as it was put forward in the early twentieth century. Part of what was at stake was whether its predictive power should override difficulties in understanding what it might mean and how it might be integrated with other areas of physics. Different scientific traditions weigh these factors in different ways.

These are the sorts of themes Kuhn stresses with his talk of incommensurability, but he is careful to limit their significance. When scientists disagree, Kuhn claims that logic will not tell them which theory is to be preferred over the other. There is no deductive procedure that determines, for example, how simplicity should be understood and how simplicity should be weighed against plausibility. Kuhn does not conclude from this that scientific theory change is irrational, or akin to a blind leap of faith. Instead, his claim is that when scientists make these decisions, they employ a form of skilled judgment, one that cannot be understood as the mechanical application of a logical algorithm. This sort of skilled judgment can be rational and reasonable, and ultimately it can sway dissenters.

Suppose I measure the height of my two children using two different rulers. I discover that one is 120 centimeters tall, the other is 3 feet and 2 inches. Which is the taller? Evidently the fact that one height is recorded in the metric system, the other in imperial, does not pose too much of a problem for my comparison, for I need only translate them into the same units. Similarly, one might imagine that so long as we can find a way to translate the findings of one paradigm into the language of another, we will be able to compare them bit-by-bit. We will have no problem in judging Einstein's system as superior to that of Newton, for we can offer an interpretation of Newton's work in Einstein's language.

Especially in Kuhn's later work, he regularly expresses the notion of incommensurability in terms of the limits of translation.[11] He illustrates these problems using the example of the French adjective *doux*. It is hard to make a case for our ability to translate that term *perfectly* into English.[12] While a French speaker calls a pillow *doux,* an English speaker would say it is soft; while he calls butter *doux,* an English speaker would say it is unsalted; while he calls wine *doux,* an English speaker would say it is sweet; while he calls the actions of a child *doux,* an English speaker would say they are gentle. What is more, to the French ear the term *doux* is not *ambiguous:* it is not like the English term *bank,* which has two entirely distinct meanings (namely, the place where you deposit your money, and the place by the side of a river). Instead, *doux* has a single meaning in French, one that is far broader then the meaning of any corresponding English term.

We should agree, then, that a term like *doux* cannot be translated perfectly into English, for no single word in English will bring with it the same broad range of resonances conveyed by the French term. The meanings of key scientific terms like *mass* or *gene* also differ as we move from the theories of Newton to the theories of Einstein, or as we move from Mendel's advocates at the beginning of the twentieth century (who knew nothing of the internal nature of chromosomes), through the work of Watson, Crick, and others on the double-helical structure of DNA, and on to the molecular biology of the present day. Kuhn's thought is that just as we cannot convey the full content of French judgments about unsalted butter using English, so we cannot convey the full content of Newton's outlook using the language of Einstein.

Once again, at the same time as stressing that the impossibility of perfect translation contributes to the incommensurability of distinct paradigms, Kuhn is also careful to contain the significance of this point. Even if French cannot be translated perfectly into English, it is possible for French and English people to communicate with each other adequately, and it is possible to formulate serviceable English translations of French texts. What is more, the failure of perfect translation does not destroy the ability of French and English speakers to disagree with each other, and to settle their disagreements to the satisfaction of both parties. If I am convinced that the waiter is going to bring us salted butter, but my French friend Philippe thinks the butter will be *doux,* then we can decide who is right by tasting some when it arrives. Likewise, Kuhn says that in spite of the fact that two scientists operating within different paradigms cannot translate their work perfectly into the language of the other, this does not mean they cannot understand each other, and it does not mean that they cannot devise experimental procedures that will determine, to the satisfaction of all parties, which paradigm is the better.[13]

Different Worlds

Kuhn does not think that scientists are trapped in bubbles inflated by their own theorizing, which prevent them from understanding, talking with, or persuading the occupants of alternative theoretical bubbles. For the most part his detailed views are altogether more sober. That said, things get more exotic in *Structure*'s famous tenth chapter, for here Kuhn argues that revolutionary changes from one paradigm to another have the most profound effects imaginable.

Kuhn's own early experiences of delving into ancient works of science led him to the view that the universe itself is transformed for investigators working in different paradigms. When Kuhn was preparing his first lecture course in the history of science, he read Aristotle's *Physics* (a work written in the fourth century BC) in a naive effort to find out "how much mechanics Aristotle had known, how much he had left for people like Galileo and Newton to discover." At first, the entirely unsurprising conclusion that Kuhn came to was that, in spite of his formidable reputation, Aristotle had known nothing of modern science. Worse, Aristotle's work was incomprehensible and incompetent. But after a little time mulling over Aristotle's claims, Kuhn experienced a revelatory transformation in his vision:[14]

> I was sitting at my desk with the text of Aristotle's *Physics* open in front of me and with a four-colored pencil in my hand. Looking up, I gazed abstractedly out of the window of my room—the visual image is one I still retain. Suddenly the fragments in my head sorted themselves out in a new way, and fell into place together. My jaw dropped, for all at once Aristotle seemed a very good physicist indeed, but of a sort I'd never dreamed possible.

Much later in *Structure,* Kuhn would generalize from his personal Aristotelian gestalt-shift, telling his readers that "after a revolution, scientists work in a different world."[15]

It is primarily because of remarks such as this one that Kuhn has been called a *relativist.* He seems to be telling us not merely that scientific ideas about the world change when one theory replaces another, but that the world itself—the very object science seeks to investigate—changes with that revolution. On this view, competing theories do not offer alternative understandings of the same universe; instead, the nature of the universe depends on the theory used to describe it. Why would Kuhn say such a thing?

It is not always clear whether Kuhn does say anything quite so radical, for the language he uses slips between mild and strong claims:

> Do we, however, really need to describe what separates Galileo from Aristotle, or Lavoisier from Priestley, as a transformation of vision? Did these men really *see* different things when *looking at* the same sorts of objects? Is there any legitimate sense in which we can say that they pursued their research in different worlds?[16]

Here Kuhn asks whether two scientists in the grip of different theories literally see things differently, or whether instead they see things in precisely the same ways, while coming to different conclusions about the significance of what they see. Kuhn thinks we should embrace the first option: he thinks theoretical commitments make a difference to how we see things. His argument draws in large part on work in the psychology of vision. If you put on special goggles that invert the image of the world arriving at your retina, initially everything will appear upside down. You will be disoriented and clumsy. But after a while you learn to compensate for the odd effects of the goggles, and things will look just the same as they did before you put the goggles on. Once habituated in this way, you will find that it is only when you remove the goggles that things once again seem the wrong way around.

Kuhn is on safe ground, then, in thinking that our visual experience is plastic; that is, how we see things can be altered over the course of our lives. More specifically, it can be transformed by our beliefs: if a few playing cards in a standard deck are doctored so that, for example, the queen of hearts is black, or the four of spades is red, then, so long as people are exposed to them reasonably briefly, they will not notice anything untoward, and will instead identify these anomalous cards as a normal red queen of hearts or a normal black four of spades. Our expectations for how things are—in this case, our familiarity with a standard deck of playing cards—make a difference to how things appear to us.

Technical training can also affect how things look: as the philosopher Ian Hacking has stressed, whereas the layperson looks at an X-ray image and sees only blobs, some of which may be suggestive of bone, the experienced doctor looks at the same image and a diagnosis leaps from the picture. She sees a tumor where we see nothing, or just a blur.[17] Kuhn's view, then, is that training and theoretical convictions make a difference not just to the conclusions scientists draw from their microscope slides, or from their telescopes, but to how they see the world they investigate with these instruments. Even so, there is a significant leap from Kuhn's mild notion that two scientists "see different things," in the sense that things look different to them, to the far stronger notion that they literally pursue their research "in different worlds."

Occasionally we get the impression that Kuhn's assertion that different scientists work "in different worlds" is merely a colorful way of making vivid his more basic, and far less contentious, conviction that the world starts to look different when your theoretical commitments have changed. But Kuhn's claim about the world changing after a scientific revolution is more than just a *façon de parler.* To understand why, we need to examine the appeal that the views of the eighteenth-century German philosopher Immanuel Kant held for Kuhn.

Kuhn's Kantianism

By and large, people agree on what colors things are. Most of us would say that ripe tomatoes are red and that grass is green. Sometimes we make mistakes about color—perhaps we look too quickly, perhaps we are viewing things under peculiar forms of illumination—but still we can correct ourselves by looking more carefully, or by taking objects into a source of natural light. In spite of all this, many scientists and philosophers (although certainly not all) would argue that colors do not exist in objects themselves.[18] Instead, they hold that colors are artifacts of human visual perception. Colors are something that objects *appear* to have, but this appearance is merely a consequence of how the human visual system processes information arriving at the eyes. Colors, on this view, are not genuine properties of material things. Nonetheless, because humans largely share the same perceptual systems, we have fairly robust standards for what count as the "true" colors of objects.

The nature of color, on this view, is not something that exists independently of experience. As a *very* rough simplification, we can say that Kant had similar thoughts about space and time. They, too, said Kant, are not features of the universe that exist independently of human experience. Kant thought that this radical proposal helped to explain some puzzling features of geometry. Up until the end of the nineteenth century, Euclidean geometry was widely thought to give an accurate description of the nature of space. But Euclidean geometry also seems to be an activity that one can do entirely from the armchair: one does not need to set up experiments to show that the angles of a triangle add up to 180 degrees. How is it possible that a science can, at one and the same time, tell us about the nature of space and yet demand no significant interaction with the world? Why don't we need to do experiments to determine the nature of space? Kant's idea, defended in his 1781 work *The Critique of Pure Reason,* was that this puzzle could be resolved if we thought of the properties of space as, in some sense, arising not from nature in itself but from how humans experience things.

Kuhn embraces a form of Kantianism. For Kuhn, the world itself does not exist independently of the way we experience it, and, as we have already seen, he also believes that the way we experience the world is affected by our scientific theories:

> 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.[19]

Just as many philosophers have been tempted to deny that the world contains *real* colors, understood entirely independently of the way human perceivers tend to see them, so Kuhn sees no reason to posit a *real* world that is independent of the way human scientists tend to see it. Of course, given that most humans do tend to see colors in similar ways, it makes sense to say that someone has made a mistake if he tells us that grass is purple. But these standards of correctness are relative to human vision in general. Different species have different visual systems, giving rise to different capacities for visual discrimination and classification of surfaces. Most humans have three types of cone cells in their eyes (although some color-blind people have only two), whereas goldfish have four, and pigeons have five.[20] It is hard to know, then, what we might mean by talking of the *true* color of a flower, if that is to be understood independently of the species of organism that happens to be looking at it.

We have seen that Kuhn stresses that scientists see the world differently before and after revolutions. He thinks of this as akin to a shift in their perceptual systems. If we are talking about scientists who share a paradigm, Kuhn is happy to say that some have gotten things right while others have got it wrong. But he denies that there is a way things are, independent of all scientific theorizing. Just as standards for correctness in color attribution are species-relative, Kuhn thinks that standards for the correctness of claims about the world are paradigm-relative. That is why Kuhn thinks that the worlds in which scientists work change with paradigm shifts.

Evolutionary Progress

Kuhn's Kantianism also explains his views about scientific progress. One might think of progress in science as the provision of an increasingly detailed picture of how the universe is. But Kuhn denies that there is a way the universe is, understood independently of any group of scientists' views about how things are. In that sense, the universe is not a stable object of investigation, which science might eventually capture. Instead, in Kuhn's view, the universe is a moving target: as our paradigms change, the universe changes, too.

Kuhn cannot claim that scientific progress consists in gradual convergence over time on stable facts about our universe, for he denies there are stable facts about the universe. How, then, can Kuhn make sense of progress at all? In *Structure*'s final chapter, entitled "Progress Through Revolutions," Kuhn invokes Darwin to illustrate his views. Kuhn hopes that an analogy with Darwinian evolution will help him explain to readers what progress might mean, if it is not progress toward some stable form of truth. Kuhn contends that Darwin, too, thought that evolution was progressive, and that Darwin, too, thought that evolutionary processes do not begin with some stable goal, specified in advance.[21]

Suppose we ask "How should a species ultimately end up, if it evolves by natural selection in a grassland environment?" There is simply no good answer if we pose our question in such a bald way. Even if we think that natural selection leads to progress via slight improvements, the question of what an improvement might look like in such an environment depends on whether we are talking about a large grazing mammal, an insect parasite, or a bird of prey. Moreover, the grassland environment itself is not fixed: species change their environments as they eat grass, as they produce dung, as they decompose, as they breathe.[22] Our question is a bad one, in part because we cannot say what counts as a forward move in the evolutionary game unless we specify what sort of a species we are talking about, in part because the environment of any species is a moving target.

Kuhn's idea, based largely on his Kantianism, is that when we ask what science is meant to conform to, we find that the universe it seeks to describe is also a moving target; and when we ask what counts as an improvement to a scientific theory, the answer depends on how that theory construes the world. Even so, says Kuhn, just as it makes sense to think that natural selection favors those organic variants that are slight improvements on what went before, so scientific communities prefer the theories that offer better solutions than their predecessors to the problems they address. Kuhn rejects the notion that science provides an increasingly accurate picture of a world whose structure is independent of what we happen to think about it. Still, science makes progress. Finally we can understand why Kuhn looked back on his work and described it "as a sort of post-Darwinian Kantianism."[23]

Evaluating Kuhn

This chapter has aimed to encourage an understanding of, and sympathy for, Kuhn's image of the processes of science. How well do Kuhn's views hold up?

For Kuhn, normal science and revolutionary science are very different in kind. Normal science consists of what he calls "puzzle solving"—that is, taking on problems, confident that the creative adaptation of respected exemplars will eventually yield solutions. After a revolution, the old exemplars are rejected, and new ones anointed. Kuhn says that when—and only when—revolutions happen, worlds change. In spite of the considerable ingenuity and importance that accompany innovations in normal science, discoveries of this more modest sort leave the world intact.

If revolutionary science and normal science are qualitatively distinct in these ways, it had better be the case that we can tell if we are dealing merely with an exceptionally insightful piece of normal science, or if instead we are in the presence of a revolutionary *bouleversement*. While that distinction may seem intuitive enough when we are talking about theories of the cosmos itself—revolutions occur when the Earth is deposed from the center of the solar system, or when Newton is deposed in favor of Einstein—it is far less clear how we are supposed to apply Kuhn's scheme once we look away from physics and toward other sciences such as biology.

By any standard reckoning, Darwin's *Origin of Species* is an exemplary scientific work.[24] It is unusual in being read regularly by practicing biologists today, in spite of the fact that it is over 150 years old. When biologists squabble over contentious scientific issues, they often try to recruit Darwin to their team. But although Darwin's work is important, it is not clear that its publication amounted to a revolution in Kuhn's sense. And yet, if Darwin's work does not count as revolutionary, we must question whether Kuhn's distinction between normal science and revolutionary science can be applied in biology at all.

Soon after Darwin's book was published in 1859, natural historians quickly converted to the "transformist" view defended in that work. In other words, they were quickly persuaded that the species we see around the world are descended from a small number of common ancestors, which had undergone a series of gradual change over vast stretches of time. It would be tempting, then, to think Darwin's work must have been revolutionary in character, on the grounds that it effected a wholesale shift in how the organic world was understood. But Darwin was certainly not the first to suggest that distinct species might be related genealogically, and he was not even the first to provide evidence for this. The same idea had been tabled by French naturalists such as the Comte de Buffon and Geoffroy St. Hilaire earlier in the eighteenth and nineteenth centuries.[25] Transformism was an idea in common circulation in scientific circles, and the anonymous publication in Britain in 1844 of *Vestiges of the Natural History of*

Creation—fifteen years before the *Origin* was published—made it an idea widely discussed among the general public.[26]

Darwin's work had a swift impact on the scientific community, but it did so by marshaling a mass of evidence, of diverse sorts, in favor of transformism, and by laying out a persuasive case in its support. Darwin did an enormous amount to make transformism respectable and compelling to the scientific elite. While this outcome allows us to say that Darwin brought about significant changes in received scientific thinking, it does not mean that Darwin's work was revolutionary in the Kuhnian sense. Transformism was not remotely alien to natural historians who read the *Origin* when it first appeared.

While transformism was not a new idea, natural selection was. Darwin put it forward as a novel explanation for the exquisite adaptations we see in plants and animals. This part of Darwin's theory was distinct from the broader transformist notion that plants and animals are modified descendants of ancestors held in common. Perhaps it is in the formulation of this hypothesis—namely, that species become adapted to their environments through a process of competitive struggle—that the *Origin* earns the right to be considered a revolutionary work in Kuhn's sense?

There are a number of problems with this interpretation. First, although natural selection was a new idea, it was formed by the creative fusion of many old ideas that would have been familiar to Darwin's readers. Darwin presented natural selection as analogous to artificial selection, a phenomenon that all of his contemporaries would have known about via the conspicuous successes of animal breeders like Robert Bakewell in improving cattle and sheep.

Darwin argued that anything the breeder can do on the farm, nature can do better in the wild. He claimed that this "selection" was achieved as a consequence of wild populations expanding in a way that outstrips available food resources, with the result that only the very best adapted would survive. That idea, too, would have been familiar to those who, like Darwin, had read Thomas Malthus's *Essay on the Principle of Population*, published in 1798. It is hard to know, then, whether we should understand Darwin as combining preexisting elements of respected work in the innovative manner characteristic of normal science, or whether instead we should understand his insight as paradigm-busting. What is more, Darwin was not able to persuade many of his contemporaries that natural selection was an important agent of adaptive change.[27]

It is not difficult to see why Darwin had trouble selling the idea of natural selection. For example, a fairly negative review of the *Origin* by the Scottish engineer Henry Fleeming Jenkin asked why we should be so sure that iterated cycles of variation and selective competition are able to produce increasingly refined adaptations.[28] Why, for example, should we think that Darwin's principle of natural selection can explain, as Darwin assures us it can, increasing running speed in wolves? Suppose that beneficial variations arise rarely. Perhaps a few members of a population of wolves are born who can run a little faster than the others. They have more babies as a result. But because these beneficial variations are rare, the chances are that when one of these faster wolves finds a mate, that mate will run at an average speed. When this couple has a baby, its running speed will probably end up closer to the population average than that of its single speedy parent. This baby, too, is likely to mate with average runners. Over time, says Jenkin, the benefit that initially accrues from faster running will be washed away owing to these repeated cycles of mating with more average specimens.

Darwin thought he had an answer to Jenkin's challenge, but it is very different from the response we rely on today. Darwin thought that slight, beneficial variations were really rather common. He thought that faster-running wolves would regularly appear in the population. He also thought that the struggle for existence was so exceptionally intense that the more average wolves would perish before they could mate. Finally, he thought the tendency to produce faster offspring could itself be inherited, with the result that once selection began to favor fast running, it would amplify the number of wolves who could run even faster still.

Darwin's effort to answer Jenkin's challenge is far from the image we have of selection today.[29] Like Darwin, modern biologists take the view that Jenkin's mistake was to think that beneficial variation would be lost because of repeated cycles of mating. Unlike Darwin, they argue that the nature of genetic inheritance—evidently something Darwin could not have known about—allows beneficial variation to be preserved even when, for example, faster-running wolves mate with others who are more average. It requires refined mathematical apparatus to make this case, and Darwin himself never dealt with complex maths. In the end it was not until the 1920s, with the mathematization of evolutionary theory at the hands of people like the Cambridge statistician and geneticist Ronald A. Fisher, that natural selection began to be widely accepted among biologists as a potent force in evolution.[30]

In retrospect we can look at Darwin's book and say that it made a strong case for natural selection, but in reality natural selection was assured its place in the explanatory toolbox of practicing biologists by much later efforts of Fisher and others. In sum, it is difficult to understand the history of biology in

Kuhnian terms, for it is unclear whether a work like the *Origin* counts as introducing a revolution. Kuhn, remember, was a physicist by training, and his approach struggles to account for the broader diversity of scientific practice. In particular, his framework of grand paradigm shifts seems ill-suited to the explanation of changing theory in biology.

The Plurality of Exemplars

There is a final problem that arises when we try to approach biology in a Kuhnian fashion, and it has broader significance for Kuhn's treatment of exemplars. Kuhn himself seems to suggest that when revolutions occur, the old exemplars are discarded and replaced with new ones. But why should this be the case? After all, an exemplar is a concrete achievement—something to be emulated. Kuhn himself stresses that the mere fact that something is seen as admirable leaves open the question of exactly what makes it admirable, or exactly how it should be emulated. That, in turn, should make us wonder why old exemplars need to be cast aside altogether after a revolution. Might they not instead be continually reinterpreted as they recede further into history?

We have seen that Darwin's detailed account of the workings of natural selection was unlike the framework biologists use today. It was, for example, free of mathematics and it relied heavily on a notion of intense struggle to counter the problems posed by Jenkin. It alleged the inheritance not just of variations but of the capacity to produce variation in a given direction, and of course it made no mention of genes. For the modern biologist, the mathematical treatment of evolutionary processes, set against a background of genetic inheritance, which Fisher put forward in his landmark 1930 work *The Genetical Theory of Natural Selection,* is a primary exemplar of how evolutionary biology should be done. But none of this means that the *Origin* isn't also a primary exemplar, for the *Origin,* too, still offers an inspiring vision of how to construct an evidentially rich account of how species evolve over time—one that has a version of natural selection (albeit not quite the version we have today) at its core.

For Darwin himself, and for his Victorian contemporaries, Newton's *Principia* was also an exemplary work of science—not because Darwin wanted to find biological analogues of Newtonian mass, or Newtonian space, but because Darwin believed that Newton's work showed in general terms how one should go about constructing a persuasive case in favor of a novel hypothesis. Today we no longer think that Newton is right about cosmology—in that sense his work has been displaced—but it does not follow that Newton's work is no longer an exemplar of diligent scientific activity, in the same very general sense that it was exemplary for Darwin. We do not need to cast exemplars aside, even after the eclipse of what might seem, for a while, to be their most important achievements. If exemplars are indeed preserved and reinterpreted across great swaths of scientific history, it becomes harder to talk of wholesale paradigm changes.

There is much to admire in Kuhn's work, especially when it comes to his insistence that exemplars play a role in guiding science in a way that does not reduce scientific activity to the mechanical application of rules. But that does not mean we need to retain what is now Kuhn's most notorious idea. It is time to bring to a close the paradigm of revolutionary paradigm shifts.

Further Reading

The single most important thing to read is, of course, Kuhn's own most influential work. A fiftieth-anniversary edition has recently been published, with a very helpful introductory essay by Ian Hacking:

Thomas Kuhn, *The Structure of Scientific Revolutions, Fiftieth Anniversary Edition* (Chicago: University of Chicago Press, 2012).

Also worth reading is an important collection of later essays by Kuhn:

Thomas Kuhn, *The Road Since Structure: Philosophical Essays 1970–1993* (Chicago: University of Chicago Press, 2000).

For discussion between Kuhn, Popper, Lakatos, and others on matters covered in this chapter, see:

Imre Lakatos and Alan Musgrave, eds., *Criticism and the Growth of Knowledge* (Cambridge: Cambridge University Press, 1970).

Two very helpful books devoted to understanding Kuhn are:

Alexander Bird, *Thomas Kuhn* (London: Acumen, 2001).

Paul Hoyningen-Huene, *Reconstructing Scientific Revolutions: Thomas S. Kuhn's Philosophy of Science* (Chicago: University of Chicago Press, 1993).

For a fascinating, and very recent, study of Kuhn in his historical and institutional context, see:

Joel Isaac, *Working Knowledge: Making the Human Sciences from Parsons to Kuhn* (Cambridge, MA: Harvard University Press, 2012).