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The Kuhnian Revolution

Thomas Kuhn's *The Structure of Scientific Revolutions* (1970, first published in 1962) challenged the dominant popular and philosophical pictures of the history of science. Rejecting the formalist view with its normative stance, Kuhn focused on the activities of and around scientific research: in his work science is merely what scientists do. Rejecting steady progress, he argued that there have been periods of normal science punctuated by revolutions. Kuhn's innovations were in part an ingenious reworking of portions of the standard pictures of science, informed by rationalist emphases on the power of ideas, by positivist views on the nature and meaning of theories, and by Ludwig Wittgenstein's ideas about forms of life and about perception. The result was novel, and had an enormous impact.

One of the targets of *The Structure of Scientific Revolutions* is what is known (since Butterfield 1931) as "Whig history," history that attempts to construct the past as a series of steps toward (and occasionally away from) present views. Especially in the history of science there is a temptation to see the past through the lens of the present, to see moves in the direction of what we now believe to be the truth as more rational, more natural, and less needing of causal explanation than opposition to what we now believe. But since events must follow their causes, a sequence of events in the history of science cannot be explained teleologically, simply by the fact that they represent progress. Whig history is one of the common buttresses of too-simple progressivism in the history of science, and its removal makes room for explanations that include more irregular changes.

According to Kuhn, *normal science* is the science done when members of a field share a recognition of key past achievements in their field, beliefs about which theories are right, an understanding of the important problems of the field, and methods for solving those problems. In Kuhn's terminology, scientists doing normal science share a *paradigm*. The term, originally referring to a grammatical model or pattern, draws particular attention to

Box 2.1 The modernity of science

Many commentators on science have felt that it is a particularly modern institution. By this they generally mean that it is exceptionally rational, or exceptionally free of local contexts. While science's exceptionality in either of these senses is contentious, there is a straightforward sense in which science is, and always has been, modern. As Derek de Solla Price (1986 [1963]) has pointed out, science has grown rapidly over the past three hundred years. In fact, by any of a number of indicators, science's growth has been steadily exponential. Science's share of the US gross national product has doubled every 20 years. The cumulative number of scientific journals founded has doubled every 15 years, as has the membership in scientific institutes, and the number of people with scientific or technical degrees. The numbers of articles in many sub-fields have doubled every 10 years. These patterns cannot continue indefinitely – and in fact have not continued since Price did his analysis.

A feature of this extremely rapid growth is that between 80 and 90 percent of all the scientists who have ever lived are alive now. For a senior scientist, between 80 and 90 percent of all the scientific articles ever written were written during his or her lifetime. For working scientists the distant past of their fields is almost entirely irrelevant to their current research, because the past is buried under masses of more recent accomplishments. Citation patterns show, as one would expect, that older research is considered less relevant than more recent research, perhaps having been superseded or simply left aside. For Price, a "research front" in a field at some time can be represented by the network of articles that are frequently cited. The front continually picks up new articles and drops old ones, as it establishes new problems, techniques, and solutions. Whether or not there are paradigms as Kuhn sees them, science pays most attention to current work, and little to its past. Science is modern in the sense of having a present-centered outlook, leaving its past to historians.

Rapid growth also gives science the impression of youth. At any time, a disproportionate number of scientists are young, having recently entered their fields. This creates the impression that science is for the young, even though individual scientists may make as many contributions in middle age as in youth (Wray 2003).

a scientific achievement that serves as an example for others to follow. Kuhn also assumes that such achievements provide theoretical and methodological tools for further research. Once they were established, Newton's mechanics, Lavoisier's chemistry, and Mendel's genetics each structured research in their respective fields, providing theoretical frameworks for and models of successful research.

Although it is tempting to see it as a period of stasis, normal science is better viewed as a period in which research is well structured. The theoretical side of a paradigm serves as a *worldview*, providing categories and frameworks into which to slot phenomena. The practical side of a paradigm serves as a *form of life*, providing patterns of behavior or frameworks for action. For example, Lavoisier's ideas about elements and the conservation of mass formed frameworks within which later chemists generated further ideas. The importance he attached to measurement instruments, and the balance in particular, shaped the work practices of chemistry. Within paradigms research goes on, often with tremendous creativity – though always embedded in firm conceptual and social backdrops.

Kuhn talks of normal science as *puzzle-solving*, because problems are to be solved within the terms of the paradigm: failure to solve a problem usually reflects badly on the researcher, rather than on the theories or methods of the paradigm. With respect to a paradigm, an unsolved problem is simply an anomaly, fodder for future researchers. In periods of normal science the paradigm is not open to serious question. This is because the natural sciences, on Kuhn's view, are particularly successful at socializing practitioners. Science students are taught from textbooks that present standardized views of fields and their histories; they have lengthy periods of training and apprenticeship; and during their training they are generally asked to solve well-understood and well-structured problems, often with well-known answers.

Nothing good lasts forever, and that includes normal science. Because paradigms can only ever be partial representations and partial ways of dealing with a subject matter, anomalies accumulate, and may eventually start to take on the character of real problems, rather than mere puzzles. Real problems cause discomfort and unease with the terms of the paradigm, and this allows scientists to consider changes and alternatives to the framework; Kuhn terms this a period of *crisis*. If an alternative is created that solves some of the central unsolved problems, then some scientists, particularly younger scientists who have not yet been fully indoctrinated into the beliefs and practices or way of life of the older paradigm, will adopt the alternative. Eventually, as older and conservative scientists become marginalized, a robust alternative may become a paradigm itself, structuring a new period of normal science.

Box 2.2 Foundationalism

Foundationalism is the thesis that knowledge can be traced back to firm foundations. Typically those foundations are seen as a combination of sensory impressions and rational principles, which then support an edifice of higher-order beliefs. The central metaphor of foundationalism, of a building firmly planted in the ground, is an attractive one. If we ask why we hold some belief, the reasons we give come in the form of another set of beliefs. We can continue asking why we hold *these* beliefs, and so on. Like bricks, each belief is supported by more beneath it (there is a problem here of the nature of the mortar that holds the bricks together, but we will ignore that). Clearly, the wall of bricks cannot continue downward forever; we do not support our knowledge with an infinite chain of beliefs. But what lies at the foundation?

The most plausible candidates for empirical foundations are sense experiences. But how can these ever be combined to support the complex generalizations that form our knowledge? We might think of sense experiences, and especially their simplest components, as like individual data points. Here we have the earlier problems of induction all over again: as we have seen, a finite collection of data points cannot determine which generalizations to believe.

Worse, even beliefs about sense impressions are not perfectly secure. Much of the discussion around Kuhn's *The Structure of Scientific Revolutions* (1970 [1962]) has focused on his claim that scientific revolutions change what scientists observe (Box 2.3). Even if Kuhn's emphasis is wrong, it is clear that we often doubt what we see or hear, and reinterpret it in terms of what we know. The problem becomes more obvious, as the discussion of the Duhem–Quine thesis (Box 1.2) shows, if we imagine the foundations to be already-ordered collections of sense impressions.

On the one hand, then, we cannot locate plausible foundations for the many complex generalizations that form our knowledge. On the other hand, nothing that might count as a foundation is perfectly secure. We are best off to abandon, then, the metaphor of solid foundations on which our knowledge sits.

According to Kuhn, it is in periods of normal science that we can most easily talk about progress, because scientists have little difficulty recognizing each other's achievements. Revolutions, however, are not progressive, because they both build and destroy. Some or all of the research structured by the pre-revolutionary paradigm will fail to make sense under the new regime; in fact Kuhn even claims that theories belonging to different paradigms are *incommensurable* – lacking a common measure – because people working in different paradigms see the world differently, and because the meanings of theoretical terms change with revolutions (a view derived in part from positivist notions of meaning). The non-progressiveness of revolutions and the incommensurability of paradigms are two closely related features of the Kuhnian account that have caused many commentators the most difficulty.

If Kuhn is right, science does not straightforwardly accumulate knowledge, but instead moves from one more or less adequate paradigm to another. This is the most radical implication found in *The Structure of Scientific Revolutions*: Science does not track the truth, but creates different partial views that can be considered to contain truth only by people who hold those views!

Kuhn's claim that theories within paradigms are incommensurable has a number of different roots. One of those roots lies in the positivist picture of meaning, on which the meanings of theoretical terms are related to observations they imply. Kuhn adopts the idea that the meanings of theoretical terms depend upon the constellation of claims in which they are embedded. A change of paradigms should result in widespread changes in the meanings of key terms. If this is true, then none of the key terms from one paradigm would map neatly onto those of another, preventing a common measure, or even full communication.

Secondly, in *The Structure of Scientific Revolutions*, Kuhn takes the notion of indoctrination quite seriously, going so far as to claim that paradigms even shape observations. People working within different paradigms see things differently. Borrowing from the work of N. R. Hanson (1958), Kuhn argues there is no such thing, at least in normal circumstances, as raw observation. Instead, observation comes interpreted: we do not see dots and lines in our visual fields, but instead see more or less recognizable objects and patterns. Thus observation is guided by concepts and ideas. This claim has become known as the *theory-dependence of observation*. The theory-dependence of observation is easily linked to Kuhn's historical picture, because during revolutions people stop seeing one way, and start seeing another way, guided by the new paradigm.

Finally, one of the roots of Kuhn's claims about incommensurability is his experience as an historian that it is difficult to make sense of past scientists' problems, concepts, and methods. Past research can be opaque, and aspects of it can seem bizarre. It might even be said that if people find it too easy to understand very old research in present terms they are probably doing some interpretive violence to that research – Isaac Newton's physics looks strikingly modern when rewritten for today's textbooks, but looks much less so in its originally published form, and even less so when the connections between it and Newton's religious and alchemical research are drawn (e.g. Dobbs and Jacob 1995). Kuhn says that "In a sense that I am unable to explicate further, the proponents of competing paradigms practice their trades in different worlds" (1970 [1962]: 150).

The case for semantic incommensurability has attracted a considerable amount of attention, mostly negative. Meanings of terms do change, but they probably do not change so much and so systematically that claims in which they are used cannot typically be compared. Most of the philosophers, linguists, and others who have studied this issue have come to the conclusion that claims for semantic incommensurability cannot be sustained, or even that it is impossible (Davidson 1974) to make sense of such radical change in meaning (see Bird 2000 for an overview).

This leaves the historical justification for incommensurability. That problems, concepts, and methods change is uncontroversial. But the difficulties that these create for interpreting past episodes in science can be overcome – the very fact that historical research can challenge present-centered interpretations shows the limits of incommensurability.

Claims of radical incommensurability appear to fail. In fact, Kuhn quickly distanced himself from the strongest readings of his claims. Already by 1965 he insisted that he meant by "incommensurability" only "incomplete communication" or "difficulty of translation," sometimes leading to "communication breakdown" (Kuhn 1970a). Still, on these more modest readings incommensurability is an important phenomenon: even when dealing with the same subject matter, scientists (among others) can fail to communicate.

If there is no radical incommensurability, then there is no radical division between paradigms, either. Paradigms must be linked by enough continuity of concepts and practices to allow communication. This may even be a methodological or theoretical point: complete ruptures in ideas or practices are inexplicable (Barnes 1982). When historians want to explain an innovation, they do so in terms of a reworking of available resources. Every new idea, practice, and object has its sources; to assume otherwise is to invoke something akin to magic. Thus many historians of science have challenged Kuhn's paradigms by showing the continuity from one putative paradigm to the next.

For example, instruments, theories, and experiments change at different times. In a detailed study of particle detectors in physics, Peter Galison (1997) shows that new detectors are initially used for the same types of experiments and observations as their immediate predecessors had been, and fit into the same theoretical contexts. Similarly, when theories change, there is no immediate change in either experiments or instruments. Discontinuity in one realm, then, is at least generally bounded by continuity in others. Science gains strength, an *ad hoc* unity, from the fact that its key components rarely change together. Science maintains stability through change by being disunified, like a thread as described by Wittgenstein (1958): "the strength of the thread does not reside in the fact that some one fibre runs through its whole length, but in the overlapping of many fibres." If this is right then the image of complete breaks between periods is misleading.

Box 2.3 The theory-dependence of observation

Do people's beliefs shape their observations? Psychologists have long studied this question, showing how people's interpretations of images are affected by what they expect those images to show. Hanson and Kuhn took the psychological results to be important for understanding how science works. Scientific observations, they claim, are theory-dependent.

For the most part, philosophers, psychologists, and cognitive scientists agree that observations can be shaped by what people believe. There are substantial disagreements, though, about how important this is for understanding science. For example, a prominent debate about visual illusions and the extent to which the background beliefs that make them illusions are plastic (e.g. Churchland 1988; Fodor 1988) has been sidelined by a broader interpretation of "observation." Scientific observation has been and is rarely equivalent to brute perception, experienced by an isolated individual (Daston 2008). Much scientific data is collected by machine, and then is organized by scientists to display phenomena publicly (Bogen and Woodward 1992). If that organization amounts to observation, then it is straightforward that observation is theory-dependent.

Theory and practice dependence is broader even than that: scientists attend to objects and processes that background beliefs suggest are worth looking at, they design experiments around theoretically inspired questions, they remember relevance and communicate relevant information, where relevance depends on established practices and shared theoretical views (Brewer and Lambert 2001).

Incommensurability: Communicating Among Social Worlds

Claims about the incommensurability of scientific paradigms raise general questions about the extent to which people across boundaries can communicate.

In some sense it is trivial that disciplines (or smaller units, like specialties) are incommensurable. The work done by a molecular biologist is not obviously interesting or comprehensible to an evolutionary ecologist or a neuropathologist, although with some translation it can sometimes become so. The meaning of terms, ideas, and actions is connected to the cultures and practices from which they stem. Disciplines are "epistemic cultures" that may have completely different orientations to their objects, social units of knowledge production, and patterns of interaction (Knorr Cetina 1999). However, people from different areas interact, and as a result science gains a degree of unity. We might ask, then, how interactions are made to work.

Simplified languages allow parties to trade goods and services without concern for the integrity of local cultures and practices. A *trading zone* (Galison 1997) is an area in which scientific and/or technical practices can fruitfully interact via these simplified languages or *pidgins*, without requiring full assimilation. Trading zones can develop at the contact points of specialties, around the transfer of valuable goods from one to another. In trading zones, collaborations can be successful even if the cultures and practices that are brought together do not agree on problems or definitions.

The trading zone concept is flexible, perhaps overly so. We might look at almost any communication as taking place in a trading zone and demanding some pidgin-like language. For example, Richard Feynman's diagrams of particle interactions, which later became known as Feynman diagrams, were successful in part because they were simple and could be interpreted in various ways (Kaiser 2005). They were widely spread during the 1950s by visiting postdoctoral fellows and researchers. But different schools, working with different theoretical frameworks, picked them up, adapted them, and developed local styles of using them. Despite their variety, they remained important ways of communicating among physicists, and also tools that were productive of theoretical problems and insights. It would seem to stretch the "trading zone" concept to say that Feynman diagrams were parts of pidgins needed for theoretical physicists to talk to each other, yet that is what they look like.

A different, but equally flexible, concept for understanding communication across barriers is the idea of *boundary objects* (Star and Griesemer 1989). In a historical case study of interactions in Berkeley's Museum of Vertebrate Zoology, Susan Leigh Star and James Griesemer focus on objects, rather than languages. The different social worlds of amateur collectors, professional scientists, philanthropists, and administrators had very different visions of the museum, its goals, and the important work to be done. These differences resulted in incommensurabilities among groups. However, objects can form bridges across boundaries, if they can serve as a focus of attention in different social worlds, and are robust enough to maintain their identities in those different worlds.

Standardized records were among the key boundary objects that held together these different social worlds. Records of the specimens had different meanings for the different groups of actors, but each group could contribute to and use those records. The practices of each group could continue intact, but the groups interacted via record keeping. Boundary objects, then, allow for a certain amount of coordination of actions without large measures of translation.

The boundary object concept has been picked up and used in an enormous number of ways. Even within the article in which they introduce the concept, Star and Griesemer present a number of different examples of boundary objects, including the zoology museum itself, the different animal species in the museum's scope, the state of California, and standardized records of specimens.

The concept has been applied very widely in STS. To take just a few examples: Sketches and drawings can allow engineers in different parts of design and production processes to communicate across boundaries (Henderson 1991). Parameterizations of climate models, the filling in of variables to bring those models in line with the world's weather, connect field meteorologists and simulation modelers (Sundberg 2007). In the early twentieth century breeds of rabbits and poultry connected fanciers to geneticists and commercial breeders (Marie 2008).

Why are there so many different boundary objects? The number and variety suggest that, despite some incommensurability across social boundaries, there is considerable coordination and probably even some level of communication. For example, in multidisciplinary research a considerable amount of communication is achieved via straightforward translation (Duncker 2001). Researchers come to understand what their colleagues in other disciplines know, and translate what they have to say into a language that those colleagues can understand. Simultaneously, they listen to what other people have to say and read what other people write, attuned to differences in knowledge, assumptions, and focus. Concepts like pidgins, trading zones, and boundary objects, while they might be useful in particular situations, may overstate difficulties in communication. Incommensurability as it is found in many practices may not always be a very serious barrier.

The divisions of the sciences result in disunity (see Dupré 1993; Galison and Stump 1996). A disunified science requires communication, perhaps in trading zones or direct translation, or coordination, perhaps via boundary objects, so that its many fibers are in fact twisted around each other. Even while disunified, though, science hangs together and has some stability. How it does so remains an issue that merits investigation.

Conclusion: Some Impacts

The Structure of Scientific Revolutions had an immediate impact. The word "paradigm," referring to a way things are done or seen, came into common usage largely because of Kuhn. Even from the short description above it is clear that the book represents a challenge to earlier important beliefs about science.

Against the views of science with which we started, *The Structure of Scientific Revolutions* argues that scientific communities are importantly organized around ideas and practices, not around ideals of behavior. And, they are organized from the bottom up, not, as functionalism would have it, to serve an overarching goal. Against positivism, Kuhn argued that changes in theories are not driven by data but by changes of vision. In fact, if worldviews are essentially theories then data is subordinate to theory, rather than the other way around. Against falsificationism, Kuhn argued that anomalies are typically set aside, that only during revolutions are they used as a justification to reject a theory. And against all of these he argued that on the largest scales the history of science should not be told as a story of uninterrupted progress, but only change.

Because Kuhn's version of science violated almost everybody's ideas of the rationality and progress of science, *The Structure of Scientific Revolutions* was sometimes read as claiming that science is fundamentally irrational, or describing science as "mob rule." In retrospect it is difficult to find much irrationalism there, and possible to see the book as somewhat conservative – perhaps not only intellectually conservative but politically conservative (Fuller 2000). More important, perhaps, is the widespread perception that by examining history Kuhn firmly refuted the standard view of science.

Whether or not that is true, Kuhn started people thinking about science in very different terms. The success of the book created a space for thinking about the practices of science in local terms, rather than in terms of their contribution to progress, or their exemplification of ideals. Though few of Kuhn's specific ideas have survived fully intact, *The Structure of Scientific Revolutions* has profoundly affected subsequent thinking in the study of science and technology.