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Thomas S. Kuhn, *The Structure of Scientific Revolutions* (1962)

"Relativism" Hits the Headlines

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It is, perhaps, a bizarre twist of fate that one of the most hotly debated books in twentieth-century philosophy (and perhaps its only real best-seller – recent reports number sales at more than a million copies in English alone, not counting its twenty-five translations) was written by a self-avowed non-philosopher. The story of Thomas Kuhn (1922–96) is the story of a physicist, turned historian of science, turned (somewhat unwillingly) the philosopher of science who was blamed for almost single-handedly creating a crisis of rationality in contemporary philosophy.

Kuhn's primary concern when writing *The Structure of Scientific Revolutions* (1962) was with the nature of scientific discovery, and, by implication, with our view of scientific progress. Nowhere did he intend to challenge the *rationality* of science as such. To his contemporaries, however, this was just what his book seemed to do. For what could Kuhn's forceful and polemical attack on time-honored notions such as truth, progress, and objectivity amount to, if not an attack on science itself? In short, would not Kuhn's insistence on radical discontinuities and revolutionary upheavals in the history of science, together with his emphasis on the contingency inherent in every research situation, seem to project the specter of relativism on to that supposed bastion of rationality, natural science? And if natural science is not free from such factors, can any other sphere of human inquiry claim immunity?

Paradigms and Normal Science

To challenge the then dominant "development-by-accumulation" view of scientific progress, Kuhn characterizes mature science as having passed through a number of distinct stages. All candidates for the status of "scientific discipline" must first undergo a period of "immaturity" before becoming a science proper. This transition to mature science coincides with the discipline's reception of its first *paradigm*. This term, introduced by Kuhn in *The Structure of Scientific Revolutions* (henceforth *SSR*), has become common currency in academic discourse. The notion is notoriously difficult to delimit and has taken on a life of its own apart from his writings. In *SSR*, however, it was intended to capture something along the lines of an *exemplary achievement*: a work that serves "implicitly to define the legitimate problems and methods of a research field for succeeding generations of practitioners" (*SSR*, 10). Frequently cited examples of such works are Newton's *Principia* and Lavoisier's *Chemistry*.

Science practiced under a paradigm is what Kuhn refers to as *normal science*. The transition from immaturity to normal science is marked by a distinct narrowing of scope; for every paradigm rigidly defines both what is to count as a problem and the range of acceptable solutions. The transition to normal science can only happen when scientists have at their disposal clear-cut criteria of relevance. These are what enable them collectively to work on one set of problems while discarding others.

Kuhn describes the tasks of normal science as threefold: (1) the determination of significant facts; (2) the matching of facts with theory; and (3) theory-articulation. All three are, in Kuhn's distinctive parlance, "mop-up work," and nowhere do they constitute the kind of "testing" of the theory that was deemed so important on the traditional view. Normal science simply does not aim at producing major novelty: "Instead, its object is to solve a puzzle for whose very existence the validity of the paradigm must be assumed. Failure to achieve a solution discredits only the scientist and not the theory" (*SSR*, 80).

Anomaly and Crisis

Yet, although innovation is not the *aim* of normal scientific research, we can rest assured that novelty will not be suppressed forever. Over time, research will produce *anomalies* – facts that thwart theoretical expectations and cannot readily be assimilated into the paradigm. The existence of anomaly does not by itself constitute sufficient reason to discard a paradigm (after all, no scientific theory, no matter how successful, has ever explained all phenomena relevant to it). The accumulation of anomaly will, however, induce in scientists a greater willingness to articulate and investigate alternative ways of accounting for the recalcitrant phenomena. The continued resistance of anomaly to traditional modes of accommodation forces scientists to bend the paradigmatic applications in ways that under normal circumstances would seem arbitrary and *ad hoc*. *Extraordinary science* comes about when the discipline enters a period of *crisis*. In such a situation, "the rules of normal science become increasingly blurred. Though there is still a paradigm, few practitioners prove to be entirely agreed upon what it is" (*SSR*, 80).

This state of crisis will resolve in one of three ways: (1) normal scientific resources will ultimately prove able to account for the seeming anomaly; (2) the problem will be deemed insoluble for the present time and set aside for future research; or (3) a new candidate for paradigm will emerge, and with it the ensuing battle over its acceptance (*SSR*, 84). This last possibility is, needless to say, the one that is of most interest to

Kuhn. This, in his opinion, is what traditional philosophy of science has crucially overlooked. If a crisis ends with the acceptance of a new paradigm, a *scientific revolution* will have taken place.

Scientific Revolutions and Incommensurability

Nothing in the foregoing account is in principle incompatible with the story of scientific development as cumulative. The notion of a scientific revolution was in circulation long before Kuhn, and was used to designate particularly great scientific advances, for example, the Copernican revolution. Kuhn did, however, introduce a number of twists in the tale, ultimately provoking some critics to outrage. The revolutionary transition from paradigm to paradigm should not be called a cumulative process, he argues: "Rather, it is a reconstruction of the field from new fundamentals . . . When the transition is complete, the profession will have changed its view of the field, its methods, and its goals" (*SSR*, 94).

Kuhn is far from oblivious to the political connotations of his choice of terminology, and explores these to find interesting parallels to scientific development. Political and scientific revolutions alike start from a growing awareness within an estranged subcommunity that existing institutions fail to deal satisfactorily with a certain set of problems. Seeking redress for their grievances, "revolutionaries" opt to change these political institutions in ways that the institutions themselves prohibit, or even to overthrow them altogether. In the interim between old and new, the community is inevitably split in two, and communication across the divide will be only partial. Since standards of adequacy and failure are exactly what are under dispute, and since neither party acknowledges any supra-institutional framework, standard forms of political recourse necessarily fail. Applying this political analogy to revolution in science, Kuhn observes that both involve a special kind of choice:

> Like the choice between competing political institutions, that between paradigms proves to be a choice between incompatible modes of community life. Because it has that character, the choice is not and cannot be determined merely by the evaluative procedures characteristic of normal science . . . Each group uses its own paradigm to argue in that paradigm's defense . . . The status of [this] circular argument is only that of persuasion. It cannot be made logically or even probabilistically compelling . . . As in political revolutions, so in paradigm choice – there is no standard higher than the assent of the relevant community. (*SSR*, 94)

As if this were not enough to infuriate his critics, Kuhn would then go on to introduce the notorious notion of *incommensurability.* Adopted from mathematics and meaning "no common measure," Kuhn used the term to pinpoint the predicament of paradigm choice in science. If we think of paradigms as languages expressing divergent cognitive commitments, two paradigms will then be incommensurable if there is no way of effecting a translation between them. Incommensurable paradigms may employ many of the same concepts (for example, space, motion, mass), but each paradigm will use them differently, since the meaning of these concepts is anchored in the theoretical structure of their respective paradigms. Kuhn thereby bids farewell to the time-honored idea that "nature" or "reality" (the supposed common measure) provides us with a neutral court of appeal, a court that would allow us unproblematically to decide between theories. "[P]aradigm changes do cause scientists to see the world of their research engagement differently. In so far as their only recourse to that world is through what they see and do, we may want to say that after a revolution scientists are responding to a different world" (*SSR*, 111).

Kuhn thus rejects the idea of a clear separation between fact and theory; between nature and the way we conceptualize it. We simply cannot analyze a paradigm to parcel out "empirical content" from "theoretical framework." A major scientific innovation does much more than simply alter the scientific community's interpretation of given data. Rather, the "proponents of competing paradigms practice their trades in different worlds" (*SSR*, 150). In other words, there exists no pre-established neutral ground by which cross-paradigmatic comparison may be effected. Nor will it be possible to decide between theories simply on the basis of their relative problem-solving ability. Different paradigms define differently what are the relevant problems that need to be solved.

Kuhn's views provoked much controversy among philosophers of science. His book was said to render theory choice in science *irrational*, or at least *arational*, and in Imre Lakatos's famous phrase, "*a matter for mob-psychology*" (Lakatos and Musgrave, 1970: 178). Kuhn's exploration of the political analogies to theory choice in science was not well received in the conservative halls of the American academy. Further chagrin was caused by the fact that Kuhn initially sought to explain incommensurability and scientific revolutions in terms of *gestalt switches* and *conversion experiences* (*SSR*, 111–14). While striking and even provocative, Kuhn himself later came to realize that drawing on concepts from individual psychology to explain what are ostensibly collective processes was clearly misguided. Moreover, *gestalt* switches and conversion experiences are typically described as non-composite events, instantaneous flashes with no rationally discernible internal structure. Small wonder, then, that Kuhn's critics were dumbfounded when he later came to describe scientific revolutions in terms of argumentation and negotiation, in order precisely to highlight their rational character.

Over the ensuing decades, Kuhn found in the philosophy of language a fruitful approach to the troublesome phenomenon of incommensurability, one better suited to the task than his earlier emphasis on individual psychology. This has done much to strengthen the general plausibility of his argument. Although the exact status of the incommensurability thesis remains a point of contention in the continuing debates over Kuhn's legacy, it is also one of the most influential aspects of his work, an idea that has reverberated through a number of academic disciplines.

Kuhn's View of Scientific Rationality

All in all, then, should we read Kuhn as asserting that paradigm changes in science have no rational basis whatsoever, as some of his critics have claimed? Of course not; scientists have numerous "good reasons" to appeal to in the period of transition between paradigms. Kuhn even cites five "characteristics of a good scientific theory"

(Kuhn, 1977: 321–2) – accuracy, consistency, broadness of scope, simplicity, and fruitfulness – as standard criteria in theory evaluation. The point remains, however, that these criteria, even in conjunction, cannot always dictate a unique conclusion. There will always be ample room for disputes about their application and relative merits. Additionally, one or several of these criteria may, and frequently will, conflict with the others. These criteria cannot, then, form anything like a neutral *algorithm* of theory choice; they can only function as *values* which inform, but not determine, our choices. The kind of unequivocal decision procedure that the received view took to be the essence of scientific rationality can only exist within the confines of a single scientific tradition, and cannot be used to legislate between competing traditions.

Is this a problem? Should we shudder at the thought that an understanding of theory choice in science must ultimately refer back to the group whose decision it is, in the sense of having to take into account values and commitments that are to some extent specific to this group? Does such an idea open the door to all kinds of subjective impulse and arbitrary fancy, thereby reducing scientific rationality to mob psychology? Kuhn, obviously, did not think so. As he aptly put it in a later text:

> [T]o describe [my] argument as a defence of irrationality in science seems to me not only absurd but vaguely obscene. I would describe it . . . as an attempt to show that existing theories of rationality are not quite right and that we must readjust them or change them to explain why science works as it does. To suppose, instead, that we possess criteria of rationality which are independent of our understanding of the essentials of the scientific process is to open the door to cloud-cuckoo land. (Kuhn, 2000: 159)

Science-as-praxis, on Kuhn's account, is not an imperfect, *ex post facto* approximation of "pure science," but is in fact an integral element in its development. To understand science, we must understand it as practiced, or not at all. Rationality and rational validation cannot be specified independently of this practice, and cannot automatically be applied across the board. Different practices at different times will have different notions of rational validation. Still, the incommensurability of highly technical languages of scientific inquiry does not leave us bereft of the means to communicate. Rather, incommensurability forces us to meditate on the fact that science – even science! – remains embedded in the larger context of community and history, where communication is never beyond our grasp.

The Popper–Kuhn Controversy

Karl Popper was perhaps the most influential philosopher of science at the time that *SSR* was published, and he and his followers were quick to take up the challenges that Kuhn's book posed. In 1965 a colloquium was arranged (documented in Lakatos and Musgrave, 1970) which saw a distinguished group of Popperians line up against what they perceived as the many dangerous shortcomings of Kuhn's work. In his contribution, Popper conceded that he had largely overlooked the phenomenon of normal science, having directed his attention almost exclusively to extraordinary science. That concession made, Popper goes on to claim that Kuhn's normal scientist cuts a sorry figure; he is a victim of indoctrination. Kuhn, Popper claims, has confused *is* and *ought*. Although Kuhn may have supplied a *descriptively* correct account of science, Popper, by comparison, meant his account to be *normative*. His "logic of discovery" attempts to lay out the proper procedure for scientific inquiry. The failure of normal science to conform to this norm only underscores the importance of a philosophy like his. The normalcy of normal science is, Popper writes, "a danger to science and indeed to our civilization" (Popper, 1970: 53).

Popper, apparently, wants science to develop through an unbroken string of revolutions. However, the danger of dogmatism that Popper sees as endemic to normal scientific research is, Kuhn claims by contrast, essential to science as we know it.

> Because they can ordinarily take current theory for granted, exploiting it rather than criticizing it, the practitioners of mature sciences are freed to explore nature to an esoteric depth and detail otherwise unimaginable. Because that exploitation will ultimately isolate severe trouble spots, they can be confident that the pursuit of normal science will inform them when and where they can most usefully become Popperian critics. (Kuhn, 2000: 141)

Popperian revolutions-in-permanence leave nothing to revolutionize. Only with the patience bred by confidence in the ultimate potential of the paradigm – a patience that cannot be instilled by proof or conclusive argument – can scientists afford to momentarily overlook the problems facing any new theory and thereby get to work on its broader articulation. Kuhn's positive and constructive point is that this gap between shared values and uniform decision is not a predicament but is in fact indispensable to scientific progress, however else we wish to define it.

Indeed, there are many ways in which Kuhn remained a firm believer in scientific progress. He believed, for one, that a successor paradigm was superior to its predecessor in terms of both theoretical sophistication and problem-solving ability. He refused to believe, however, that scientific progress can usefully be said to carry us ever closer to *the truth*. He makes this point by recalling Darwin's theory of evolution. The most controversial aspect of Darwin's work, Kuhn points out, was neither the notion of species change nor of man's descent from the apes. What bothered Darwin's contemporaries was the fact that his account had no teleology – *evolution ceased to be a goal-directed process*. Kuhn, then, invites us to see the progress of science as an evolution *from* primitive beginnings but *toward* no goal (*SSR*, 171).

Conclusion

Looking back over the forty years that have passed since *SSR* was first published, it seems clear that the work's most striking impact has been to ignite a number of fierce debates on relativism and rationality. These debates have surfaced in a number of disciplines, not just in philosophy, and they continue unabated to this day. Kuhn is commonly held to be one of the first thinkers of his generation to take an explicitly historical (or "hermeneutic") approach to the philosophical study of science, betraying a kinship to thinkers such as Collingwood and Gadamer. While that is true, we should not underestimate his affinities with philosophers pursuing a very nearly perpendicular axis, namely naturalists such as Wittgenstein, Quine, and Sellars. Indeed, the way in which *SSR* faces up to this tension between naturalistic and historical approaches to the philosophy of science may be a crucial key to explaining its originality and enduring appeal.

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