MUST KUHN ALLOW CROSS-PARADIGM EVIDENCE?

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Abstract. Does Kuhn's thesis that successive paradigms are incommensurable necessarily entail denying that the same evidence can be employed under successive paradigms? In this paper, I will argue *no*. In supporting that conclusion, I will argue for an even stronger point: Kuhn must be committed to the claim that the same evidence can be employed across paradigms, or his account of anomalies makes no sense.

Keywords: incommensurability; paradigm; anomaly; evidence; scientific revolution.

1. THE PROBLEM: INCOMMENSURABILITY AND IRRATIONALITY

Kuhn's scientific revolutions break the continuity of scientific progress and bring disillusion to a cherished image of science: as an unbroken thread of discovery that only accumulates results over time, and which is comprehensible as a coherent whole of rational inquiry. Kuhn's argument in *The Structure of Scientific Revolutions* that successive paradigms are incommensurable is one of the keystones of his challenge to the cumulative image of science[1]. In response to anomalies building up in an existing paradigm, Kuhn argues, scientists may achieve a new paradigm that radically changes the way the terms of the theory hook up to the phenomena, as well as the way scientists approach the phenomena in constructing experiments and performing measurements. The new paradigm does not just make claims that are in conflict with the old paradigm. The claims of the new paradigm are not recoverable within the old one, and vice versa, because

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[1] This is not intended to be a purely historical paper about what Kuhn himself thought. I do intend to be accurate in representing Kuhn's statements in *Structure*, "Function", and other texts. But there are arguments in the paper that involve the following reasoning: I think Kuhn *must be committed to* claim X, *even if he said otherwise*.

the paradigms approach and conceive of the phenomena differently, and take divergent paths from there[2].

Scientific revolutions are in conflict with our cherished image of science in two ways. First, when moving to a new paradigm, scientists may lose access to results achievable in the existing one ("Kuhn loss"). This is true even though scientists achieve new results under the novel paradigm. Second, after a revolution scientists can't combine their results under a single heading, since there is no coherent conceptual framework that unifies the old and new paradigms. That is another way of stating Kuhn's controversial thesis of the "incommensurability" of successive paradigms.

On Kuhn's account, when anomalies build up under a paradigm, that paradigm enters a state of "crisis science". In a crisis, scientists begin to consider options that would have seemed outrageous when the paradigm was working well. They consider radical changes to the fundamental approach the paradigm takes to the phenomena under investigation. The paradigm no longer works as well in practice as it did. Thus, the decision to try another approach – to adopt a novel paradigm – may not be directly determined by the evidence. Instead, it is a choice responding to deeply practical problems, motivated by the values and goals of the scientist and of the research community in which the scientist works.

Kuhn's view that scientists' choice to change a paradigm may be motivated by values and goals (that is, by pragmatic considerations in the philosophical sense) and not determined entirely by the evidence has been deeply unpopular in some quarters. The objection turns on the weight society places on science as a "veritistic" enterprise that seeks the truth above all[3]. Alvin Goldman argues that society invests so many resources in science because science is purely veritistic: scientists pursue epistemic enlightenment exclusively and eschew social, political, and personal motivations. Kuhn's account challenges the exalted social value the veritistic account accords to science, by arguing that scientists allow values to influence their choices in at least some cases[4]. These objections dovetail with a related objection to Kuhn's account of values, that it lets social and political

[2] For discussions of the concept and development of Kuhnian incommensurability, see James A. Marcum, "The Evolving Notion and Role of Kuhn's Incommensurability Thesis", ch. 9 of W. Devlin, A. Bokulich (eds.), *Kuhn's Structure of Scientific Revolutions—50 Years On*, Dordrecht, Springer, 2015; Xiang Chen, "Thomas Kuhn's Latest Notion of Incommensurability", in *Journal for General Philosophy of Science/Zeitschrift für allgemeine Wissenschaftstheorie* 28 (2), 1997, pp. 257–273; and Eric Oberheim, Hanne Andersen, Paul Hoyningen-Huene, "On Incommensurability", in *Studies in History and Philosophy of Science* 27 (1), 1996, pp. 131–141.

[3] Alvin Goldman, *Knowledge in a Social World*, Oxford, Clarendon Press, 1999; and Philip Kitcher, "Veritistic Value and the Project of Social Epistemology", in "Review of *Knowledge in a Social World* by Alvin Goldman", in *Philosophy and Phenomenological Research* 64 (1), 2002, pp. 191–198.

[4] Kuhn argued that epistemic values will usually be predominant (see Alexander Bird, "Thomas Kuhn", in E. Zalta (ed.), *The Stanford Encyclopedia of Philosophy*, 2022, §6.3, URL = <https://plato.stanford.edu/archives/spr2022/entries/thomas-kuhn/>), but he did allow for other values to influence scientists' choices.

agendas in by the back door[5]. And they are linked with a familiar and more general objection to Kuhn: that he is a relativist about truth because he allows for incommensurable paradigms.

Overall, Kuhn's account of paradigm shifts has been said to undermine the "rational reconstruction" of scientific theory change, and to explain scientific revolutions in terms of extra-rational choices on the part of individual scientists. This charge has come from both philosophers and historians. Imre Lakatos[6] argues that what Kuhn explains in terms of non-epistemic values can be accounted for, instead, in terms of whether a new theory uncovers more novel facts than the old one. Stephen Toulmin writes: "with experience, it has become clear to political historians that nothing is achieved by saying 'and then there was a revolution', as though that exempted one from the need to give any historical analysis of a more explicit kind. To do only that is not to perform the historian's proper intellectual task, but to shirk it."[7]

Lakatos and Toulmin are strange bedfellows, but they share an objection. The criticism is that Kuhn does not truly explain scientific revolutions, but rather uses speculation about scientific revolutions to explain historical phenomena like Kuhn loss and incommensurability. Lakatos wants Kuhn to explain the epistemic reasons behind the decision to set aside an old theory and adopt a new one[8]. Lakatos agrees that these choices may rest on convention to some extent, but he argues that ultimately the choice to adopt a new theory is based on a sophisticated version of falsificationism: an old theory is falsified if it ceases to solve new problems, and that gives scientists an epistemic reason to adopt a new theory that is undergoing a "progressive problemshift" – that is, can account for more novel facts[9].

[5] Alexander Bird remarks: "Feminists and social theorists… have argued that the fact that the evidence, or, in Kuhn's case, the shared values of science, do not fix a single choice of theory, allows external factors to determine the final outcome... Furthermore, the fact that Kuhn identified *values* as what guide judgment opens up the possibility that scientists ought to employ different values, as has been argued by feminist and post-colonial writers." (§6.3).

[6] Imre Lakatos, "The Methodology of Scientific Research Programmes", in I. Lakatos, A. Musgrave (eds.), *Criticism and the Growth of Knowledge*, Cambridge, Cambridge University Press, 1970, p. 116.

[7] Stephen Toulmin, "Conceptual Revolutions in Science", in *Synthese* 17 (1), 1967, pp. 75–91, p. 84.

[8] Lakatos is the source of a frequent misreading of Kuhn, that paradigm shifts are identical with theory change. A paradigm is not (just) a theory (Lydia Patton, "Kuhn, Pedagogy, and Practice: A Local Reading of Structure", in Moti Mizrahi (ed.), *The Kuhnian Image of Science: Time for a Decisive Transformation?*, Lanham, Rowman and Littlefield, 2018, 114ff.).

[9] Lakatos approved of Kuhn's rejection of "naive methodological falsificationism", which would base paradigm shifts on simple experimental tests of hypotheses. "According to the logic of dogmatic falsificationism", Lakatos observes, "science grows by repeated overthrow of theories with the help of hard facts" (*op. cit*., p. 97). Lakatos admits that dogmatic falsificationism is "untenable". But Lakatos responds that Kuhn unfairly neglected a more sophisticated version of Popperian falsificationism: "For the sophisticated falsificationist a theory is 'acceptable' or 'scientific' only if it has corroborated excess empirical content over its predecessor (or rival), that is, only if it leads to the discovery of novel facts" (p. 116). Lakatos divides theories into those with a "progressive" or "degenerating" problemshift, which corresponds to whether the theory supports novel results or not.

If a scientific revolution is to be connected with the events before and after it, and to fit into the nexus of historical explanation, Toulmin argues that we must give a continuous historical reconstruction of the change[10]. Kuhn's description of the adoption of a novel paradigm is as a "conversion" in which, as Toulmin puts it, "Newthink swept aside Oldthink completely"[11]. Toulmin argues instead that there is significant continuity between the new and the old. In particular, experimental and inferential methods are retained, and scientists who have worked in both paradigms "could afterwards explain quite articulately the considerations which moved them to change"[12].

Toulmin's and Lakatos' objections come under the heading of objections to the 'irrationality' of scientific revolutions in *Structure*.

Lakatos: A sophisticated falsificationist methodology demonstrates why it is rational for scientists to adopt a new theory, even if elements of that new theory are chosen by convention.

Toulmin: We can construct a coherent historical narrative showing that scientific methods and reasoning persist through paradigm shifts. Those methods and reasoning can be put to use to explain why scientists chose a new paradigm, even if the choice was underdetermined by the evidence.

There are two things to note about Lakatos's and Toulmin's objections:

1. Both Lakatos and Toulmin accept that the choice of a new paradigm is not uniquely determined by the evidence and that there may be a degree of convention to scientists' choices.

2. Both Lakatos and Toulmin argue that a continuous rational explanation of scientific change depends on appeal to scientific **methods**.

Point (2) will become important below. While anomalies may not be semantically commensurable across paradigms, there must be at least a local method to compare the existing and novel paradigms with respect to the anomalous results.

2. CAN KUHN ALLOW CROSS-PARADIGM EVIDENCE?

The problem that motivates this paper is found in *Structure*'s account of anomalies in science. Anomalies must be measured quantitatively under an existing paradigm. If anomalies are measured successfully using a measurement process from an existing paradigm, they must be commensurate with the methods of the *old* paradigm. But aren't anomalies supposed to be better explained by the new

[10] St. Toulmin, *op. cit.*, p. 84: "Even in political revolutions, the break is never absolute. Continuities of law, and custom and administrative procedure always survive… So, nowadays, statements about the occurrence of political revolutions are accepted only as posing deeper questions about 'the occasions on which and the processes by which' supreme authority changes hands in a 'revolutionary' way."

[11] *Ibidem*, p. 85.

[12] *Ibidem*.

paradigm – and in that case, shouldn't they be commensurate with the *new* paradigm? Finally, then, anomalies should be commensurate with both paradigms.

Thus, it appears that anomalies are a counterexample to Kuhnian incommensurability. Of course, Kuhn does not argue that everything is incommensurable between successive paradigms. But surely if anything should be incommensurable, anomalies should. After all, anomalies are among the main reasons for changing paradigms. And there are textual reasons for concluding that anomalies are incommensurable across paradigms. For instance, one might conclude from Kuhn's position on the "holistic" nature of revolutionary change[13] that evidence, on Kuhn's account, cannot be assessed across paradigms.

The main argument of this paper is that Kuhn must allow for evidence to be assessed across paradigms in some sense. There is a fairly simple prima facie argument for this claim. For Kuhn's reasoning about revolutions in *Structure* to be valid, evidence against the existing paradigm has to come to be regarded as evidence for the new paradigm. Otherwise, his account of the behavior of scientists makes no sense. But if this is true, then the anomaly must bear negatively on the old paradigm and positively on the new one[14].

There are several ways to respond to this point right at the outset. One would be to say that, whatever Kuhn intended, he was wrong about the implications of his theory: his account cannot allow for the same evidence to bear on distinct paradigms.

Another response would be to argue that, in fact, Kuhn is consistent, because he does not talk about "evidence against" the old paradigm, he talks about *anomalies*. Anomalies are precisely facts or observations that the old paradigm cannot accommodate. A new paradigm is formulated in order to make sense of anomalies, which are then evidence for the new paradigm, but not evidence against the old one, or so we might argue. On this reading, anomalies are not evidence of any kind according to the old paradigm, because they don't fit into it at all.

The question turns on what Kuhn means by "evidence". Do anomalies count as evidence against the old paradigm? Kuhn gives an account in his paper "The Function of Measurement in Modern Physical Science", which came out one year before *Structure*. There, Kuhn analyzes how the background assumptions of physical theories ground measurement in normal and extraordinary science, and he distinguishes quantitative from qualitative measurement.

We might argue that this motivates something like the usual reading of Kuhn: A Kuhnian anomaly is something that cannot be measured in the old paradigm, or

[13] Thomas S. Kuhn, *The Road Since Structure*, Chicago, University of Chicago Press, 2000, pp. 28–29.

[14] An observation that has been made before, I believe. Now, of course, someone could object that an anomaly can't really be "the same evidence" if it's formulated differently in the existing and novel paradigms. But I am not defending the claim that equivalent *statements* about the evidence can be made in both paradigms. Instead, I am arguing for just what I said: that an anomaly can be evidence against an existing paradigm and for a novel one.

at least, not measured quantitatively. But "The Function of Measurement" does not make that point, and it actually makes the opposite point: An anomaly is a measurement in the *existing* paradigm.

In "The Function of Measurement in Modern Physical Science" Kuhn notes that identifying something as an anomaly can take quite a bit of work. It often means a shift from scientists' usual practice of trying to find what Kuhn calls "reasonable agreement" between a theory and predicted measurements. The shift involves recognizing that there is disagreement between theory and phenomena that can be measured quantitatively. In the first instance, scientists must investigate over time how to elaborate and articulate the practical consequences ('testable predictions') of a novel approach to the phenomena.

> The new order provided by a revolutionary new theory in the natural sciences is always overwhelmingly a potential order. Much work and skill, together with occasional genius, are required to make it actual. And actual it must be made, for only through the process of actualization can occasions for new theoretical reformulations be discovered.[15]

In particular, the consolidation of a novel approach involves investigating how to generate sophisticated measurement processes that generate specific outcomes. These measurement processes are not exclusively experimental: they may involve a great deal of theoretical and mathematical reasoning. In the case of the Newtonian theory of planetary motion, it took the combined efforts of Euler, Lagrange, Laplace, Gauss, and others to formulate the anomaly in the motion of Mercury.

> To discover how to treat these deviations by Newtonian theory, it was necessary to devise mathematical estimates of the "perturbations" produced in a basically Keplerian orbit by the interplanetary forces neglected in the initial derivation of Kepler's laws. Newton's mathematical genius was displayed at its best when he produced a first crude estimate for the perturbation of the moon's motion caused by the sun. Improving his answer and developing similar approximate answers for the planets exercised the greatest mathematical minds of the eighteenth and early nineteenth centuries, including those of Euler, Lagrange, Laplace, and Gauss. Only as a result of their work was it possible to recognize the anomaly in Mercury's motion that was ultimately to be explained by Einstein's general theory. That anomaly had previously been hidden within the limits of "reasonable agreement".[16]

In this passage, Kuhn states explicitly that the quantitative anomaly was identified *prior to* Einstein's formulation of a new theory. The quantitative treatment of an anomaly, or the construction of a process to measure that anomaly,

[15] Th. S. Kuhn, "The Function of Measurement in Modern Physical Science", in *Isis* 52 (2), 1961, pp. 161–193, p. 168.

[16] *Ibidem*, p. 170.

does not come only with the formulation of a new paradigm. In fact, it often comes in the process of articulating and extending an existing approach.

> The bulk of scientific practice is thus a complex and consuming mopping-up operation that consolidates the ground made available by the most recent theoretical breakthrough and that provides essential preparation for the breakthrough to follow. In such mopping-up operations, measurement has its overwhelmingly most common scientific function.[17]

Kuhn is clear, here and elsewhere, that anomalies in physics have to be identified via hard work that includes mathematical and experimental reasoning resulting in quantitative measurement. And much of that work happens under the existing paradigm.

In "The Function of Measurement" Kuhn asserts that increasingly precise measurements can identify anomalies that had been concealed as "measurement error" or "reasonable agreement". Refinement to measurement techniques and instrumentation may result in increasing distance between the paradigm and measurement outcomes, which can become (the basis of) anomalies. To identify an anomaly requires measuring a distance between the predictions of the existing paradigm and the actual quantity measured - under the *existing* paradigm. That is consistent with the account in *Structure* according to which anomalies "build up" that the existing paradigm can't explain. As can be seen in the example of the perihelion of Mercury, anomalies can hang around for quite a while before a new paradigm explains them.

Kuhn argues for a somewhat surprising claim in "Function": *the fact that a measurement is an anomaly for a paradigm is usually proven within that paradigm.* There is a broader argument hidden within Kuhn's reasoning here, which I will sketch as follows:

THE MEASUREMENT ANOMALY ARGUMENT

1. Most physical measurement is made in the context of a paradigm. An existing paradigm may make a given measurement process possible and enable the convergence of that process on a specific measurement outcome[18].

2. Take a measurement process MP that converges on a measurement outcome M in the context of a given paradigm P. In some cases, M will be an *anomaly* for P.

[17] *Ibidem*, p. 168.

[18] I am using Eran Tal's language (Eran Tal, "Calibration: Modelling the measurement process", in *Studies in History and Philosophy of Science* 65–66, 2017, pp. 33–45), but Kuhn does say that textbooks are written sometime after the "discoveries and confirmation procedures whose *outcomes* they record" (*op. cit.*, p. 167), which is close.

3. The *more accurately* the measurement process MP converges on M, the *more evident* it becomes that the measurement M is inconsistent with the paradigm P. But the accuracy of M may even depend on P in some cases (premise 1).

4. The fact that M is inconsistent with a paradigm P becomes evidence against P.

Conclusion: The proof that a measurement is evidence against a paradigm is usually carried out under the existing paradigm and may even depend on it.

Premise 3 is quite surprising and even paradoxical, and it gets to the heart of what Kuhn means by "anomaly". An anomaly is usually *not* a measurement made by a rogue researcher doing experiments under a new paradigm in her basement. Nor is it a measurement made using new instruments or concepts invented for a new paradigm. It is an anomalous result of measurements carried out under and using the existing paradigm.

An anomaly for Kuhn is not a result achieved under a new approach that confirms that approach. It is a usually deep and precise result achieved under the existing paradigm which turns out to challenge that paradigm's conceptual and practical framework. An anomaly is often the result of honest and rigorous inquiry carried out under the existing paradigm. That is precisely what makes it such a problem for that paradigm.

Scientists should be able to show that the same evidence bears on the existing paradigm negatively and the novel paradigm positively, or it would be very puzzling why they would adopt the new one. Ultimately, Kuhn argues that the formulation of anomalies reveals the limits of the old theory, and provides us with information about how a new theory should be formulated. Evidence and measurement must cross paradigms.

3. ARE ANOMALIES INCOMMENSURABLE?

But if evidence about anomalies must cross paradigms, it seems that this is inconsistent with Kuhn's account of incommensurability. Paul Hoyningen-Huene and Howard Sankey note that we can distinguish two versions of incommensurability: semantic and methodological. First, "semantic incommensurability" "derives from the claim of Kuhn and Feyerabend that the meaning of the terms employed by theories varies with theoretical context"[19].

Semantic (or taxonomic) incommensurability is potentially a problem for my argument, so we will investigate it here. Semantic incommensurability is motivated by Kuhn's account of paradigms as integrated pictures of the phenomena, which can be understood only as a coherent whole. For instance, Aristotle's mechanics

[19] Paul Hoyningen-Huene, Howard Sankey, "Introduction" to *Incommensurability and Related Matters*, Dordrecht, Kluwer, 2001, p. xv.

can be understood only given his definition of motion, and Planck's choice of 'oscillator' rather than 'resonator' for energy sources can be understood only in the context of his switch to regarding energy as a quantum[20].

In "What Are Scientific Revolutions?" Kuhn describes Planck's change from referring to energy "elements" to energy "quanta"[21]. Describing energy as an "element" implies that energy exists as a part of something else. The word "quantum" implies, in contrast, that energy is "an atomlike entity that could exist by itself"[22]. The change from "element" to "quantum" thus necessitated another terminological change. At the same time, Planck shifted from calling energy elements "resonators" to calling energy quanta (like light quanta) "oscillators"[23].

Planck's theory of energy "quanta" had the consequence, according to Kuhn, that "'resonator' was not an appropriate term" any longer (p. 28). The term "resonator" implied continuous vibration on analogy with acoustic vibration, and Planck's theory now involved energy quanta that exhibited discrete vibration. A classical quantum theory could attempt to preserve the picture according to which energy exhibits continuous vibration or 'resonance'. But Planck's intention was to construct a novel account that captured a set of interconnected changes to our understanding of 'energy': that it is a discrete entity, and that it exhibits discrete vibration.

In both cases, the novel understanding of 'energy' involved changing, not just a descriptive picture, but the type of theoretical explanations that were possible using the concept. Kuhn emphasizes that these changes are interdependent: "Revolutionary changes are somehow holistic. They cannot, that is, be made piecemeal, one step at a time, and they thus contrast with normal or cumulative changes like, for example, the discovery of Boyle's law. […] An integrated picture of several aspects of nature has to be changed at the same time."[24] When that picture changes, there is a Gestalt shift in how the paradigm approaches the phenomena.

Kuhn's view that revolutionary changes are "holistic" and represent "Gestalt shifts" or "conversions" has also been taken to mean that none of the results of an old paradigm can be assessed by the standards of the new one. If the basic methods and approach of the theory have changed, then there is no common method of appraisal – not even a diverse set of methods that can cross paradigms to allow for

[20] That is not to say that one was necessitated by the other.

[21] For more recent assessments of Kuhn's account of the quantum theory, see Jan Potters, "Conceptualizing Paradigms: On reading Kuhn's history of the quantum", in *Annals of Science* 79 (3), 2022, pp. 386–405; and Adam Timmins, "Between History and Philosophy of Science: The Relationship between Kuhn's Black-Body Theory and Structure", in *HOPOS* 9 (2), 2019, pp. 371–387.

[22] Th. S. Kuhn, *The Road Since Structure*, p. 28.

[23] The term "resonance" was used in 19th century science, especially by Hermann von Helmholtz, to describe continuous acoustic vibrations, and a "resonator" is something (for instance, a column of air, or a material) that exhibits continuous vibration.

[24] Th. S. Kuhn, *The Road Since Structure*, pp. 28–29.

comparison. That is the thesis of *methodological incommensurability*. Methodological incommensurability is quite distinct from the semantic version, as Sankey and Hoyningen-Huene argue:

> According to the thesis of methodological incommensurability, there are no shared, objective methodological standards of scientific theory appraisal. Standards of theory appraisal vary from one paradigm to another. There are no external or neutral standards which may be employed in the comparative evaluation of competing theories. As a result, alternative scientific theories may be incommensurable due to absence of common methodological standards capable of adjudicating the choice between them.[25]

There is an important distinction to be made regarding methodological incommensurability. First, there is the **global** version of the thesis presented above: whether there is a set of "common methodological standards capable of adjudicating the choice between [paradigms]". Global methodological incommensurability is cited to explain why Kuhn can't rationally reconstruct paradigm shifts.

But there is another aspect of (in)commensurability: the **local** version, which Kuhn developed in the 1980s[26]. Kuhn first articulated this as local taxonomic incommensurability. After a paradigm shift, Kuhn said, "most of the terms common to the two theories function the same way in both; their translation is simply homophonic. Only for a small subgroup of (usually interdefined) terms and for sentences containing them do problems of translatability arise"[27]. Kuhn used this claim to argue "that only a section or subset of the new paradigm is incommensurable with the old one", with the aim of differentiating local incommensurability from the radical claim that "global or extreme incommensurability exists between two competing or successive theories or paradigms"[28].

Despite Kuhn's efforts, the global, holistic version of incommensurability has persisted. One reason for the persistence is that global incommensurability fits in nicely with various versions of theoretical holism. Michael Friedman[29] notes that

[25] P. Hoyningen-Huene, H. Sankey, "Introduction" to *Incommensurability and Related Matters*, p. xv.

[26] Th. S. Kuhn, "Commensurability, Comparability, Communicability", in *Philosophy of Science Association Proceedings* 1982 (2), 1983, pp. 669–688.

[27] *Ibidem*, pp. 670–671. See Xiang Chen, "Thomas Kuhn's Latest Notion of Incommensurability", p. 258.

[28] Kuhn developed changes to his account of incommensurability in the 1980s and 1990s in response to criticism, including "a linguistic theory of scientific revolutions (the theory of kinds), a cognitive exploration of the language learning process (the analogy of bilingualism), and an epistemological discussion on the rationality of scientific development (the evolutionary epistemology)" (X. Chen, "Thomas Kuhn's Latest Notion of Incommensurability", p. 257). Cf. James A. Marcum, "The Evolving Notion and Role of Kuhn's Incommensurability Thesis", p. 115.

[29] Michael Friedman, "Kant, Kuhn, and the Rationality of Science", in M. Heidelberger, F. Stadler (eds.), *History of Philosophy of Science*, Dordrecht, Kluwer, 2002.

Kuhn is often read through the lens of Quinean holism, according to which no element of a scientific approach, including logical and mathematical axioms, is immune to revision when confronted with recalcitrant evidence – that is, anomalies. A further Quinean (and Carnapian!)[30] argument is often brought to bear: that there are no hard and fast rules governing the translation of a proposition in one conceptual framework into an equivalent proposition of another.

These two points have been combined to argue that Kuhn's holistic paradigm shifts result in global untranslatability. Global untranslatability is cited as a reason why Kuhn can't rationally reconstruct paradigm shifts, which is then taken to show that local commensurability is impossible (despite Kuhn's attempts to defend it). For instance: If scientists change standards of measurement, then a measured anomaly in one paradigm might become entirely consistent with and even a confirmation of a different paradigm. And there is no general way to translate propositions about the measurement in the first paradigm, where it is treated as an anomaly, into exactly equivalent propositions about the measurement in the second paradigm, where it is treated as a confirmation of the theory (or approach) in question.

If the only way to establish that an anomaly is cross-paradigm evidence is linguistic translation, that will usually be true. But there is a difference between the following two claims:

Semantic Claim. Statements about an anomaly in one paradigm can be translated into exactly equivalent statements about that same result in a new paradigm, now treated as confirming evidence.

Methodological Claim. The same measurement results can be used as evidence against one paradigm and for another, precisely because scientists can come to understand, by reflection on methods and results, that the approach to the measurement in the new paradigm is superior.

The basic argument of this paper is that the semantic claim about anomalies is false and the methodological claim is true.

It is a shame that Kuhn's work in *Structure* has become so entangled with the quite different debates about translation that Quine, Davidson, and others were engaged in in the 1970s[31]. Kuhn's incommensurability in *Structure* is not Quinean indeterminacy of translation. There are many reasons for this, but I will give one simple reason. Translation takes place between languages. Paradigms are not

[30] Something like this point is made in "Empiricism, Semantics, and Ontology", as well as in the classic Quinean texts on indeterminacy of translation more generally. Cf. W.V. Quine, "On the Reasons for Indeterminacy of Translation", in *The Journal of Philosophy* 67 (6), 1970, pp. 178–183.

[31] Kuhn does say that "The claim that two theories are incommensurable is then the claim that there is no language… into which both theories… can be translated without residue or loss." (*The Road Since Structure*, p. 36). First, he says "theories" here and not paradigms, which I believe is quite consequential. Second, this is years after *Structure*, when Kuhn himself had been persuaded that his own claims were linguistic, something I think was a mistake.

reducible to languages, or even to theories. They are approaches to the phenomena that are appealed to even when theories or languages fail[32].

Thus, one solution to the problem of anomalies for Kuhn would appeal to the distinction between semantic/taxonomic and methodological incommensurability. Anomalies have a double face. They must be evidence against the existing paradigm and evidence for the next one. Anomalies usually won't be given the same semantic account in the new paradigm – that is, their description usually won't latch on to the phenomena in the same way. But anomalies are usually methodologically commensurate with both paradigms in some sense[33]. Thus, while anomalies may become semantically incommensurable as paradigms change, it would have to be very rare for an anomaly to become entirely methodologically incommensurable if Kuhn's account of the history of science is correct.

For Kuhn's account of the history of science to make any sense, anomalies have to be seen as a reason to give up the existing paradigm and a reason to adopt the new one. Scientists working in the same field, focusing on the same targets, have to see the buildup of anomalies as a reason to abandon an existing paradigm and to take up a new paradigm. That is not to say that propositions about the anomalous results in the existing paradigm are smoothly translatable into propositions about the results that now confirm the novel paradigm. As Kuhn puts it,

> The hypotenuse of an isosceles right triangle is incommensurable with its side or the circumference of a circle with its radius in the sense that there is no unit of length contained without residue an integral number of times in each member of the pair. There is thus no common measure. But lack of a common measure does not make comparison impossible.[34]

While it may be true that scientists can't smoothly translate an anomaly in one paradigm into a confirming result of the next paradigm, they can certainly compare the two paradigms with respect to the experiment or result in question. That comparison must rest on some kind of local methodological commensurability (a point Kuhn was strangely reluctant to make). Local methodological commensurability doesn't have to mean that the same methods can show that an anomaly is evidence for a new paradigm. It does mean that scientists can give a coherent rational explanation for why a given measurement is an anomaly under the old paradigm, and why it is simultaneously a reason to adopt the novel paradigm.

[32] Reporting later on a conversation with Margaret Masterman, Kuhn says, "I can't make [what she said] work quite but it's very deeply to the point: a paradigm is what you use when the theory isn't there" (*The Road Since Structure*, p. 300). See Vasso Kindi, "Kuhn's Paradigms", in V. Kindi, T. Arabatzis (eds.), *Kuhn's* The Structure of Scientific Revolutions *Revisited*, London, Routledge, 2012, for a more comprehensive discussion of Kuhn's paradigms.

[33] As Lakatos and Toulmin would agree (§1 above).

[34] Th. S. Kuhn, *The Road Since Structure*, p. 36.

Kuhn must allow for evidence about anomalies to cross paradigms, in the narrow sense that an anomaly must be conceived of as evidence against the existing paradigm and for the novel one. There must be some set of local methods that can show that the same result bears witness against one paradigm and for the other. In "The Function of Measurement in Modern Physical Science", Kuhn demonstrates that, paradoxically, the more sophisticated a measurement process becomes in the existing paradigm, the more likely it is to uncover anomalies that require a new paradigm. But that very sophistication gives us indications of reasons to abandon the existing paradigm, which cross over smoothly into reasons to construct and adopt the new paradigm.

Statements about anomalies in the existing paradigm may not be translatable into exactly equivalent statements about confirming results in the new paradigm. But statements about the anomalies as reasons to give up the existing paradigm can certainly be understood simultaneously as reasons to adopt the new paradigm. This may or may not even require translating the reasons into new terminology (appealing to different taxonomic kinds, as Kuhn might put it).

In "The Function of Measurement in Modern Physical Science", Kuhn explains the Janus face of anomaly particularly well. First, measurements intended to articulate, extend, and confirm an existing paradigm are exceedingly difficult to make[35]. In the case of most physical theories "with quantitative implications" it was immensely difficult to find many problems that [permit] quantitative comparison of theory and observation. Even when such problems were found, the highest scientific talents were often required to invent apparatus, reduce perturbing effects, and estimate the allowance to be made for those that remained. This is the sort of work that most physical scientists do most of the time insofar as their work is quantitative. Its objective is, on the one hand, to improve the measure of "reasonable agreement" characteristic of the theory in a given application and, on the other, to open up new areas of application and establish new measures of "reasonable agreement" applicable to them[36].

Only by establishing specific measures of "reasonable quantitative agreement" between theory and observation can scientists extend the application and confirmation of an existing approach to the phenomena. Finding the quantitative consequences of an approach (paradigm) is not simply a matter of applying a rule: it is an extremely complex puzzle.

> For anyone who finds mathematical or manipulative puzzles challenging, this can be fascinating and intensely rewarding work. And there is always the remote possibility that it will pay an additional dividend: something may go wrong.[37]

[35] Th. S. Kuhn, "The Function of Measurement in Modern Physical Science", pp. 169–170.
[36] *Ibidem*, pp. 170–171.
[37] *Ibidem*.

In the midst of the muddy and complicated work of finding whether observations are in reasonable agreement with theory, scientists may discover that no standard of reasonable agreement will account for one or more of the observed phenomena. This is rare, according to Kuhn, but it is a landmark event.

The measurement of Mercury's perihelion is a perfect example of a Kuhnian anomaly, because it is simultaneously a great achievement of the Newtonian tradition of measurement and a reason to look forward to the Einsteinian paradigm of general relativity[38]. Kuhn emphasizes both aspects of this result in "The Function of Measurement". Newton, Euler, Lagrange, Laplace, and Gauss devoted significant energy to the problem of finding precise measurements of Mercury's orbit. Every step they took toward making that measurement more precise was a step toward proving an anomaly for the Newtonian paradigm. It was *the Newtonians*, not Einstein, who first established the anomaly. It would be absurd to say that Laplace or Gauss could not understand why that anomaly was a reason to challenge Newton's approach - they understood better than anyone. Similarly, if presented with the theory of general relativity, they could surely understand why that same measurement might be evidence in favor of Einstein's rival approach.

Now, does that mean that if we were to revive Laplace or Gauss, they could immediately begin work as relativistic physicists? Even geniuses require training. Just because they could immediately understand why the result they proved is evidence that a new paradigm is needed, and even why it's evidence for general relativity, does not mean that they would instantly absorb general relativity's broader approach to physical problems.

That was much of Kuhn's original point about incommensurability in *Structure*. The context of pedagogy is often missed. Scientists must be *trained*[39]. Hard work is required to make the transition from working in one paradigm to working in another. It's not just a matter of translating the claims of one theory into the claims of another. If a fundamental change of approach has been made, then figuring out how to turn theoretical claims into observable quantitative consequences requires new practical methods, as Kuhn emphasizes in "The Function of Measurement". New practical methods in a novel physical paradigm often require significant training. Moving from classical to relativistic physics as a working physicist is not just a matter of saying, "Aha - wherever Newton said 'space and time' Einstein says 'spacetime'. I'll just remember to translate that in my head and

[38] To a historian looking at these events in retrospect, of course, not for Newton who could have had no inkling of this.

[39] As Kuhn says repeatedly, including in the opening section of "The Function of Measurement". See L. Patton, "Kuhn, Pedagogy, and Practice: A Local Reading of Structure", especially the references to the work of others including Rouse; Richardson ("The Structure of Philosophical History: Thoughts after Kuhn", ch. 11 of V. Kindi and T. Arabatzis (ed.), *Kuhn's The Structure of Scientific Revolutions Revisited*, New York, Routledge, 2012); Kaiser; and Andersen (E. Oberheim, H. Andersen, P. Hoyningen-Huene, "On Incommensurability", in *Studies in History and Philosophy of Science* 27 (1), 1996, pp. 131–141).

therefore will become a relativist." Someone who does this will entirely miss the point, and will not be able to achieve as much in practice as someone who actually adopts the substance of the relativistic approach.

4. CONCLUSION

Anomalies are in some sense commensurate with the existing paradigm. After all, the existing paradigm supported a measurement process that eventuated in a precise quantitative measurement of the anomaly. Paradoxically, the more precisely the existing paradigm allows for the measurement outcome to be determined, the clearer it is that the result is an anomaly for the paradigm. A new paradigm must be constructed in order to account for this perplexing state of affairs. But the new paradigm isn't constructed merely to allow for the quantitative measurement of the anomaly, for a very simple reason: the *existing* paradigm already does that.

Philosophy and values enter when we reach Kuhn's "crisis science". The point of the new paradigm is not just to measure the anomaly, but to achieve some further aim. This could be conceptual, practical, epistemic, social, and so on. All these reasons might be 'scientific' in some sense, as Kuhn always insisted. But he also maintained that the change from one paradigm to another isn't necessary. The way the old paradigm hooks up to the world just stops being as effective. The teeth of the existing paradigm stop digging as deeply into the world, and scientists start thinking about new approaches that could dig further.

Understanding Kuhn's paradoxical view on anomaly illuminates one of his most perplexing views, that experimental anomalies do not rationally necessitate revolutions in science. When Kuhn was challenged on the "irrationality of paradigm shifts", he responded that revolutions *are* rational, they just don't rationally require or entail revolutions in science. The account of anomaly helps us to understand what he meant. The existing paradigm is commensurate with the new evidence, since after all scientists measured that new evidence under the existing paradigm. But the very process of making the new measurement gives us new information about the limitations of the existing paradigm. It doesn't hook on to the world in the right way, or it screens off relevant data, or it doesn't allow for broader calculations or reasoning that newly seem crucial. So, *from the perspective of the existing paradigm*, it is rational to seek another way to approach the same phenomena, and even to approach the same experimental process. Moreover, the process of measuring an anomaly gives scientists information about how the new paradigm could be constructed. While Kuhn's account of anomaly does not rescue the cumulative image of science, it does explain how Kuhn thinks the practice of scientific investigation is rationally connected over time.