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Beyond Kuhn: Methodological Contextualism and Partial Paradigms

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1. Kuhn's Image of Science: Normal Science, Crisis, and Extraordinary Science

Kuhn's view of science is as follows.[1] Science involves two key phases: normal and extraordinary. In normal science, *disciplinary matrices* (DMs) are large and pervasive.[2] DMs involve "beliefs, values, techniques, and so on shared by the members of a given community" (Kuhn 1996, 175). "And so on" is regrettably vague, but Kuhn (1977, 1996) mentions three other key elements: symbolic generalizations (such as $\mathbf{F}=d\mathbf{p}/dt$), models (such as Bohr's atomic model), and exemplars (which I explain below). These components of DMs overlap somewhat. For instance, symbolic generalizations may feature in techniques and beliefs, and models may exhibit values.

To be a (genuine) scientist, in the normal science phase, is to *puzzle solve* within the boundaries of the DM. It is to buy into the ruling dogma (Kuhn 1963) and to accept that "failure to achieve a solution discredits only the scientist ... 'It is a poor carpenter who blames his tools'" (Kuhn 1996, 80). Puzzle solving involves a wide variety of activities, including

[1] This introduction recapitulates material appearing elsewhere in this book to some extent. However, interpretation of Kuhn's work is not straightforward. Moreover, interesting aspects of his view of science are typically omitted even to the extent that it is. A key complication is that he moderated his treatment of several issues over time. I will therefore start by presenting an overview of my own interpretation of his view in the early to middle stage of his career, with supporting quotations from his work. As might be expected, my understanding of Kuhn's "image of science" bears many similarities to those of other authors, such as Hoyningen-Huene (1993), Bird (2001), and Wray (2011). However, it differs in several details that are potentially significant when it comes to assessing the plausibility of said image. I will not make it my business to highlight such details, as this will prove to be too much of a distraction. Instead, I will say only the following. The view of Kuhn *as I interpret it* is worth considering and engaging with irrespective of whether it is, or was, "the one true view" of the master. My treatment is also of special interest, in the present book, in so far as it doesn't focus primarily on (methodological or semantic) incommensurability.
[2] "Disciplinary matrix" is one sense of the term "paradigm," whereas "exemplar" is another. As this is confusing, and because Kuhn plausibly used the term "paradigm" in more than these two ways in the main body of *The Structure of Scientific Revolutions* – see Masterman (1970) – I avoid using the term. When "paradigm" appears in quotations, I clarify whether the meaning is "disciplinary matrix" or "exemplar."

bringing observations into closer agreement with theories (e.g., by altering auxiliary assumptions), articulating existing theories (e.g., by measuring constants), and classifying things, or kinds of thing, in line with the DM (e.g., measuring the magnitude of a star or the Young's modulus of an alloy).[3] It can sometimes resemble exploration, but isn't genuinely exploratory.

Kuhn (1996, 187) declares that exemplars are "the central element of ... the most novel and least understood aspect" of normal science. These are "concrete puzzle-solutions which, employed as models or examples, can replace explicit rules" (Kuhn 1996, 175) for puzzle solving. They typically involve shared theories and models – perhaps also shared symbolic generalizations – and exhibit shared values. They invariably use shared techniques. In essence, exemplars provide templates for tackling new puzzles as well as means by which to assess potential solutions to puzzles. Moreover, they can help in the identification of some new puzzles. Using them effectively involves spotting similarities, which is more practicable than attempting to internalize and follow rules.[4] Imagine, for instance, that a student has worked through a problem concerning circular satellite motion around Earth (involving classical mechanics and Newton's law of gravitation). She is subsequently taught Coulomb's law of electrostatic attraction. Provided she spots the relevant similarities – e.g., in the form of Newton's law and Coulomb's law – then she will be well-equipped to consider how a charged massless body might circularly orbit an oppositely charged massless body. It might also naturally occur to her that she could potentially deal with more complex puzzles, involving stable circular orbits with both gravitational and electrostatic forces present, in a similar way.

Kuhn holds that the practice of normal science will almost inevitably lead to the appreciation of various anomalies or "violations of [DM-based] expectations" (Kuhn 1996,

[3] For more detail on such puzzle solving activities, see Rowbottom (2011a and 2011b).

[4] Kuhn (1996, Ch. 5) discusses the significance of similarity judgements in considerable depth, with reference to Wittgenstein's remarks on family resemblance.

ix). Especially noteworthy are puzzles that are taken to be significant but unsolved. These can be tolerated, to the extent that they might reasonably be expected to be transient:

> Failure with a new sort of problem is often disappointing but never surprising. Neither problems nor puzzles yield often to the first attack … There are always some discrepancies. Even the most stubborn ones usually respond at last to normal practice (Kuhn 1996, 75, 81).

However, long-term anomalies may eventually begin to shake the confidence of scientists in their DM: “insecurity is generated by the persistent failures of the puzzles of normal science to come out as they should” (Kuhn 1996, 68). At some point – whether this happens will depend on a variety of factors, some of which might be external to science – a “crisis” may result: “when confronted by even severe and prolonged anomalies ... [scientists] may begin to lose faith and then to consider alternatives, [although] they do not renounce the paradigm [DM] that has led them into crisis” (Kuhn 1996, 77). They do not renounce the DM because there is no live alternative, and because a DM is required:

> Once a first paradigm through which to view nature has been found, there is no such thing as research in the absence of any paradigm. To reject one paradigm without simultaneously substituting another is to reject science itself. That act reflects not on the paradigm but on the man. Inevitably he will be seen by his colleagues as “the carpenter who blames his tools” (Kuhn 1996, 79).

However, crisis leads to *extraordinary* science in so far as it directs efforts towards removing the anomaly or anomalies at its heart – in so far as it focuses scientists, especially eminent scientists, on solving the key unsolved puzzle or puzzles, which come to be perceived as *problematic* in character: “more and more attention is devoted ... by more and more of the field’s most eminent ... If it still continues to resist, as it usually does not, many of them may come to view its resolution as *the* subject matter of their discipline” (Kuhn 1996, 82-83).

Initially, Kuhn says, attempts to solve such problems will proceed from within the DM. But over time, as attempts fail, more and more liberties will be taken, and different articulations or versions of the DM will arise. And hence the (implicit) rules for puzzle-solving will be relaxed. Kuhn (1996, 83) puts it as follows:

> Through this proliferation of divergent articulations (more and more frequently they will come to be described as *ad hoc* adjustments), the rules of normal science become increasingly blurred. Though there still is a paradigm [DM], few practitioners prove to be entirely agreed about what it is.

Yet extraordinary science may go even further in so far as it may result in "the willingness to try anything … [and] the recourse to philosophy and to debate over fundamentals" (Kuhn 1996, 91). Thus it is implicit in Kuhn's writing – on a note to which we will return – that extraordinary science, like crisis, is a matter of degree.

The presence of crisis and the shift to extraordinary science sets the stage for a new DM to emerge. But this is not the only possible outcome. Sometimes normal science under the existing DM saves the day. On other occasions, no new candidate DM is found in a reasonably timely fashion, and work in the area is suspended. Suspension might be temporary – new technology might pave the way for progress, for instance – or permanent.

From the Descriptive to the Normative

Thus far, we have seen how Kuhn *describes* science, based largely on his historical studies. (It should be remembered, though, that observation in the history of science is plausibly as theory laden as observation in science.) Kuhn doesn't stop there, however, because he thinks that one can derive some "oughts" from the "ises." In his own words, "the descriptive and the normative are inextricably mixed" (Kuhn 1970b, 233).[5] He also explicitly states, in the postscript to *The Structure of Scientific Revolutions*, that he does "present a viewpoint or theory about the nature of science … [which] like other philosophies of science … has consequences for the way in which scientists should behave if their enterprise is to succeed" (Kuhn 1996, 207). His justification for this move appears to be evolutionary in character; he claims – but does not argue – that the methods used by scientists "have been developed and selected for their success" (Kuhn 1996, 208). And on this basis, he thinks his "descriptive generalizations are evidence for the theory precisely because they can also be derived from it" (Kuhn 1996, 208). Kuhn (1963) makes a similar move in his argument for the value of dogma and indoctrination (of a fashion) – including intentional distortion and misrepresentation of the history of science – in science education. In short, he claims that science education *as he describes it* is well suited to the task of creating good ("normal") scientists.[6]

But why does Kuhn think that science *as he describes it* is good science? The key to the answer lies, as I've already intimated, in the normal phase, which Kuhn (1970a, 6) takes to be more central, in characterizing science, than the extraordinary phase:

> it is for the normal, not the extraordinary practice of science that professionals are trained… If a demarcation criterion exists (we must not, I think, seek a sharp or decisive one), it may lie in just that part of science…

Normal science is crucial, on Kuhn's view, for several *strategic* reasons.[7] The following three are key, and involve not only its immediate products, but also its role in fomenting crises and

[5] Slightly later, Kuhn (1970a, 237) adds: "If I have a theory of how and why science works, it must necessarily have implications for the way in which scientists should behave if their enterprise is to flourish."
[6] For more on Kuhn on science education, with special reference to indoctrination and dogma, see Matthews (2004) and Rowbottom (2016).
[7] Kuhn (1970b, 243, 246) emphasizes the strategic nature of his aforementioned claim as follows:

> Even given a theory which permits normal science ... scientists need not engage the puzzles it supplies. They could instead behave as practitioners of the proto-sciences must; they could, that is, seek potential weak spots, of which there are always large numbers, and endeavour to erect alternate theories around

thus extraordinary science (of a particular kind). First, the confidence (or even faith) involved means that the group is focused on completing similar tasks, including tasks with practical benefits, and is able to concentrate its efforts on completing those tasks. In the absence of a DM to which the community is *strongly* committed, hypercriticism – involving prolonged and unproductive squabbles about fundamental matters (of a metaphysical variety, *inter alia*) – is a serious potential problem. Kuhn puts it as follows. On the one hand, "Because they can ordinarily take current theory for granted, exploiting rather than criticizing it, the practitioners of mature sciences are freed to explore nature to an esoteric depth and detail otherwise unimaginable" (Kuhn 1970b, 247). And, on the other hand, "The scientist who pauses to examine every anomaly he notes will seldom get significant work done" (Kuhn 1996, 82).[8]

Second, on a related but subtly different note, it is far more efficient to solve puzzles using existing techniques, assuming they are fit for the purpose, than it is to endeavour to try to solve them in another fashion. In the words of Kuhn (1996, 76):

> So long as the tools a paradigm [DM] supplies continue to prove capable of solving the problems it defines, science moves fastest and penetrates most deeply through confident employment of those tools. The reason is clear. As in manufacture so in science—retooling is an extravagance to be reserved for the occasion that demands it.

> them. Most of my present critics believe they should do so. I disagree but exclusively on strategic grounds ...

[8] On occasion, Kuhn goes considerably further. For example, he claims that "during the period when the paradigm [DM] is successful, the profession will have solved problems that its members … would never have undertaken without commitment to the paradigm [DM]" (Kuhn 1996, 24-25). This is imprudent, especially from a historian, in so far it involves a counterfactual historical claim. (For more on such claims, see Tucker (2004).) And in any event, the claim is plausibly false even if one assumes it is truth-valued. This is because people may be motivated to solve problems or puzzles for a variety of reasons, both intrinsic and extrinsic. For instance, I might employ someone else's theory to solve what they take to be a "puzzle" (using their preferred exemplars), despite entirely disbelieving in said theory; I might have set out to refute the theory by showing that the puzzle couldn't be solved, but have succeeded against my expectations. Think also of the way in which one can solve problems in relevant logic, or other systems of deviant logic, without thereby "buying into" such logics in any significant sense. On a related note, this shows that Kuhn (1996, 80) is also imprudent to write of puzzles "for whose very existence the validity of the paradigm [DM] must be assumed." Kuhn would have been wiser to claim only that the presence of a DM is a major element of a set of *sufficient*, but not *necessary*, conditions for developments of the kinds he values. I will return to this issue in the next section.

Third, and finally, Kuhn believes that long periods of normal science are useful in so far as they are liable to indicate the true limits of the ruling DM, and especially to identify puzzles that are genuinely insoluble within the constraints of said DM. Kuhn (1996, 65) puts it so: "By ensuring that the paradigm [DM] will not be too easily surrendered, resistance guarantees that scientists will not be lightly distracted and that the anomalies that lead to paradigm [DM] change will penetrate existing knowledge to the core." Elsewhere, he amplifies the point as follows: "Because … exploration [of the DM] will ultimately isolate severe trouble spots, they [normal scientists] can be confident that the pursuit of normal science will inform them when and where they can most usefully become Popperian critics" (Kuhn 1970b, 247). In short, that's to say, Kuhn's idea is that extraordinary science *coming after an extended period of normal science* will tend to be superior to the similar kind of approach that may occur in nascent (or proto-) science, because its focus will be sharper and more appropriate. This point is especially persuasive if one accepts that many practical problems on which science bears – problems of engineering, for instance – may persist through DM-change.

This concludes my account of Kuhn's (early-middle period) image of science. To the extent that it is vague or silent in several significant respects – for example, on how different DMs should be distinguished from variants of the same DM, on how many people it takes to form (and thus what legitimately counts as) a community, on when and how precisely normal science terminates and extraordinary science begins, and on what socio-economic societal conditions are necessary (or at least sufficient) for science to occur – I typically hold Kuhn, rather than myself, responsible.

I will cover some such defects of Kuhn's image in the next section, although these will not be my sole focus. I shall also cover several problems with the most developed aspects of his account. In the third section, I shall propose a means by which to address these problems. I will explain how this involves rejecting the Kuhnian image while retaining some elements thereof.

2. Assessing Kuhn's Image

Kuhn's image is problematic in both the descriptive and normative dimensions. Concerning the former, one may legitimately doubt – as Toulmin (1970) does – whether all science, or even most science, fits the Kuhnian mould. Consider a domain in a mature science, in a particular year, picked at random: atomic theory in 1910, biomechanics in 1970, and so forth. Should we expect this to exhibit normal science (of a crisis-free variety)? More trenchantly – since the former expectation would rest on a probability claim – if we make many such random picks will we find that the relative frequency of normal science (of a crisis-free variety) is considerably greater than one half, as Kuhn suggests? The simple truth is that we don't know, as we didn't when Toulmin (1970) was writing. Moreover, even if this research were to be assiduously conducted, legitimate disagreements would arise about which picks instantiated normal science. Recall, for example, Kuhn's (1996, 79, 83) claims that there is "no such thing as research in the absence of any paradigm [DM]" and that there may be a "paradigm [DM] … [although] few practitioners prove to be entirely agreed about what it is." Combine these with his insistence that there are *versions* of theories and DMs – which features in claims such as: "by proliferating versions of the paradigm [DM], crisis loosens the rules of normal puzzle-solving" (Kuhn 1996, 80) – and the difficulties should be evident. One can't look to what the practitioners think to determine whether there's a single disciplinary matrix (although one *might* look to what they do, in so far as this is historically possible). Moreover, one can't be sure that differences in approach – even apparently pronounced differences – entail differences in disciplinary matrix, because Kuhn gives no guidance about how to judge when two different collections of shared commitments – to exemplars, methods, values, and so forth – constitute versions of the same disciplinary matrix, as opposed to different disciplinary matrices.

The likely riposte to this is as follows: it's a straightforward matter of similarity. However, this will not do. Even granting that there is a way to select a principled unique measure of similarity – and this is far from obvious (in so far, for example, as there may be no context-independent fact of the matter about the extent to which overall resemblance obtains between any two things)[9] – it is evident that *similarity comes in degrees*.[10] Thus it is implausible that there is a "critical point" at which a single change to a disciplinary matrix makes it cease to be a *version* of its predecessor, rather than an entirely different disciplinary matrix. Indeed, the very notion that disciplinary matrices may be *versions* of one another appears to be arbitrary. Why think this, rather than simply that they may be similar to greater or lesser degrees? The problem for Kuhn is that the retreat to discussing only degrees of similarity allows that many changes might legitimately be made, and the very division between normal and extraordinary science is under threat. In its place is a continuum. It is natural to think, moreover, that this continuum will – and to foreshadow the discussion below, should – vary according to the scientists' degree of confidence in a DM, or confidence in a group of available DMs, being able to solve the set of available or pressing puzzles (some of which might count as anomalies).

Two further remarks are in order before I proceed to discuss the normative aspects of Kuhn's account, which will be my focus in the remainder of this chapter. First, Kuhn emphasizes only degrees of change when doing so tends to suit his narrative about science,

[9] On the prospects of so-called "anti-resemblism," with special reference to its role in Lewis's philosophy, see Guigon (2014).

[10] The point may be illustrated, albeit imperfectly, by a simple analogy. Consider a picture taken by a digital camera, stored on your computer. You may easily create versions with smaller file sizes, by reducing the resolution of the picture. The more the resolution is reduced, the more grainy, and hence less similar to the original, any such version becomes. For an attempt to formally capture degrees of similarity, see Williamson (1998).

rather than the narratives of his opponents (such as Popper). For example, he writes: "the existence of a crisis does not by itself transform a puzzle into a counterinstance. There is no such sharp dividing line" (Kuhn 1996, 80). That's to say, he emphasizes the role of degrees to which anomalies are seen as significant (which one might take to bear on degrees of confidence in any DM). Second, note that many of Kuhn's claims about DMs are too strong, because they neglect the possibility of *sets* of active DMs. Consider, for example, Kuhn's (1996, 65) claim that "anomaly appears only against the background provided by the paradigm [DM]." Nowhere does Kuhn argue that anomaly cannot appear instead against the background provided by a group of active DMs. And it might, in so far as each of a group of active DMs – in one and the same community – may involve some of the same puzzles. One could *declare* any such DMs to be "versions of the same DM," but we have already seen that this is arbitrary.

Now let's consider Kuhn's image of science from a normative perspective, even granting that it is descriptively accurate. The most implausible aspect lies precisely in the putative link between the descriptive and the normative. Wouldn't it be a miracle if mature science *as it is done* were optimal, or at least approximately so, from a "strategic" perspective? Appeal to an evolutionary analogy – recall Kuhn's (1996, 208) suggestion that the methods used by scientists "have been developed and selected for their success" – will not do the trick. Not all mutations have been explored, and the possible mutations can change over time; we now have access to technologies we previously didn't, for example. Moreover, what's perceived as success can change over time – curiously, Kuhn argues for this – and thus what was previously selected against might not be selected against were it to later reappear (and it might be unlikely to so reappear, if it has been forgotten). Analogously, we know that we are far from optimal in many respects despite having gone through (along with our evolutionary ancestors) many more generations than science (or its methods, theories or matrices). One of the most striking recent illustrations of this was the invention of an exoskeleton that "consumes no chemical or electrical energy and delivers no net positive mechanical work, yet reduces the metabolic cost of walking by 7.2 ± 2.6% for healthy human users under natural conditions" (Collins et al. 2015, 212).

Perhaps Kuhn would respond to this by granting that improvements are possible, while denying that any of these would involve changing the process he sees as *central* to, and *necessary for*, good science – the cycle of normal science (i.e., dogmatic puzzle-solving under a single DM), crisis, extraordinary science, and revolution. Yet how does Kuhn purport to know this, even assuming that he has spotted a genuine pattern in the history of science? There are numerous alternative processes, many of which, no doubt, Kuhn didn't conceive of. And this throws serious doubt on his remarkably strong claims about the impossibility of achieving many things in any other way, which appear throughout *The Structure of Scientific Revolutions* (and which I've already criticized to some extent, with reference to counterfactual history, in footnote 8). Here's a selection:

> during the period when the paradigm [DM] is successful, the profession will have solved problems that its members … would never have undertaken without commitment to the paradigm [DM] (Kuhn 1996, 24-25).
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> within those areas to which the paradigm [DM] directs the attention of the group, normal science leads to a detail of information and to a precision of the observation-theory match that could be achieved in no other way … (Kuhn 1996, 65).
>
> [there are puzzles] for whose very existence the validity of the paradigm [DM] must be assumed (Kuhn 1996, 80).

I have already said something about the final claim – the quick refutation, recall, is that one can recognize and even solve puzzles in frameworks one doesn't think are valid, e.g., deviant logics – so will now focus on the previous two. Imagination suffices to see their probable falsity. Consider the first. Imagine a community of scientists who think that the DM they use is invalid (whatever exactly that means). They have no dogmatic commitment to it. Perhaps they think it's dumb. But they're prisoners. Slaves. Their captors insist that the scientists work on this DM, or face terrible punishments. The captors need not even be committed to the DM. They could have many different groups of scientists held captive, each working on a different DM, in order to hedge their bets. They could be after a particular practical outcome, such as the development of a powerful new weapon. They could also be knowledgeable enough to do the work themselves – this would explain their ability to design the DMs – but prefer only to oversee it, given their wicked ways.

Now consider the second claim. The example above has already also refuted this, provided normal science fundamentally involves dogmatic commitment, as it does on Kuhn's view. But even if we relax this constraint, other plausible counterexamples are easy to come by. Consider, for example, a small group of dissident scientists, who refuse to use the dominant DM in their area despite the absence of crisis. Instead, they work together to develop and use alternative approaches. (If preferred, one can think that they have their own DM. But Kuhn *doesn't* advocate two DMs existing in the same area simultaneously – or even DM variants existing in the same area simultaneously – in the absence of crisis.) Their twin aims are to show that the dominant DM should be replaced and to solve some pressing practical problems that its users have not yet been able, although they expect eventually to be able, to solve. Why should we think that this dissident group will not acquire the same "detail of information" or "precision of observation-theory match" as the orthodox community? Why might they not strive for and achieve more, per capita per unit time, than the orthodox community? They might be especially driven by the fact that their results may have to be *more* precise than those of the orthodox group, in order for their work to be taken seriously (by third parties, perhaps, in the first instance).

The only reply that appears feasible – and then, only *prima facie* – is that if the dissident scientists joined the orthodox community, that community would do better (in terms of "detail of information" acquired and "precision of observation-theory match").[11] I suppose this may seem plausible from a highly simplistic perspective, according to which progress made is directly proportional to time spent. But this perspective is fundamentally flawed. Productivity per capita per unit time depends on numerous factors, including group cohesion and structure, the roles of and capacities of group members, and so on. And in the example above, attempts by the dissident scientists to integrate might be disastrous. They might prove to be somewhat disruptive influences in several respects, even if they did their best to toe the line. They might hamper efficiency. As we academics know all too well, for instance, just one malicious colleague can seriously diminish the efficiency of a department, in many respects. Meetings may take longer. Results may be the result of awkward compromises. Some junior department members might even be unfairly denied tenure, and drop out of academia despite having much to contribute.

Several other legitimate criticisms of Kuhn's image of science are possible – see, for instance, Mizrahi's (2015) attack on the semantic incommensurability thesis – but those I offer above are sufficient to suggest what a suitable replacement might look like. In the penultimate section, which follows, I shall say something about this.

3. Beyond Kuhn's Image: Rational Piecemeal Change

In Rowbottom (2011a & 2013), I argue that science may be better served by normal science and extraordinary science – or something akin to each – co-existing to some extent. More specifically, I argue that science may benefit from having puzzle solving functions, critical

[11] The only other evident option is that it would be better for all concerned if the dissidents just gave up trying to do science altogether. I take this to be false on its face, so won't argue against it here.

functions and imaginative functions being performed simultaneously by its practitioners. But this doesn't mean that each and every scientist should perform all such functions. One of the merits of the picture is that it recognizes that there are "horses for courses": it embraces the fact that some scientists are better suited to, and hence deployed in, performing some functions rather than others. For instance, a highly critical and imaginative scientist may be poor at puzzle solving: he may be too flighty, too easily bored by such activities. Nevertheless, he may be excellent at identifying previously unknown anomalies, and generating promising new theories (that others might want to investigate at some point). His presence might therefore result in the identification of anomalies more swiftly than would happen in normal science.[12] (This scientist may be well aware of what puzzle solving has been, and is being, done. This only requires keeping up with the literature in the field.)

Similarly, a highly dogmatic scientist, intent on defending her pet theories and approaches come what may, might be excellent at pushing those resources to the maximum. She might continue to try to save her favoured theories when less stubborn scientists would have given up – and even when the field, as a whole, considers them falsified. And it's possible that such activity might prove beneficial. Even if there's only a small probability that it will prove successful, it's allowable, from the perspective of the group as a whole, in so far as this scientist might be useable in no other, or at least no better, way. Thus the group may exploit her, even if her dogmatism is irrational, provided that her outputs are evaluated and employed appropriately.

So how should the balance between the different functions – and sub-functions, such as articulation and evaluation, which I will not enumerate here – be struck? This is context dependent. It depends on the scientists available, as intimated above, as well as the state of

[12] I here allude to one of Kuhn's aforementioned reasons for thinking normal science is good – namely, that it identifies persistent, significant, anomalies. All I suggest here is that the presence of some critical scientists may increase the number of available anomalies per unit time, or make the anomaly finding process swifter. The anomalies need not be seen as counterinstances by any of the puzzle solvers, even if they are seen as counterinstances by some of the critical scientists.

science at the time. With respect to the latter, for instance, the amount of effort devoted to imagination may usefully rise in response to a situation where existing theories don't appear to be sufficient to solve several puzzles or problems (and usefully lower when there are few apparently insoluble puzzles). But there needn't be a crisis, or extraordinary science involving all (or almost all) scientists, or anything like that. In short, science may be highly adaptive. This is what I mean by *methodological contextualism*, which I commend to you as a more attractive (social epistemological) theory of change than Kuhn's comparatively rigid, cyclical, alternative.

I hope this makes the basic view clear. In the remainder of this section, I will build on this previous work by considering how *piecemeal change in science might rationally occur* on such a picture. I shall argue that *partial* DMs and groups thereof may be used as (temporary) fixed points to enable exploration.

There are three things I should say to prepare the ground. First, I will make free use of many of the concepts used by Kuhn, and discuss many of the aspects of science that he perceptively identified. Especially noteworthy are his aforementioned ideas that dogmatism can prove useful and that exemplars are of considerable epistemic and pragmatic significance.

Second, my account is inspired by the reticulated view of change presented by Laudan (1984). The basic idea behind Laudan's view is simple: scientists may reasonably hold some parts of their framework fixed, while considering how the others might beneficially change. With this, I wholeheartedly agree. However, Laudan's view was mainly concerned with the triad of theories, methods, and aims; in essence, he argued that scientists might hold two members of the triad fixed, while exploring the wisdom of altering the other item. My account differs from Laudan's primarily, as you will shortly see, in so far as it is:

more fine-grained – it involves more than theories, methods, and aims; and less restrictive – it allows rational piecemeal change to involve altering several items simultaneously.[13]

Third, I should draw brief attention to the notion of "incommensurability," which Kuhn uses to refer to the "relation of methodological, observational and conceptual disparity between paradigms [DMs]" (Sankey 1993, 759) in his early writing. On the view I'll present, such disparity may be limited such that it is never so severe as Kuhn suggests it is. More specifically, I will show how it's possible for it to be incorrect that "the transition between competing paradigms [DMs] cannot be made a step at a time" (1996, 150), and that "transfer of allegiance from paradigm to paradigm [DM to DM] is a conversion experience" (1996, 151). I take the falsity of these claims to be a *consequence* of the way that piecemeal change can occur.

This brings me to my alternative account of scientific change. The idea I want to promote is that a given group of scientists may legitimately work with a *partial* DM, or a set of *partially overlapping partial* DMs. A partial DM is just like a DM, but with some parts stripped away or diminished. So it is partial precisely in so far as it doesn't involve all the elements that Kuhn takes DMs to involve, or involves some such elements in a lesser capacity.[14] For instance, a partial DM might involve no shared theory, despite having many well-defined shared values, shared methods, and shared metaphysical commitments.[15] The absence of any shared theory doesn't entail that there aren't active theories. On the contrary,

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[13] I also endorse the revision to Laudan's view proposed by Resnik (1994, 343), namely allowing that "criteria of goal assessment have no privileged status."

[14] One might doubt whether Kuhn requires all elements to be "filled" for a DM to exist. However, he does advocate the view that DMs *should* ideally include all such elements: "The more precise and far-reaching [a] paradigm [DM] is, the more sensitive an indicator it provides of anomaly and hence of an occasion for paradigm [DM] change" (Kuhn 1996, 65). I may be understood to be denying this normative claim, at this juncture.

[15] I am taking a *scientific* theory to be more than a universal generalization of the form "All Xs are Ys." Almost always, some such generalizations will be believed by all scientists: I put those under the category of other shared beliefs (and perhaps metaphysical commitments). One might think of some of these as absolute presuppositions – see Toulmin (1967) for more on this notion, which stems from Collingwood (1940) rather than Kuhn.

there may be several active theories. There could be considerable difference of opinion, in the community, about which theories are superior (given the values and evidence).

Partial DMs may also involve a relative paucity of shared content – be diminished by comparison with a DM, that's to say – in a particular respect. A partial DM of this kind, for instance, might involve very few shared exemplars. Many different shared exemplars might nevertheless exist in different sub-communities: there might just be widespread differences of opinion about which approaches are best, perhaps because the shared values don't suggest a unique supremacy ordering.

One needs to think about how the various components of DMs relate in order to grasp what's possible when it comes to partial DMs. That's because these components are interconnected in interesting ways: for instance, some methods and exemplars are theory-specific. But not all need be. This is clear in the case of methods, which may be experimental: for example, there might be established ways to categorize the brightness of celestial bodies without there being any shared theories about what the bodies are, how they move, and so forth. With exemplars, matters are less clear; but note that one can have the same kind of puzzle solving processes – at *some* level of abstraction, at least – when employing two different theories or symbolic generalizations. Consider two different theories about the forces exerted by a spring relative to its displacement. One is $\mathbf{F}=-k\mathbf{x}$, where k is a constant which may differ in value for different springs. (This is our very own Hooke's law.) The other is superficially the same, $\mathbf{F}=-k\mathbf{x}$, but k is a universal constant (i.e., the same for all springs). It is clear that many of puzzles about the forces (putatively) exerted by springs are going to be solved in a similar way in both cases. But the theories *aren't* the same. Something similar is true, moreover, if we allow relatively minor variation in symbolic generalizations. Consider, for instance, $\mathbf{F}$=-k$\mathbf{x}$+1 or $\mathbf{F}$=-k/$\mathbf{x}$.[16]

This brings me to *partial overlap* between partial DMs, which should now be easy to comprehend. Partial overlap may exist in so far as some elements may be shared. The relative extent of partial overlap is also easy to determine in some cases. For example, consider three partial DMs: A, B, and C. All components of B and all components of C are also components of A. Moreover, all components of C are also components of B. However, A has more components than B, and B has more components than C. Thus, B overlaps with A more than C overlaps with A.[17]

Consider now how partial DMs may be employed on a regular, even ongoing, basis in science. First, scientists may use them in order to explore a broader range of approaches simultaneously than would be allowed by a DM. So rather than waiting for crisis, the variety of partial DMs employed might be adjusted, on an ongoing basis, in line with the extent to which anomalies prove persistent. Moreover, much more significantly, *using multiple partial DMs may be useful for exploring possibilities that Kuhn's recommended approach doesn't allow*, and which are especially interesting from an anti-realist perspective on science. For example, it may be the case that no available (or even conceivable) theory can solve all the available puzzles (in a domain), although using two or more different and conflicting available theories, in different contexts, suffices to solve the puzzles. The idea here, in short, is that scientists might allow inconsistency at the global level, and even welcome it as a better alternative than a consistent system with less puzzle-solving power.

[16] If a non-hypothetical example is wanted, consider that some attempts to account for the unexpected motion of Mercury involved proposing laws of gravitation that were only approximately inverse square.

[17] Kuhn similarly allowed, *at least at some points*, that DMs could overlap. For instance, Kuhn (1977, 355) suggested that some values may be present in all DMs:

> whatever their initial source, the criteria or values deployed in theory-choice are fixed once and for all, unaffected by their participation in transitions from one theory to another. Roughly speaking, but only very roughly, I take that to be the case. If the list of relevant values is kept short ... and if their specification is left vague, then such values as accuracy, scope, and fruitfulness are *permanent attributes of science* [my emphasis].

In situations where it's clear that a partial DM or group of partial DMs will need to be discarded (e.g., due to persistent anomalies), second, it is possible to explore a variety of similar alternative partial DMs and to see to what extent it's possible to preserve what's already shared. For example, only a change in shared values, or only a change in shared theories, may be needed. It might even be the case that the set of shared exemplars or the set of shared models are inadequate, in so far as more members of each are required. Indeed, typically a theory in isolation is predictively sterile – one normally requires models of specific systems, in order to issue predictions using the theory.[18] Not all cases will be like this, of course. But the point of piecemeal exploration is to avoid unnecessary retooling, for pragmatic – and potentially, if one is a realist, epistemic – reasons. As Kuhn (1996, 76) remarks, recall, "retooling is an extravagance to be reserved for the occasion that demands it." To this, however, we may add that one should only retool to the extent that retooling is necessary: one need not replace all of one's tools if the desired end can be achieved by only replacing some of those tools. (And sometimes there may be more than one way to replace only a proper subset of tools in order to achieve the desired result.)

This concludes my outline of how we may move beyond the Kuhnian image in the normative dimension. (It incidentally suggests also how one might move beyond it in a descriptive dimension. For example, one might take partial DMs, or sets thereof, to exist as a matter of fact now, and to have been present at various points in the history of science.) I have only been able to give a sketch on the present canvas, but I believe that it's a promising one. Details may be added in many different ways.

4. Conclusion

[18] For more on this, with special reference to the simple pendulum, see Rowbottom (2015; Manuscript, Ch. 1).

I expect the following criticism of the proposal I've made. It seems complicated, messy even, and doesn't give as clear a picture of science, or ideal science, as Kuhn does (or as his key opponents, such as Popper, do). My answer to this is as follows. In philosophy of science, as in science, simplicity is not an indicator of truth-likeness; it is a pragmatic (theoretical) virtue.

The most important message of this chapter is that we should strive to embrace complexity to a much greater extent, if we want to understand scientific method. Yet (general) philosophers of science have an unfortunate tendency to overgeneralise, coupled with a desire to find a single simple "master" account (or image) of their target.

In reality, two or more mutually inconsistent yet simple accounts of science might each have the virtue of highlighting – and rendering easily comprehensible – different descriptively or normatively significant aspects of science.[19] The complex true account – and perhaps any approximately true account, with the same putative scope – might tend to obscure those aspects, not least in so far as it must detail how they interrelate. Thus, "embracing complexity" in the sense I intend it need not involve developing such a single, more complex, account of science. It certainly need not involve *merely* developing such an account. Rather, it may involve developing new models, new images, of science. Some may even be wilfully intended to distort some aspects thereof, in order to effectively represent others.

We might also endeavour to get clearer about the extent to which existing accounts should be taken seriously, and the extent to which they should not be. By analogy, it's clear that the model of the simple pendulum captures how changes in length tend to affect period of swing. And we use it (and teach it) for that reason, among others. But we don't take pendulums to be perpetual motion machines (and we don't want science students to think they are). We appreciate the virtues and limits of the model.

[19] To some extent, I think this is true of both Kuhn's account and Popper's account. This is why the overarching functions they identify each have a part in the more complex methodological picture developed in Rowbottom (2011a & 2013).

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