Kuhn-Loss, Persuasion, and Incommensurability Again -

Can Paradigm-Change be Rationally Justified?

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Introduction

Chapter 12 of *The Structure of Scientific Revolutions* is called "The Resolution of Revolutions". It deals with the post-revolutionary period in which a rival paradigm has been proposed and now the proponents of the new paradigm must persuade the holdouts supporting the dominant paradigm. Kuhn is frank that the skeptics may never be persuaded. What follows is an interpretation of his account of the resolution of revolutions in Chapter 12. Kuhn's position begins from the claim that experimental falsification is *not* the motivation for the proposal of a new paradigm. Instead, novel paradigms are developed to solve new problems in cases where doing so requires a new way of looking at and understanding the phenomena: an interlocked set of non-empirical assumptions (SSR, 147) all change at once. As a result, persuading adherents to the former paradigm is not a matter of presenting evidence, but of changing how they see and understand science. It may even mean persuading them to adopt new ways of doing and understanding science itself.

1. Paradigms and The New Historiography

Kuhn's explanation of paradigm shifts begins with discoveries that prompt a crisis within an existing paradigm. A period of "crisis science" emerges, in which scientists work to accommodate the new discovery along with any anomalies they have newly recognized (SSR, Chapter 7). Then, Kuhn continues, "[a]fter the discovery had been assimilated, scientists were able to account for a wider range of natural phenomena or to account with greater precision for some of those previously known. But that gain was achieved only by discarding some previously standard beliefs or procedures and, simultaneously, by replacing those components of the previous paradigm with others" (SSR, 66). The discarding of 'previously standard beliefs' is now known as Kuhn-loss.

Chapter 12 of *The Structure of Scientific Revolutions* centers around one question: "What is the process by which a new candidate for paradigm replaces its predecessor?" (SSR, 143). According to the rival 'development-by-accumulation' picture of science Kuhn discusses in the Introduction, called "A Role for History", science develops "by the accumulation of individual discoveries and inventions" (SSR, 2). For this account, new discoveries simply need to be made coherent with earlier ones, so that a cumulative framework for science can be constructed.

In the introduction, Kuhn appeals to the history of science to correct the 'development-by-accumulation' picture. There is a "historiographic revolution in the study of science" underway, Kuhn says, in the work of scholars such as Alexandre Koyré. Kuhn writes,

> Gradually, and often without entirely realizing they are doing so, historians of science have begun to ask new sorts of questions and to trace different, and often less than cumulative, developmental lines for the sciences. Rather than seeking the permanent contributions of an older science to our present vantage, they attempt to display the historical integrity of that science in its own time. They ask, for example, not about the relation of Galileo's views to those of modern science, but rather about the relationship between his views and those of his group, i.e., his teachers, contemporaries, and immediate successors in the sciences. (SSR, 3)

Although Kuhn does not mention it again, the concept of 'historical integrity' is crucial to his approach. Chapter 11 deals with scientific textbooks, which Kuhn sees as an important source of standards and methods for scientists. Kuhn's objection to 'textbook science' emerges only when textbooks are used to establish the historical record.[1] Instead of analyzing scientific research communities that emerged in specific conditions, textbooks often present "science" as a homogenized practice that everyone undertakes in essentially the same way. In a textbook, Galileo and Einstein engage in the same enterprise of 'physics' and Vesalius and Salk practice 'medical research' in the same way. The romantic picture painted by earlier histories of science and by textbooks describes science as a universal, timeless practice. While it unfolds over time, science to these earlier historians is not specific to a time in history.

Kuhn argues that these researchers are separated by more than years. The idea of a scientific paradigm is Kuhn's contribution to the new historiography.[2] The fact that previous researchers worked with a different paradigm is not just a historical curiosity. It is necessary to understanding their research. Scientific paradigms can be employed to understand scientific communities.[3] When we ask, as Kuhn says the new historians do, "about the relationship between [Galileo's] views and those of his group, i.e., his teachers, contemporaries, and immediate successors in the sciences", we ask about how Galileo's work was related to the dominant paradigm of his day. To say that we

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[1] Brorson and Andersen (2001) discuss Kuhn and Ludwik Fleck on scientific literature.
[2] For Kuhn and the 'new historiography of science', see Pinto de Oliveira 2012.
[3] I do not mean this statement to be a complete definition of a paradigm. My point is that Kuhn uses paradigms as tools for historical research. See Kindi 2012 for explanation of Kuhn's paradigms.

understand Galileo's work in relation to a paradigm is *not* to say that we explain Galileo's own discoveries as emanating from the paradigm. Instead, Galileo's discoveries can be properly understood as revolutionary only if we can see exactly how they challenge the dominant paradigm.

2. Challenging the Dominant Paradigm

The question of Chapter 12 is: "What is the process by which a new candidate for paradigm replaces its predecessor?" Kuhn's new historiography sees this process as taking place within a scientific community. He writes:

> Any new interpretation of nature, whether a discovery or a theory, emerges first in the mind of one or a few individuals. It is they who first learn to see science and the world differently [...] How are they able, what must they do, to convert the entire profession or the relevant professional subgroup to their way of seeing science and the world? What causes the group to abandon one tradition of normal research in favor of another? (SSR, 143)

Those who aim to 'convert' others to their new way of 'seeing science and the world' are usually younger scientists. They had some exposure to the existing paradigm during their training, but they are also increasingly aware that the paradigm does not account for everything. These younger people are more willing to abandon the dominant paradigm, Kuhn says, since they haven't invested as much in it.[4] It's not their age per se but their position in the profession that explains their willingness to try new approaches.[5]

Kuhn's explanation of the younger scientists' motivation to challenge a paradigm is often misunderstood. It is sometimes considered to be a species of Popperian theory testing: that the paradigm is 'put to the test' by constructing experiments that challenge it. When the paradigm fails a series of tests, on this Popperian account, newer scientists then abandon the paradigm. In Chapter 12, Kuhn explicitly distinguishes his approach from Popper's. While Kuhn does sometimes speak of theories failing experimental tests, he does not think falsification is the stimulus for reconsidering a paradigm.

Instead of basing his account on theory testing, Kuhn explains why scientists begin to question paradigms in the first place. Researchers carry out normal science within a paradigm, trying to solve puzzles in the usual way: "In so far as he is engaged in normal science, the research worker is a solver of puzzles, not a tester of paradigms. Though he may, during the search for a particular puzzle's solution, try out a number of alternative approaches, rejecting those that fail to yield the

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[4] Wray (2003) critically evaluates Kuhn's claim that science is a 'young man's game'.

[5] Still, Kuhn quotes Max Planck's *Scientific Autobiography* that "a new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it" (1949, 33-4).

desired result, he is not testing the *paradigm* when he does so" (SSR, 144). After all, the researcher can only approach the puzzles in question using the paradigm.

A reader may be puzzled about why Kuhn says searching for the solution to a puzzle under a paradigm does not test the paradigm. After all, scientists can test theories by trying to solve a problem using a theory, and reasoning backwards from the success or failure of that trial. But it is crucial to remember that Kuhnian paradigms are not reducible to theories, although they may incorporate theories. Paradigms are, among other things, *approaches* to the phenomena: ways of seeing the world and setting up problems. A scientist trained that electricity is exclusively a fluid will have difficulty recognizing a dry cell battery as an electricity source. In order to recognize a dry cell as a battery, that scientist will have to change a set of interlocking beliefs about how electricity works and how it can be generated. That is precisely the type of Gestalt shift in perception that heralds the development of a new paradigm. If a scientist develops a new way of looking at or conceiving of the phenomena in question while trying to address persistent failures of the current approach, that scientist is likely developing a new paradigm.

There is a significant distinction between two types of scientific crisis: the first, a theory's failing a test, and the second, a crisis within a paradigm. If a theory fails a test, it makes a straightforward prediction and that prediction is falsified: what the theory predicted is wrong. But the paradigm is not necessarily challenged when a theory is falsified.

The crisis that arises in paradigms is different from theories failing to make correct predictions. If a theory's predictions are false, Kuhn does not think that challenges the paradigm in question. In fact, that is a sign of a good paradigm, that it is a strong framework for setting up rigorous scientific puzzles.

When a paradigm is in crisis, it cannot accommodate the new observations and events. Scientists working under the paradigm don't know what to make of what they're dealing with in the first place. They are making observations of new things[6] that don't fit well into their existing categories and approaches. Researchers can't understand what those things are in the first place, much less test their theories with them. That gives rise to a crisis of normal science, not a falsification of a theory.

3. The Resolution of Revolutions

Paradigms are only 'tested' when there are two rival paradigms in the same domain, as Kuhn writes: "paradigm-testing occurs only after persistent failure to solve a noteworthy puzzle has given rise to crisis. And even then it occurs only after the sense of crisis has evoked an alternate candidate for paradigm" (SSR, 144). The rival paradigms are 'tested' against each other. But since a paradigm is not (just) a theory, the testing involved is not the familiar process of theory testing.

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[6] I say "things" rather than "phenomena" because the new observations may not even count as phenomena: they are just observations not yet connected or interpreted by any coherent conceptual framework.

Understanding why requires saying more about paradigms. A reader may wonder about the insistence that paradigms are not just theories. After all, we refer to ‘the Newtonian paradigm’ or ‘the germ paradigm’, and it certainly seems reasonable to think of the Newtonian theory or the germ theory when doing so. Paradigms can certainly be built around theories. But Kuhn relies on two additional features of paradigms in Chapter 12, at least one of which is not normally associated with theories. First, a paradigm is a general framework for puzzle-solving. Kuhn denies that a paradigm provides a step-by-step guide to solving any relevant puzzle. Scientific puzzles - even in normal science - are complex and usually not solvable a priori. There is no universal formula for solving them. But a paradigm provides a connected set of assumptions and methods that allow scientists to approach problems in a coherent and effective way. The methods may be associated with a theory, or they may not: they may consist of experimental techniques, ways of ‘seeing’ the phenomena, ways of connecting phenomena with experimental methods, and so on.

Thus, secondly, a paradigm sets norms and standards for community behavior. In the Postscript to *Structure* added in 1970, Kuhn discusses the use of ‘exemplars’, and the theoretical, metaphysical, methodological, and value-based beliefs and practices embedded in ‘disciplinary matrices’, to elaborate on these features of paradigms.[7] Kuhn notes that, had he written *Structure* slightly later, he would have elaborated more on the social features of paradigms, especially how they function as “constellations of group commitments” (the title of section 2 of the Postscript). When confronted with some object or event in her field, a scientist will respond by engaging in a highly organized set of behaviors to deal with the phenomenon: conceiving of it in a certain way, modeling it, measuring it, quantifying it, experimenting on it, drawing inferences from those experiments, and so on. These behaviors may be guided by a theory in some cases, but in many cases they are not, and they are usually not directly derived from the theory. The behaviors are guided by general norms for methods in the field, which are considered constitutive of a scientific approach to a problem or phenomenon.

The proposal of a new paradigm may challenge, not just an existing theory, but the disciplinary matrix - a set of norms for group behavior that have, up to that point, defined the scientific approach to problems. That is why the ‘resolution’ of a paradigm shift described in Chapter 12 is so complex. Replacing one paradigm with another is not simply a matter of convincing scientists that a new theory is justified by the evidence. Paradigms may be *incommensurable*, which Kuhn described at first as a global phenomenon according to which statements in one paradigm can’t be translated into the other (semantic incommensurability), or methods commonly used in one paradigm aren’t available in another (methodological incommensurability). If rival paradigms are incommensurable, it may not be possible to persuade scientists still hooked on the old paradigm

[7] “Scientists themselves would say they share a theory or set of theories, and I shall be glad if the term can ultimately be recaptured for this use. As currently used in philosophy of science, however, ‘theory’ connotes a structure far more limited in nature and scope than the one required here. Until the term can be freed from its current implications, it will avoid confusion to adopt another. For present purposes I suggest ‘disciplinary matrix’: ‘disciplinary’ because it refers to the common possession of the practitioners of a particular discipline; ‘matrix’ because it is composed of ordered elements of various sorts, each requiring further specification” (SSR, 181).

using evidence, because the evidence (and even the reasoning behind it) may not ‘travel’ across paradigms. Finally, Kuhn introduces the “the third and most fundamental aspect” of incommensurability, namely the sense in which scientists working in rival paradigms “practice their trades in different worlds” (SSR, 149). That is because “two groups of scientists see different things when they look from the same point in the same direction”. One group may see a “flat, the other… a curved matrix of space”. One group’s world “contains constrained bodies that fall slowly, the other pendulums that repeat their motions” (SSR, 149). Because scientists working in rival paradigms see things differently, they refer to different things when describing what they are working with.

That is the source of the criticisms that Kuhn’s incommensurability undermines the rationality of science. However, many critics have pointed out that there is much more continuity across paradigms than Kuhn seems to admit. While a relatively small set of methods or concepts may change, a much larger cohort of scientific practices remain the same across paradigm shifts.

And, when pressed, even Kuhn does not defend the claim that incommensurability across paradigms is global or absolute. Instead, he defends something more like a historiographical thesis, referring to what Ian Hacking (1983, 67) later calls ‘dissociation’. Dissociation “describes the experience of the historian of science as she tries to make sense of some scientific practice of the past that is significantly different from current scientific practices” (Wray 2011, 66).

Kuhn’s early argument in *Structure* and *The Essential Tension* is clearly historical. He opposes the accumulationist picture as a historical thesis. But Kuhn thinks paradigms can be rationally evaluated in terms of each other. Kuhn insists that “lack of a common measure” between paradigms “does not make comparison impossible” (RSS, 35). As James Marcum observes, in a 1982 talk responding to critics at the Philosophy of Science Association (reprinted as chap. 2 of RSS), Kuhn “admitted that his primary intention for incommensurability was more ‘modest’” (Marcum 2018, 9). Marcum continues, “Rather than radical or universal changes in terms and concepts—what is often called ‘global’ incommensurability—Kuhn claimed that only a handful of terms and concepts are incommensurable after a paradigm shift. He called this thesis ‘local’ incommensurability” (9).

Indeed, in the decades after the publication of *Structure*, Kuhn limited his definition of incommensurability significantly (Sankey 1993). Oberheim and Hoyningen-Huene (2018, §2) observe, “Kuhn initially used the term holistically to capture methodological, observational and conceptual disparities between successive scientific paradigms that he had encountered in his historical investigations into the development of the natural sciences. Later, he refined the idea arguing that incommensurability is due to differences in the taxonomic structures of successive scientific theories and neighbouring contemporaneous sub-disciplines.” Marcum has argued that this revised account of incommensurability is key to Kuhn’s evolutionary account of philosophy of science: “the ‘explanatory payoff’ for taxonomic incommensurability with respect to the revised Kuhnian evolutionary philosophy of science is that such incommensurability provides isolation for a scientific specialty and its lexicon so that it can evolve from a parental stock. For, without the conceptual isolation to develop its lexicon, a specialty cannot evolve" (Marcum 2018, 12). This is indeed a much more 'modest' role for incommensurability. Instead of sweeping scientific revolutions installing a new regime in which scientists work 'in a new world', the later Kuhn describes a process of specialization in which scientists develop distinctive terminology, defining a lexicon for practical reasons.

Still, in Chapter 12 Kuhn insists that the move to a new paradigm will always involve some degree of persuasion. Whether we think of paradigms as heralding increasing specialization or as causing wholesale theory change, paradigms involve changes in the kinds of scientific problems that are considered to be salient. Kuhn writes,

> If there were but one set of scientific problems, one world within which to work on them, and one set of standards for their solution, paradigm competition might be settled more or less routinely by some process like counting the number of problems solved by each. But, in fact, these conditions are never met completely. The proponents of competing paradigms are always at least slightly at cross-purposes. (SSR, 147)

Kuhn's talk of different 'worlds' in which scientists work has been taken as a strong ontological claim, amounting to an ontological constructivist position. Massimi (2015) argues that Kuhn is describing a kind of semantic mind-dependence of scientific theories and approaches, not an ontological mind-dependence (p. 83). In keeping with that reading, Kuhn continues just after the passage cited above to say that "Neither side will grant all the non-empirical assumptions that the other needs in order to make its case. Like Proust and Berthollet arguing about the composition of chemical compounds, they are bound partly to talk through each other" (SSR, 147).

Revolutions in science need to be 'resolved' because those committed to competing (or successive) paradigms see things differently and thus approach the phenomena in different ways. Kuhn goes on to say that this is effectively a matter of how they *understood* the phenomena and concepts in question.

> Within the new paradigm, old terms, concepts, and experiments fall into new relationships one with the other. The inevitable result is what we must call, though the term is not quite right, a misunderstanding between the two competing schools. The laymen who scoffed at Einstein's general theory of relativity because space could not be "curved" - it was not that sort of thing - were not simply wrong or mistaken. […] What had previously been meant by space was necessarily flat, homogeneous, isotropic, and unaffected by the presence of matter. If it had not been, Newtonian physics would not have worked. (SSR, 148)

The motivation for the general theory of relativity - or so Kuhn urges - was not the falsification of a hypothesis generated within the Newtonian paradigm. It was Einstein's increasing recognition that the way the fundamental concepts and phenomena of the paradigm were related to each other could be reorganized in a way that would explain new puzzles and generate even more interesting puzzles. Just as the reason for its development was not that the old paradigm was falsified, the reason to accept the new relativistic paradigm is not that the new paradigm is better confirmed. Relativity is not 'better confirmed' in the sense that it accounts for *more* facts - taken collectively - than the Newtonian paradigm. Instead, GR accounts for different facts differently.

Trying to tell Kuhn's skeptical laymen directly that space is curved would be a fool's errand. But one could possibly approach them as follows. What if we stepped back from what we mean by 'space' in the case of the physical universe? And what if we considered the possibility that matter affects the curvature of 'space'? What if thinking of things this way allows us to conceive of a much simpler approach to gravity, and to solve problems that Newton's theory doesn't even have a good way to tackle?

Kuhn's point in Chapter 12 is that no amount of evidence will persuade a skeptic to adopt a new paradigm, since paradigms aren't the sort of thing that are confirmed or falsified by evidence alone. Paradigms are approaches to the phenomena. And so persuading someone to adopt a new paradigm - even as a hypothesis - requires persuading them to think of things differently, and even to see them differently.

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