Normal Science: not uncritical or dogmatic

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Abstract

When Kuhn first published his *Structure of Scientific Revolutions* he was accused of promoting an "irrationalist" account of science. Although it has since been argued that this charge is unfair in one aspect or another, the early criticism still exerts an influence on our understanding of Kuhn. In particular, normal science is often characterized as dogmatic and uncritical, even by commentators sympathetic to Kuhn. I argue not only that there is no textual evidence for this view but also that normal science is much better understood as being based on *epistemically justified* commitment to a paradigm and as *pragmatic* in its handling of anomalies. I also argue that normal science is an example of what I call Kuhn's program of *revisionary rational reconstruction*.

Keywords: Kuhn, normal science, dogmatism, rationality, rational reconstruction

1 Introduction

The early reception of Kuhn's *Structure of Scientific Revolutions* was highly critical (see Shapere 1964, Lakatos and Musgrave 1970, Scheffler 1982). The criticisms were diverse: they ranged from the vagueness of the paradigm notion to the threat of epistemic relativism, irrationality, and what Lakatos called "mob psychology".[1] These criticisms have had a lasting effect on the way in which Kuhn has been received in our community, as can be gleaned from philosophy of science textbooks. Okasha, for example, writes in his *Philosophy of Science: A Very Short Introduction:* "With some justification, Kuhn's readers took him to be saying that science is a *largely non-rational activity*, characterized by dogmatic adherence to a paradigm in normal periods, and sudden 'conversion experiences' in revolutionary periods" (Okasha 2002/2016, 84; my emphasis). Other philosophy of science textbooks similarly discuss Kuhn's account as a potential threat to scientific rationality (see e.g., Chalmers (1999), Bird (2001), Ladyman (2002/2006), and Bortolotti (2008)).

Since the early criticisms, several authors have argued that Kuhn's view is compatible with rationality in one way or another. For example, some authors have argued that Kuhn's exemplar-based view of science need not be irrational, despite not being rule-based (Bird 2001); that Kuhn's

[1] See see Wray (2021b) for an overview.

account makes better rational sense of the diachronic dimension of scientific theories than Popper (Díez 2007); that Kuhn's account is compatible with the collective rationality of communities (rather than individuals) (Brown 1988, Hoyningen-Huene 1992, Wray 2011, Mladenović 2017); that Kuhn's account is best thought of in terms of a coherentist account of epistemology (Kuukkanen 2007); or that Kuhn's view is best viewed in terms of rational pursuit, rather than acceptance (Šešelja and Straßer 2013). Still others have argued that Kuhn provides an account of rational disagreement (Seidel 2021).

Most of these defenses have focused on Kuhn's idea of paradigm change, which is clearly the most controversial part of Kuhn's account. However, the charge of irrationality was originally directed also against normal science (Popper 1970, Watkins 1970). Few authors have since been as critical of normal science, but the view is still widespread that normal science is dogmatic and uncritical, which are precisely those attributes that Popper and Watkins took to be detrimental to scientific progress. The received view of normal science seems to be that normal science *is* dogmatic and uncritical *but not* irrational.

I will argue here that the received view not only makes the notion of normal science epistemically problematic, but it also mischaracterizes normal science: Kuhn himself never espoused the language of dogmatism or uncritical attitude in *Structure.* Positively, I will argue that normal science activity is best characterized as *epistemically justified* by the successful applications of the paradigm and *pragmatic* about paradigm's imperfections, making normal science a perfectly epistemically rational endeavor. I will also argue that there is evidence that Kuhn himself thought about normal science along these lines. In my analysis I mostly focus on Kuhn's main work, *Structure*, but I also consider later works that I believe are consistent with Kuhn's characterizations of normal science in *Structure*.

I will proceed as follows. In section 2 I briefly introduce the notion of normal science, Popper's early criticism of it, and what I take to be the received view about normal science. I give three reasons for why I think the received view is mistaken. In the following sections I spell out these three reasons in detail. In section 3 I argue that normal science and commitment to a paradigm is best understood as epistemically justified and pragmatic. I also show that there is textual evidence that Kuhn himself thought along these lines and no evidence for the received view. In section 4 I argue that apparent textual evidence in Kuhn's work for the received view must be read as pertaining to the education of science students rather than scientists engaged in normal science research. In section 5 I turn to some objections to my interpretation of normal science. In section 6, I show that normal science is a prime example of Kuhn's meta-philosophical reflections about his own work; his approach to the history of science, I argue, is best described as *revisionary rational reconstruction*. Section 7 concludes this essay.

2 Normal science and its reception

In this section I will first present normal science in a nutshell, then briefly review Popper's accusation that normal science is dogmatic and uncritical, and thus irrational. I will then discuss what I regard as the received view about normal science.

2.1 Normal science in a nutshell

Kuhn's basic ideas from *Structure* are widely known, and they need not be rehearsed here in detail. Just roughly, Kuhn believed that the history of science can be broken down into periods of normal science and periods of revolutionary science. Periods of normal science are characterized by the scientific community adhering to a paradigm, which encompasses a governing theory (such as Newtonian mechanics) metaphysical views (such as action-at-a-distance), certain types of experimental practices, instruments, skills, and "exemplars", i.e., examples of good scientific practice.[2] In scientific revolutions, paradigms are replaced by new, radically different, paradigms.

Normal science activity consists in attempts to increase the 'scope' and the 'precision' of a paradigm. When a paradigm is first proposed, naturally, the success of a paradigm is "largely a promise of success discoverable in selected and incomplete examples" (23-24).[3] Normal science seeks to actualize this promise (24). There are several dimensions of this normal science activity: the determination of constants of equations set out by the paradigm (e.g., the gravitational constant G in Newton's law of gravitation), increasing the match between the predictions of the theory associated with a paradigm and observations, increasing the precision of instruments, increasing the range of applications of the paradigm, and more (25-34). Normal science, as the name suggests, is the default mode of scientific activity. Accordingly, scientists spend "almost all their time" carrying out normal science activities (5). In contrast, revolutionary science Kuhn also describes as "extraordinary" (82f.) and as happening only "occasionally" (79).

Kuhn helpfully compares normal science to 'puzzle solving' activity (chapter IV of *Structure*).[4] There are two important aspects of this analogy: first, puzzle-solving requires rules and second, puzzle-solving is premised on puzzles having a solution that is to be found by adhering to these rules. Take for example a jigsaw puzzle. An essential rule of that kind of puzzle is that one must fit the pieces together so that they ultimately constitute a whole. Certain 'moves' are not allowed in the game: e.g., cutting off the edges of the pieces so that they fit would not count as a solution to the puzzle. Kuhn compares such rule violations to scientists occasionally proposing modifications of Newton's law of gravitation in order to accommodate the orbit of Earth's moon, before a solution was found in 1750, about 60 years after the first publication of Newton's *Principia* (39 and 81).

When one engages in the attempt to solve a jigsaw puzzle, one also has a clear expectation that there actually exists a solution that can be found by following the rules of the game (usually depicted on the back of the puzzle box). Without such an expectation, one would not be willing to

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[2] Kuhn himself – after friendly criticism (Masterman 1970) – attempted to clarify his notion of a paradigm by distinguishing between 'disciplinary matrices' and 'exemplars', as one element of disciplinary matrices. Disciplinary matrices, or paradigms in the broader sense, refer to all the 'group commitments' of the community (such as 'light is corpuscular' or 'action at a distance is intelligible'). Exemplars refer to the successful applications or 'achievements' of a research community. I will not make this distinction in what follows and will use the term 'paradigm' in the broader sense.

[3] If not otherwise specified, page numbers refer to the 3rd edition of Kuhn's *Structure* (Kuhn 1962/1996).

[4] For accounts de-emphasizing the role of rules see Bird (2001) and Nickles (2003).

spend hours trying to fit together the pieces into a coherent whole. The problems tackled by normal science, according to Kuhn, are just like that: they are also firmly believed to have a solution. In order for scientists to willingly engage in normal science activity, which can appear 'subtle' and 'esoteric' to outsiders (164), they must accept the fundamental assumptions underlying the paradigm.

2.2 Popper's accusation

Popper took exception to normal science and accused Kuhn of promoting a view of science that would present scientists dogmatic, unreflective, and uncritical:

> The 'normal' scientist, as described by Kuhn, has been badly taught. He has been taught in a dogmatic spirit: he is a victim of indoctrination. He has learned a technique which can be applied without asking for the reason why (Popper 1970, 53).

Popper goes on to argue that scientists trained in such a way are *applied* rather than 'pure' scientists and that puzzle solving activity in normal science concerns 'routine problems' and merely "applying what one has learned" (53). Popper denied that this kind of work was in any way 'normal' in science and that *if* it were it would be "a danger to science and, indeed, to our civilization" (53). Watkins – who alongside Popper in the same volume criticized Kuhn's concept of normal science – also picks up on the dogmatism theme and concludes that "Kuhn sees the scientific community on the analogy of a religious community and sees science as the scientist's religion" (Watkins 1970, 33). From these quotes it is quite obvious, I think, that both Popper and Watkins believed that Kuhn's normal science was absolutely detrimental to critical science and that if scientists actually were behaving the way Kuhn thought they did in normal science, then science would cease to be the rational and knowledge-generating enterprise that it is.

2.3 The received view

Contemporary philosophers of science have not nearly been as critical of Kuhn's notion of normal science as Popper and Watkins were. Still, Popper's and Watkins' characterization of Kuhn's normal science as dogmatic and uncritical has survived them. Here is a quote by Wray, in his recent book *Kuhn's Intellectual Path*, which I think epitomizes the received view:

> looking at the historical record, Kuhn came to believe that *scientists were rather dogmatic, uncritically accepting the theory they were taught*. Kuhn did not, however, intend this observation as a criticism of science. Rather, he believed that the *dogmatism that characterizes science* explains why scientists are so successful in accomplishing their research goals. When *scientists uncritically accept the theory they were taught*, it gives them the determination to make nature fit the conceptual boxes or categories that the accepted theory provides. (Wray 2021b, 86; added emphasis)[5]

[5] To be fair, Wray – seemingly in tension with the aforementioned quote – states in another publication from the same year that "Popper's characterization of the normal scientist as unreflective and dogmatic is thus a gross misrepresentation of what Kuhn had in mind" (Wray 2021a, 231). There, Wray focuses on paradigms as exemplars, though, and highlights that, on Kuhn's view, normal science allows for a lot of "flexibility" and creativity in the use of exemplars (Wray 2021a, 231).

Standard introductions to philosophy of science have adopted a similar language. For example, Chalmers, in his influential philosophy of science introduction, writes:

> Normal scientists *must be uncritical* of the paradigm in which they work. It is only by being so that they are able to concentrate their efforts on the detailed articulation of the paradigm and to perform the esoteric work necessary to probe nature in depth. (Chalmers 1999, 110, added emphasis)

Ladyman (2002/2006), in his widely used introduction to the philosophy of science, concludes his chapter on Kuhn by saying that for Kuhn "much of the practice of science is relatively routine, requiring a great deal of technical knowledge, but not necessarily a great deal of critical thought" (120). Two books dedicated to Kuhn's work come to similar conclusions: Bird attributes to Kuhn the view that normal science is "low on innovation and high on dogma" (Bird 2001, 36) and Hoyningen-Huene speaks of a "quasi-dogmatic element of normal science" (Hoyningen-Huene 1993, 194-196). There is even a paper dedicated entirely to reconciling Kuhn's "dogmatism" and Popper's "criticism" (Rowbottom 2011). Most recently, in a chapter on normal science in a volume dedicated to the 60th anniversary of *Structure*, Melogno (2024) argues throughout that "conservative thinking and suppressing critical activity are fundamental for normal science to develop" (89).

Abstracting away from the particular readings of normal science, one may say that according to the received view normal science is dogmatic and uncritical *without* entailing irrationality. The accepted rationale for normal science seems to be something like this:

> *Normal science is not irrational because scientists would not be willing to dedicate their time and resources to solving the puzzles set out by the paradigm if they were not uncritical and dogmatic about a paradigm's basic assumptions.*

I will refer to this as the *rationale of the received view*. I claim that this rationale is mistaken, for the following reasons:

(i) the received view misrepresents Kuhn's account of normal science; Kuhn never characterized normal science in terms of uncritical or dogmatic attitudes;

(ii) describing scientists' attitudes as dogmatic and uncritical may suggest – and certainly leaves open the possibility – that scientists adhere to a paradigm for no good epistemic reasons. This would render normal science activity an epistemically irrational endeavor;

(iii) normal science behavior can be fully accounted for by paradigms being epistemically justified and scientists making pragmatic choices, even by Kuhn's own terms; there is no need at all to invoke dogmatic and uncritical attitudes.

Claim (i) must be surprising, if not to say almost outrageous to proponents of the received view. However, Kuhn never *once* used the term "dogmatism" or its cognates in *Structure* in association with normal science. He did not even speak of scientists being "uncritical" towards paradigms or such.[6] Even in his work beyond *Structure*, Kuhn's linguistic usage was consistent (see section 4). The reason, I think, is evident: this was simply not his view. His view of normal science is much better captured in different terms, as I will show momentarily.

That claim (ii) is problematic should be obvious. Consider the possibility that scientists dogmatically and uncritically adhere to a paradigm for no good epistemic reasons and purely because of e.g., social reinforcement: they would adhere to a paradigm purely because they have been taught to do so or because their peers do so too. Such a view, Kuhn himself clearly rejected. As he put it in his *Reflections on my Critics*:

> no part of the argument here or in my book implies that scientists may choose any theory they like so long as they agree in their choice and thereafter enforce it. Though different solutions have been received as valid at different times, *nature cannot be forced into an arbitrary set of conceptual boxes*. On the contrary the history of proto-science shows that *normal science is possible only with very special boxes*, and the history of developed science shows that *nature will not indefinitely be confined in any set which scientists have constructed so far*. (Kuhn 1970b, 363; added emphasis)

I am not suggesting that Kuhn's sympathizers necessarily endorse the view that Kuhn rejects here. But usually, they do not invoke the explanation for the adherence to a paradigm that I think they ought to invoke. Before providing support for claim (iii) and further support for claim (i), let us first complete our picture of the received view.

There are not only those who outrightly condemn Kuhn's notion of normal science as irrational (such as Popper and Watkins), and those who are not opposed to normal science even though they misdescribe it as dogmatic and uncritical (as the ones just mentioned). There are even commentators who seem to reject criticisms of Kuhn's view as irrational *and* at the same time criticize Kuhn for not providing enough theoretical machinery for supporting a rational picture of science. Bird's view on Kuhn's normal science is particularly noteworthy (Bird 2001).

Bird argues that normal science is best understood not as rule-based activity, but rather as learning through concrete exemplary puzzle solutions and similarity relations between different solutions (Bird 2001, 71). He describes this type of learning as "a non-intellectual exercise that does not require the exercise of reasoning, inference, or the following of rules" (ibid.). Bird believes that this type of learning proceeds via a "feel for puzzles, an *intuition* for solutions, and sense of the rightness of answers" (70; original emphasis). Although Bird does not think that learning of exemplary puzzle-solutions justifies the criticism of "subjectivity and irrationality" that was mounted against Kuhn (74), he does suggest that *intuitions* about puzzle solutions need to be complemented by "reflective reason", which he thinks is lacking on Kuhn's picture (88). Bird even argues that "if there is room for *rational* argument then Kuhn needs to provide some mechanism for this in his explanatory apparatus. *And that is just what non-reflective judgments of similarity to exemplars seem to lack*" (90; my emphasis). In other words, Bird's verdict with regards to rationality in Kuhn's account seems divided at best. Maybe there is a way to regard intuitive judgments as

[6] There are only two occurrences of the term dogma and its cognates in *Structure*: on p. 75, it refers to ancient Greek "science", and on p. 162 it refers to theology (Kuhn 1962/1996).

rational. And maybe one way of doing so is to understand the learning processing underlying these judgments as running on computational neural nets, as Bird suggests (71-75).[7] But it certainly seems dubious that scientists could exercise any *critical* capacities towards normal science without making reflective judgments: how could one possibly be critical of something without reflecting upon it? Thus, similarly to the received view, scientists in normal science periods on Bird's view are unreflective (and therefore uncritical) about the paradigm they work in. Whether or not normal science is a rational activity is not decidedly answered by Bird. Finally, as we have already seen earlier in this section, Bird seems to agree with the description of normal science as dogmatic.

3 Normal science: epistemically justified and pragmatic

According to the received view, normal science is not irrational because scientists would not engage in puzzle solving activity if they were not to uncritically accept a paradigm and if they were not dogmatic about it. Against the received view, I will support claim (i) from the previous section by showing that Kuhn did not characterize normal science in terms of dogmatism or uncritical attitudes, even in places in which it may seem so at first sight. But I will leave that for section 4. In the current section I want to support my claim (iii):

> *Normal science behavior can fully be accounted for by paradigms being epistemically justified and scientists making pragmatic choices, even by Kuhn's own terms; there is no need at all to invoke dogmatic and uncritical attitudes.*

My argument for this claim is this:

1. Scientists' commitment to paradigms is *epistemically justified*: paradigms are empirically well supported. So long as this justification exists, and no better alternatives are available, there are no good epistemic reasons for scientists to change their paradigm.
2. Likewise, when there is epistemic justification for a paradigm and no better alternatives are available, there are no good epistemic reasons for scientists to *criticize* the paradigm (with the view of abandoning it); but this is not to be confused with scientists being uncritical, as that would suggest that scientists are unwilling or incapable of exercising their critical capacity, even if it was appropriate for them to do so.
3. Because all paradigms have their shortcomings, scientists are *pragmatic* when they commit to a paradigm: if they were not pragmatic, they would have to give up on any paradigm before having fully explored its explanatory potential.
4. Given 1-3, there is no need to attribute to scientists an uncritical or dogmatic attitude during normal science periods. This averts the problem for the received view identified in the previous section (claim (ii)).

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[7] Bird is not very explicit on this, but the rationale seems to be that non-rule based learning can be *reliable* in discriminating good from bad exemplar solutions (71-75), and therefore it can be rational to rely on such learning. Still, Bird is quite cautious in deeming this view rational: he concludes only that similarity judgments are "compatible" with such judgments being rational, or, as he puts it even more cautiously "at least with their not being classified as irrational" (88).

I will support this argument by spelling out what I take to be the relevant good epistemic and pragmatic reasons and I will also argue that Kuhn himself provided them. Before that, let me hasten to clarify: "better alternatives" in this argument are not to be understood as alternatives that are closer to the truth; Kuhn was no realist. Instead, "better alternatives" are to be understood here purely in terms of alternatives that would allow a higher degree of problem-solving capacity (Kuhn 1962/1996, 153-4 and 169). That measure is no absolute one, as the problem-solving capacity of a paradigm at time t1 may be higher than at t2. For example, Einstein's theory of relativity arguably would have had a lower problem-solving capacity at a time when Newton first proposed his theory in 1667, as many problems Einstein addressed successfully (e.g., the advance of Mercury's perihelion) were not yet known at that time. Even if physicists had had the ability to use Einstein's theory at the time (which they of course did not) Newton's theory would have been simpler and the more obvious choice in the problems physicists did face, such as the calculation of planetary orbits. It is no accident that Newtonian mechanics is still used today to shoot rockets to the moon (although Einstein's theory is required today to solve many other problems).

I will proceed as follows: In section 3.1 I argue that commitment to a paradigm is justified by the early success of the paradigm in explaining a particular domain of the world; there is thus *epistemic justification* for paradigm acceptance and adherence. In section 3.2 I argue that the imperfection of paradigms, for Kuhn, calls for a doubly pragmatic attitude of scientists and that, for Kuhn, scientists have good epistemic reasons not to abandon a paradigm when anomalies occur. There is no need to assign dogmatic or uncritical attitudes to scientists in normal science periods. Section 3.3 gives a brief conclusion.

3.1 Epistemically justified commitment

The commitment scientists make to a paradigm is epistemically justified because there is empirical support for the paradigm and there are no better alternatives. Kuhn argues this himself.

When a scientific field transitions from a pre-paradigm to a paradigm stage, Kuhn argues that a paradigm must be "sufficiently *unprecedented* to attract an enduring group of adherents away from competing modes of scientific activity" (Kuhn 1962/1996, 10; added emphasis). For example, before Franklin in the 1740s was able articulate a theory of electricity that would come to serve as a paradigm for the field, there were many competing theories of electricity that explained some phenomena but had trouble explaining others. Franklin's theory, in contrast, could account "for nearly all of these effects" and it was for *that* reason that practitioners were able to agree on a common paradigm (15). In other words, practitioners had *good epistemic reasons* for the acceptance of Franklin's theory as the paradigm: it accounted for most of the known facts and did so better than its competitors.

Elsewhere Kuhn says that when paradigms first appear, they "gain their status [as paradigms] because *they are more successful than their competitors* in solving a few problems that the group of practitioners has come to recognize as acute" (Kuhn 1962/1996, 23; added emphasis). Again, this suggests that the early success a paradigm enjoys is unique: there are no better alternatives for explaining the problems at hand. Kuhn does go on to note that, when a paradigm is first proposed, it is limited "in both scope and precision" and it is for normal science to actualize "the promise of success" associated with the paradigm (ibid.). Still, the early successes of a paradigm are real and absolutely central in convincing practitioners to adopt the paradigm in to engage in this normal science activity.

Examples of impressive early successes of a paradigm are Darwin's application of the principles of his theory of evolution to the famous Darwin finches, Lavoisier's notion of a balanced chemical reaction (and its applications), Newton's derivation of planetary motion, and Einstein's explanation of the advance of Mercury's perihelion.[8] These achievements of paradigms are described in classic texts (such as Darwin's *Origin of Species* and Newton's *Principia*) and serve to persuade a community to adopt a paradigm. Without these successful applications of the paradigm to the world, a new theory or paradigm "would not even be a candidate for acceptance", as Kuhn put it himself (Kuhn 1962/1996, 46). By laying out solutions to problems, a paradigm proves that it has explanatory power and appeal. And because a paradigm stands out among other alternatives a paradigm gets accepted for its *unique success*, which tends to be both empirical and conceptual.

As a paradigm is developed over time by scientists during normal science, the epistemic support of a paradigm increases: normal science increases the scope and precision of the paradigm by finding more successful applications of the paradigm to the world and by improving the fit of those applications. Scientists adhering to a paradigm have then no reason to be uncritical or dogmatic: they can simply point to the empirical and conceptual support that the paradigm has already achieved and continues to achieve. All of this provides *inductive support* to scientists' expectations that the paradigm will help them solve the puzzles that it sets out for scientists to solve also in the future.

3.2 The imperfection of paradigms: pragmatic scientists and epistemic warrant

It goes without saying that normal science not only produces successes: any given paradigm will run into problems at some point. Perhaps theory and fact cannot be matched, intricate conceptual difficulties arise, or the instruments are not working in the intended manner. Kuhn referred to such difficulties as "anomalies". In this section I argue that scientists have both good *epistemic* and *pragmatic* reasons to adhere to a paradigm when anomalies occur. The received view that invokes dogmatic and uncritical attitudes, in contrast, faces difficult questions.

3.2.1 Doubly pragmatic scientists

Kuhn mentions two pragmatic rationales for why scientists do not give up on a paradigm when it encounters anomalies.

[8] Most of these examples are my own. It is interesting to note that Kuhn does not nearly talk as much about the already successful applications of paradigms as he does about the work that still needs to be carried out in normal science in order to make good on the promise of a paradigm. This may have contributed to a distorted image. One can only speculate as to why Kuhn decided to proceed in this way, but plausibly he did so to emphasize aspects of science which he may have thought were underappreciated.

First, if scientists were to reject a paradigm *whenever* the world resists paradigm assimilation, then "all theories ought to be rejected at all times" (Kuhn 1962/1996, 146), because there are no theories that are in perfect agreement with all of the relevant phenomena. Hence, to get any normal science work done, scientists need to be pragmatic and work with the paradigm that gives them the best chances of solving the largest number of puzzles.

Second, if scientists were to aim at completely resolving any anomaly they encounter before moving on to the next problem, scientists would not be able to put the paradigm to fruitful use in all of those cases in which the world *can* successfully be assimilated to the paradigm. As Kuhn put it, "the scientist who pauses to examine every anomaly he notes will seldom get significant work done" (Kuhn 1962/1996, 82).[9] So scientists need to be pragmatic when facing anomalies and focus on those problems that they *can* solve with the help of the paradigm.

Normal science is thus not dogmatic, but pragmatic, even by Kuhn's own lights: the community ultimately commits to the best available paradigm, in full appreciation that no paradigm is ever in perfect agreement with the phenomena.

3.2.2 Epistemically justified adherence and change

Despite the anomalies a paradigm is bound to encounter, scientists also have excellent *epistemic* reasons to adhere to a paradigm: it has proven its value already when first proposed and it continues to produce successes, despite the anomalies, until crisis hits. As mentioned in section 3.1, a paradigm's previous explanatory and empirical success gives scientists *inductive warrant* to think that the paradigm will be successful in the future in solving problems similar to the past ones. Accordingly, when scientists do not criticize their paradigm then they do *not* do so because they are uncritical, but because their inductively warranted expectation is that the paradigm will ultimately overcome the anomalies it is facing. Scientists are keen to get anomalies addressed, if possible.

For example, when Newtonian physics was adhered to for more than 200 years that was *not* because scientists were uncritical or dogmatic towards it. On the contrary, scientists were well aware of various anomalies, such as the advance of Mercury's perihelion (discovered in 1859) and were desperate to find solutions to it. No solutions were available within the Newtonian framework and scientists were happy to switch to Einsteinian mechanics, once it became available in 1905. *If they were uncritical and dogmatic towards the paradigm, then why would scientists ever want to switch paradigms?*

The same point can be made of other examples of highly resilient anomalies: the stellar parallax shift, which was entailed by Copernicus's solar system, was not observed until 1838, i.e., no less than 295 years after Copernicus first proposed his system (namely, 1543). The Higgs boson, which was predicted in the 1960s, was also only discovered in 2012, i.e., 52 years after its first prediction and after several failed searches that had begun already in the 1980s (Ellis et al. 2012),

[9] Godfrey-Smith (2009) compares normal science to a bomb whose ultimate purpose it is to explode, but not too early, at every 'minor buffet' (83).

none of which resulted in the rejection of the standard model. Examples like these should count as evidence for scientists' dogmatic attitudes, if scientists are ever dogmatic at all. But of course, scientists are usually perfectly aware of a paradigm's weaknesses and open gaps and are keen to find appropriate solutions. In our two examples, scientists were very keen to find evidence for the stellar parallax shift and evidence for the Higgs boson. *If scientists were uncritical or dogmatic about their paradigms, why would they even want to make any efforts to address a paradigm's weaknesses and filling open gaps?*

Kuhn believed that paradigm-based science carries the seeds for its own undoing. Paradigms set expectations for what is "normal" and thus guide scientists in noticing when nature behaves differently. It is therefore that normal science is a "particularly effective way of inducing paradigm change" (ibid., 52). With normal science activity leading to an increase in the precision and scope of the paradigm, normal science contributes to scientists gaining an ever better understanding of the limitations of the paradigm they are working in. At one point, Kuhn even speaks of the "revolutionary efficacy of normal science research puzzles" (Kuhn 1970b, 257). *If scientists were dogmatic and uncritical, it's unclear how this could be the case at all*: how could scientists come to understand that the present paradigm has ceased to guide their puzzle solving endeavors and that the time has come to change the paradigm? This problem is *especially* pressing if an dogmatic and uncritical attitude would entail no openness to revision whatsoever, which is how these attitudes are usually understood as in the philosophical literature (Unger 1975, Roberts and Wood 2007).

3.3 Conclusion

In this section I argued that the received view about normal science is ripe for revision. When paradigms are epistemically justified (as they are, also on Kuhn's view), and no better alternatives are available, then there is no need for scientists to criticize paradigms (with the view of rejecting them). Accordingly, philosophers need not invoke dogmatic or uncritical attitudes for making sense of normal science: scientists can be as critical as they want to be, if the current paradigm is epistemically justified and there are no better alternatives, then they will end up retaining the current paradigm anyway. Furthermore, scientists are pragmatic because all paradigms have shortcomings. Instead of ending up without any paradigm, they would rather accept and retain the one that is better justified than the alternatives. The view proposed here thus fully accounts for how Kuhn claims the scientific community behaves in the face of anomalies. The received view, in contrast, must answer some difficult questions as to how paradigm change would be possible at all.

4 Apparent textual evidence for the received view

There are some passages in *Structure* and beyond that may suggest that – contrary to what I have argued here – the received view is the correct view about normal science. In what follows, I will argue that the first appearances are misleading and there is no textual evidence for the received view in *Structure* or beyond. Sections 4.1 and 4.2 focus on *Structure* and section 4.3 and 4.4 on two of Kuhn's later papers.

4.1 Suppression of alternatives?

In Kuhn's *Structure* there are passages that may suggest that the consideration of alternatives gets actively suppressed in normal science. One may take this as evidence for Kuhn attributing dogmatic and uncritical mindsets to scientists after all.

For example, Kuhn says that "scientists normally [do not] aim to invent new theories, and they are often intolerant of those invented by others" (Kuhn 1962/1996, 24). Kuhn does not explain how he wants this passage to be understood, but, prima facie, it does seem to suggest what I have been arguing against, namely a dogmatic and uncritical mind-set. Note though that the passage is in fact compatible with my view: if scientists are in the possession of a paradigm that is explanatorily and empirically successful, then proposing theories incompatible with the paradigm may be seen as far-fetched and unreasonable, especially when the explanatory and empirical success of the new proposals is dubious. Scientists' reservedness towards such alternatives is therefore just all too understandable.

In support of my reading, one may mention another passage in *Structure*, which appears at the end of Kuhn's chapter on paradigm crisis, and in which Kuhn mentions, similarly to the previous passage, that the "invention of alternates [to a paradigm theory] is just what scientists seldom undertake", except during pre-paradigm stages of science and during revolutions (Kuhn 1962/1996, 76). What follows is crucial for the understanding of Kuhn's view on the non-consideration of alternatives:

> So long as the tools a paradigm 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*. The significance of crises is the indication they provide that an occasion for retooling has arrived. (Kuhn 1962/1996, 76; added emphasis)

In other words, for scientists to change a paradigm, there needs to be a good reason for doing so. And such an *epistemic* reason Kuhn provides himself here: when a paradigm encounters too many anomalies it enters a mode of crisis, in which scientists realize that the explanatory potential of their paradigm has been exhausted and in which they realize that the time has come to look for alternative theories and ways of doing science.

4.2 Education and authority

There is clear evidence that Kuhn attributes uncritical attitudes to scientists *in their education*. For example, Kuhn says that "science students accept theories *on the authority* of teacher and text, *not because of evidence*" (Kuhn 1962/1996, 80; added emphasis). Kuhn also suggests that students would probably be more doubtful about a paradigm if they were taught not just about the paradigm's successes but also about its failures. Perhaps it is passages like these that prompted Popper to speak of "indoctrination" when describing normal science (see section 2.2).

However, none of this can cast any doubt on my argument against the received view. Learning science is hard; it takes time for science students to obtain the ability and knowledge to see eye to eye with their teachers. Once science students have reached a certain degree of maturity, they become capable of making their own informed judgments, and they can decide for themselves whether the paradigm deserves the adherence that it commands in the community. If they end up concluding that a paradigm is the best available alternative, then they would indeed have no reason to abandon the paradigm. But they would not do so because they are uncritical or indoctrinated, but because criticizing the paradigm with a view of abandoning it would amount to withdrawing their own basis for their normal science research.

4.3 The "Essential Tension" paper

Structure is not the only place in which Kuhn talked about the restraints of teaching. In the essay "The Essential Tension: Tradition and Innovation in Scientific Research", Kuhn spoke about "convergent thinking" and "tradition" in science and science education and juxtaposed this to "flexibility and open-mindedness" (Kuhn 1977). At one point, he even speaks about "dogmatic initiation" (229). To illustrate, Kuhn mentions the use of textbooks for both undergraduate and graduate students and that students are instructed to solve problems "very closely related in both method and substance" to problems students encounter in textbooks (229). Not until their doctoral dissertations, are students asked to read research papers or to even "attempt trial research projects" (228). But again, one should not confuse education with the practice of scientists.

When Kuhn finally turns to normal science in this paper, he mentions the familiar functions of normal science of increasing the scope and precision of paradigms and his view that normal science does not aim at novelty (233). Accordingly, he describes the "normal" scientist as "solver of puzzles" rather than "innovator" (234). At the same time, he repeats the familiar argument that it is precisely normal science activity that – through its focused efforts – is most capable of eventually producing "tradition-shattering novelties" (ibid; see also section 3.2.2). And it is for this reason that he speaks of an "essential tension" in the nature of normal science (227).

There is thus no evidence in this paper of Kuhn's that he thought of normal science as dogmatic or uncritical.

4.4 Kuhn's "Function of Dogma" paper and the London colloquium

Intriguingly, in a paper titled "The Function of Dogma in Scientific Research" Kuhn *did* use the term 'dogmatism' on one occasion to describe normal science. He presented that paper in 1961 and the paper was published two years later, just one year after *Structure* (Kuhn 1963). Does this not prove that normal science *can* after all be described as dogmatic? I think not, for three reasons.

First, Kuhn did not use "dogma" in the sense later commentators have: there is no place in the text in which Kuhn would associate dogmatism with scientists being uncritical; and not once does he mention that normal science would require such attitude. What Kuhn does associate most strongly with the term "dogma" is "deep commitment" to a paradigm and the expectation that the puzzles set out by the paradigm are solvable. But as we explained earlier, this expectation can be accounted for epistemically: scientists have inductive support for it. And to illustrate the reasons for commitment, Kuhn uses the example of the history of electricity in the early eighteenth century that we mentioned already in section 3. As already explained, these reasons were epistemic reasons. In this context, it is worth noting that in this paper Kuhn also speaks of scientists "gaining" a paradigm "with difficulty" (302) and of "achieving" a paradigm (307). That is, paradigms are hard won through experimental and theoretical work, not through some fairly arbitrary community decision, as it might be when a community were to *uncritically* adopt a paradigm.

Second, Kuhn does describe education as "dogmatic" in unequivocal terms, but we already commented on that at some length in section 4.1. Fittingly, Kuhn says that students are "neither invited nor *equipped* to evaluate" the problem-solving tradition of a paradigm (304; my italics), whereby the emphasis really should be on the second part of this phrase.

Finally, it is interesting to note that Kuhn himself obviously had regrets about describing normal science as "dogmatic" in the "Function of Dogmatism" paper: not only did he abandon any talk of dogmatism in *Structure* (not even the first draft he circulated contained any mention of it), but he also explicitly forbade the reprinting of his dogmatism paper, as correspondence in the Kuhn archive has revealed (Reisch 2019, 269).[10] Relatedly, Kuhn insisted (in writing both to Popper and Lakatos) that the title of his session in the famous 1965 London colloquium be changed from "Dogma and Revolution in the History of Science" to "Criticism and the Growth of Knowledge" (Gattei 2008, 42, fn 46 and 48). Kuhn felt very strongly indeed about this requested change: he even made it a "condition" for his participation and considered it "essential" for accommodating his contribution to the conference.[11]

Perhaps slightly ironically, in his contribution to the proceedings of the London colloquium, which were published in Lakatos and Musgrave (1970), Kuhn *did* speak of normal science as "abandoning critical discourse" (Kuhn 1970a, 6-7). This is the only place that I am aware of in which Kuhn did so. This remark occurs in a context in which Kuhn contrasts his view against Popper's and where he invites the reader to think of his view regarding the demarcation problem as turning "Sir Karl's view on its head". However, what underlies the contrast between Kuhn's and Popper's view, I believe, is not so much a difference in critical (or dogmatic) attitude, but rather a different attitude towards apparent counterevidence: whereas counterevidence must lead to the rejection of theories on Popper's view, it normally does not on Kuhn's view (see section 6). Instead, apparent counterevidence is treated as anomalous and may – if everything goes well – ultimately be assimilated by the paradigm. As we have argued earlier, this has got nothing to do

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[10] Reisch (2019) claims that the "functional burden" of dogma in his paper in *Structure* was now carried by terms such as "*binding, commitment, rigid, accepts without question, relatively inflexible box, take for granted, assurance,* and *confidence in their paradigm*" (269; original emphasis). Of these terms, "accepts without question" and "take for granted" may seem the most problematic ones for the view that I am defending here, namely that normal science is best read as *not* dogmatic and uncritical. However, the first phrase occurs only once in *Structure* (on p. 47) in the context of education and refers to "particular problem-solutions already achieved". We already discussed the context of education in section 3.1. The second phrase also occurs only once and refers to "the cleavage" between art and science that happened after fine art gave up on representation as a goal (161).

[11] The quotation is from a letter by Kuhn to Popper, dated 30 June 1965 (Popper archive 317/17). See also Kuhn's letter to Lakatos from 23 June 1965 (Popper archive 80/9).

with scientists' being uncritical; their confidence in the paradigm is justified and they have inductive warrant to expect that the paradigm will succeed also in the future.

This interpretation is supported by the subsequent discussion by Kuhn: in the rest of his contribution to the volume (37 pages long, no less) he does not once talk again about scientists being uncritical (or dogmatic). Kuhn also in a footnote refers the reader to pages 10-22 and 87-100 in *Structure*, where he talks about the route to normal science and response to crisis, respectively. In none of these places does Kuhn characterize scientists as uncritical or dogmatic. I therefore think that it is fair to say that this one place in Kuhn's writing where he allowed himself to describe normal science as uncritical owes more to Kuhn's desire to contrast his view to Popper's – who placed so much emphasis on science being critical – than to what he actually wanted to convey with his idea of normal science.

5 Objections

In this section I will consider some possible objections one may have to what I have argued in this paper.[12]

***Objection 1**:* the target of the paper as normal science not being dogmatic or uncritical is merely a verbal dispute. One should not take talk about dogmatism and uncritical attitudes in the received view too literally.

Reply: A verbal dispute is a dispute that is not about a substantial or deep matter; the parties are not really disagreeing, even though they may appear to be, owing to the divergent uses of the relevant portion language (Chalmers 2011, Jenkins 2014). But the dispute about whether normal science is dogmatic and uncritical or whether it presupposes epistemically justified commitment is not of that kind: it is a disagreement of substance, not just of language. Philosophically and practically, it surely makes all the difference whether or not one possesses good reasons for acceptance and adherence or not. In contrast, if one is uncritical or even unreflective, then it is hard to see how the reasons for one's acceptance of a paradigm could be *good* reasons. And Kuhn never denies that there are good reasons for paradigm acceptance. If anything, he thinks there are too many good reasons.

Objection 2: I have argued that scientists have good epistemic reasons for adopting a paradigm: a new paradigm comes with a range of successful "applications" to the world (see section 2.1). But is this claim not undermined by the fact that Kuhn also thought it rational for "holdouts" not to adopt a new paradigm (Kuhn 1962/1996, 151-2)? How can one be epistemically justified in adopting and declining to adopt a new paradigm?

Reply: Although I have here focused on the reasons why scientists may adopt and defend a new paradigm (section 2.1 and 2.3), this does not imply that there are no good reasons to stick to an old paradigm. In particular, if the old paradigm has been successful in the past in solving puzzles, then one may very well be rational in resisting change, because one may still expect the old

[12] The objections are inspired by the critical comments by one of the anonymous reviewers for this journal.

paradigm to continue being successful or to overcome more recent and recalcitrant obstacles (Kuhn 1962/1996, 151-2). All this may be so despite the early success of the new paradigm and its "promise" to provide a better outlook on the world. In other words, what I have said here is fully consistent with what Kuhn said about resistance to paradigm change in a scientific community: good reasons to adopt a new paradigm do not necessarily invalidate the good reasons to keep the previous paradigm, and vice versa.

In spite of this, Kuhn explicitly acknowledges that the new paradigm may solve problems the old paradigm failed to solve and that this may be one of the most effective means of swaying the community (Kuhn 1962/1996, 153). He even says that a new paradigm is especially likely to persuade scientists to adopt it if it "displays a quantitative precision strikingly better than the older competitor" (153-4). However, he also makes clear that practitioners must ultimately be persuaded that a new paradigm in the future will guide them better in their puzzle-solving activity than the predecessor. This decision cannot be algorithmicized (200).

***Objection 3*:** I have argued in section 4.1 that Kuhn's claim that "science students accept theories *on the authority* of teacher and text, *not because of evidence*" (Kuhn 1962/1996, 80, added emphasis) can really be read as what it says, namely as a claim about science students, rather than as a claim about scientists. One may object to this by insisting that one's education exerts a strong influence long into one's professional life and that especially Kuhn thought that a paradigm restrains scientists' perspective they take on the world. Given that restraint, one may think that Popper's description of normal science as "indoctrination" is perhaps appropriate after all, and that scientists – at least on Kuhn's view – "think and act in the way they have been taught".[13]

Reply: I think this objection contains a false dilemma: either one is strongly influenced (and in some way restrained) by one's education and thus be indoctrinated or one is not. But neither horn of the dilemma is true. One may very well be strongly influenced (and restrained) by one's education – I think that is generally so – and still grow critical about what has been taught when the occasion requires it. I do think this view fits what Kuhn had in mind about normal science much better than the (received) view that scientists are indoctrinated thus incapable or unwilling to exercise their critical capacities at all.

6 Normal science and Kuhn's revisionary rational reconstruction

Up until this point I have argued that normal science is not dogmatic or uncritical; instead, it is *epistemically justified* and *pragmatic* in its handling of anomalies. I take all of these characteristics to be fully consistent with rational behavior and conducive to the goal of producing knowledge. Moreover, I take these attitudes to reflect how good scientists *ought to behave* when they want to further this goal. As we will see in this section, Kuhn himself in fact thought along those lines when reflecting about his own philosophy. He even thought – rightly I believe – that his account makes *better* rational sense of scientific practice than other accounts.

[13] This phrase has been suggested to me by a reader of a previous version of this paper.

6.1 Revisionary rational reconstruction

Although it is still not much appreciated, Kuhn saw himself as engaged in making rational sense of science.[14] As he put it: "I am no less concerned with rational reconstruction, with the discovery of essentials than are philosophers of science" (Kuhn 1970b, 236, see also Kuhn 1971). Where Kuhn clearly differed from other philosophers, though, was his willingness to revise apparently rational norms in the face of our knowledge about science. What one might refer to as *revisionary rational reconstruction*, Kuhn described thus: "if history or any other empirical discipline leads us to believe that the development of science depends essentially on behavior that we have previously thought to be irrational, then we should conclude not that science is irrational but that *our notion of rationality needs adjustment here and there*" (Kuhn 1971, 144; added emphasis). This is because "science is the best example of rationality that we have" (Kuhn 1971, 144) and it would therefore "open the door to cloud-cuckoo land" to delude oneself into thinking that it was possible to obtain, what he called, "criteria of rationality" independently of science (Kuhn 1970b, 264). However, Kuhn was not so naïve as to think that "any scientist behaves rationally at all times, or even that many behave rationally very much of the time" (Kuhn 1971, 144). Rather, Kuhn thought that one had to somehow focus on and identify the "essentials of the scientific process" for one's account of science (Kuhn 1970b, 264).

Kuhn not only believed that his own theory of science was in agreement with these essentials that he thought he had extracted through historical analysis, but he also made clear in his *Postscript* that he thought that other accounts fared worse than his own:

> one set of reasons for taking the theory [i.e., Kuhn's theory of science] seriously is that scientists, whose methods have been developed and selected for their success, do in fact behave as the theory says they should. My descriptive generalizations are evidence for the theory precisely because they can also be derived from it, *whereas on other views of the nature of science they constitute anomalous behavior*. (Kuhn 1962/1996, 207-8; added emphasis)

Kuhn did not give any examples of *what* historical facts he had in mind that "we have previously thought to be irrational" and that motivated him to revise "our notion of rationality" (see the earlier quote). But I believe scientists' resistance to abandoning a paradigm, which we discussed in section 3.2, is such an example, and normal science is the concept that Kuhn devised to make sense of it. I also think that Kuhn's account is superior to Popper's in making sense of instances of resistance to abandonment.

6.2 Rational progress over Popper

The core of Popper's falsificationism is simple and straightforward: if the consequences derived from a theory are inconsistent with statements describing observations (such as: "metals gain weight when heated"), then the theory ought to be rejected. Not rejecting a theory in the face of negative evidence, on Popper's view, is irrational and unscientific. Of course, this picture is complicated by the Duhem thesis, which Popper was in fact well aware of. As he said himself, "no

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[14] The introduction to philosophy of science by Godfrey-Smith (2009) stands out as one that attributes normativity to Kuhn's notion of normal science.

conclusive disproof of a theory can ever be produced" (Popper 1959/2005, 28). Still, in the same passage, Popper noted that scientists who choose to wait for (unrealistic) disproof and to defend their theories "dogmatically" despite apparent counterevidence "are adopting the very reverse of that critical attitude which in my view is the proper one for the scientist" (Popper 1959/2005, 28). In light of this, Worrall's comment that "Popper never seems to have fully absorbed the simple lessons of Duhem's analysis" thus seems apt (Worrall 2002, 72). Kuhn himself made this very point about Popper in his *Reflections*: "Having barred conclusive disproof, he has provided no substitute for it, and the relation he does employ remains that of logical falsification. Though he is not a naive falsificationist, Sir Karl may, I suggest, legitimately be treated as one" (Kuhn 1970b, 14).

In the absence of a cogent reply to the Duhem problem by Popper, Kuhn concluded that Popper had provided no "logic of knowledge at all" but an "ideology". Instead of methodological rules, Popper supplied only "procedural maxims", which scientists may only rarely live up to (Kuhn 1970b, 15).[15] Popper was furious about this characterization,[16] but he nevertheless signaled agreement with Kuhn "on some points" and made an odd concession: "I have always stressed the need for some dogmatism: the dogmatic scientist has an important role to play. If we give in to criticism too easily, we shall never find out where the real power of our theories lies" (Popper 1970, 55).[17] Popper immediately interjected that this concession was different from "the kind of dogmatism Kuhn wants", namely "a ruling dogma over considerable periods" (Popper 1970, 55). He then admitted that it is "much easier to discuss puzzles within an accepted common framework", but he rejected the idea that such frameworks themselves "cannot critically be discussed" (Popper 1970, 56).[18]

We have already seen in the previous section that such attributions to Kuhn are mischaracterizations of Kuhn's position. Kuhn never claims that scientists *cannot* be critical towards their paradigms. They can be, but they usually are not, because they adopt a paradigm *for good reason*: the paradigm has demonstrated its value in successfully solving puzzles (which is by no means to be guaranteed for any odd theory and associated practice). Furthermore, the tension between the above quote by Popper and his other views on dogmatism could not be starker: it is precisely the absence of dogmatism and the critical attitudes that Popper centrally associates with science. With his concession Popper thus made zero progress with regard to the Duhem problem

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[15] Lakatos (1970) agreed with Kuhn's criticisms of Popper's falsificationism and sought to accommodate these criticisms with his "sophisticated falsificationism". See Duerr (2023) and Gattei (2008) for the view that Popper never was a naïve falsificationist.

[16] See Gattei's analysis of material from the Popper Archive (Gattei 2008, 46, fn 102).

[17] Still Popper recognized Kuhn's concept of normal science as "Kuhn's main discovery […]. Very important and very new: I certainly did not see it"(cited in Gattei 2008, 49, fn 117).

[18] See also Díez (2007), who has argued that the Kuhn – Popper controversy can be resolved by disambiguating two sense of "theory": Kuhn's diachronic sense (in the form of paradigms), which Díez calls a D-theory, and Popper's "synchronic" sense, an S-theory. Díez argues that not rejecting a paradigm (or D-theory) when it is falsified is not irrational, because "there is no purely logical reason for abandoning one D-theory and starting another" (548). However Díez does not explain why it would be *rational* to keep pursuing a paradigm in the face of falsifiers

and he produced a problematic tension with the rest of his account, especially with his solution to the demarcation problem (see also Worrall (2002) and Lakatos (1968)).[19]

We can conclude with Kuhn that the notion of normal science indeed makes better sense of the phenomenon of resistance to abandonment than falsificationism. Popper's falsificationism instructs scientists to give up their theories when their consequences are inconsistent with statements describing observations and he does not have a good story to tell why scientists would legitimately choose not to do so. It is one of Kuhn's lasting achievements not only to have shown that even the best scientists resist abandonment of their theories, but also why it is rational for scientists to do so.

7 Conclusion

In this paper I argued that the received view about Kuhn's notion of normal science is mistaken: normal science is not dogmatic or uncritical. Kuhn never described normal science in these terms, and where he seemingly did, he consistently referred to the education of science students, not to practicing scientists. Science students must first bring themselves into a position to assess paradigms by undergoing rigid and year-long training. Once they have reached a stage at which they can see eye-to-eye with their teachers, they may still decide to work within the same paradigm: when the work of the scientific community has successfully guided their puzzle-solving activities, and when no better alternatives are available, there is no reason for scientists to criticize paradigms with the view of rejecting them. There is thus no need for philosophers to invoke uncritical or dogmatic attitudes to account for the work of scientists during normal science periods. Instead, commitment to a paradigm in normal science is better described as *epistemically justified* and – given the shortcomings of any paradigm – *pragmatic* relative to alternatives that would do worse. This not only describes Kuhn's own ideas about normal science better than the received view, but it also avoids the kind of questions that proponents of that view see themselves confronted with. Finally, I argued that Kuhn's concept of normal science is a prime example of what I called *revisionary rational reconstruction*, because – contrary to Popper's account in particular – it helps us understand on *epistemic grounds* why scientists are rational when continuing to work in a paradigm in the face of apparent counterevidence. Dogmatic and uncritical attitudes have no place in Kuhn's view, and nor should they.

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[19] Lakatos fittingly called these rare concessions by Popper of scientists' resistance to falsifiers "a reluctant admission of an undigested anomaly in the Popperian research programme" (Lakatos 1968, 167, fn. 55).

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