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WHAT IS KUHN'S PROBLEM?

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Inspired by the work of Kuhn, we might want to develop an account of science that explains how it is that while much of science involves the investigation of a world as articulated by a paradigm, the scientist is nevertheless an observer and rational interpreter of a mind-independent world that does not change its character over time. Kuhn himself recognizes that there is a challenge here that he does not know how to meet. I argue that progress can be made on this challenge by carefully examining and criticizing Kuhn's account of *deliberation* in science. Inspired by certain views about Gestalt psychology and examples such as the duck/rabbit picture, Kuhn takes deliberation in science to be a *consequence* of seeing things a certain way, rather than rational deliberation in science making new ways of seeing things possible. I argue that the most serious problems of Kuhn's view of science stem from this fact, and that we can free ourselves from these problems by not following Kuhn here. In particular, I argue using material from Hanson and Peirce that we should think of the revolutionary scientist as being revolutionary not merely in virtue of seeing things in a new way, but rather for showing – typically through painstaking deliberation – that certain conjectures connected with new ways of seeing the world are *reasonable* (even prior to anything like inductive confirmation.) This makes coming to see the world differently a deliberative process that is importantly *unlike* seeing a rabbit/duck picture differently. Such a way of thinking allows us to view the articulation of a new paradigm as a deliberative process that does *not* take some paradigm or other for granted, but rather as a deliberative process that interrogates existing orthodoxy for its suitability to survive into the next paradigm. The result is a (sketch of a) view of science that maintains much of what is important to Kuhn, but departs from him where his view is least convincing.

Keywords: Kuhn, Peirce, Hanson, paradigm, conjecture, gestalt

В ЧЕМ СОСТОИТ ПРОБЛЕМА КУНА?

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Вдохновленные работой Куна, мы могли бы захотеть разработать описание науки, объясняющее, почему, несмотря на то, что большая часть науки связана с исследованием мира (в том виде, в каком он определен парадигмой), ученый тем не менее является наблюдателем и рациональным интерпретатором независимого от сознания мира, который не меняет своих характеристик с течением времени. Сам Кун признает, что здесь есть вызов, и он не знает, как с ним справиться. Я утверждаю, что можно добиться прогресса в решении этой проблемы путем тщательного изучения и критики куновского описания делиберации в науке. Вдохновленный некоторыми взглядами на гештальт-психологию и такими примерами, как картина утки/кролика, Кун считает, что дискуссия в науке является следствием видения вещей определенным образом, а не рациональным обсуждением, делающим возможными новые способы видения вещей. Я утверждаю, что самые серьезные проблемы, связанные с куновским взглядом на науку, проистекают из этого факта, и что мы можем освободиться от этих проблем, не следуя в этом за Куном. С опорой на идеи Хэнсона и Пирса, я, в частности, утверждаю, что мы должны думать об ученом-революционере как о революционере не только в силу того, что он видит вещи по-новому, но скорее потому, что он показывает — обычно путем тщательных размышлений — что определенные предположения, связанные с новыми способами видения мира разумны (даже до того, как тому получены какие-либо индуктивные подтверждения). Это делает процесс перехода к иному взгляду на мир делиберативным процессом, который существенно отличается от видения картины кролика и утки по-разному. Такой способ мышления позволяет нам рассматривать формулировку новой парадигмы как делиберативный процесс, который не принимает ту или иную парадигму как нечто само собой разумеющееся, а скорее как делиберативный процесс, который ставит под сомнение способность к воспроизводству сложившегося канона в рамках будущей парадигмы. В результате я предлагаю набросок образа науки, который сохраняет многое из того, что важно для Куна, но отходит от него там, где его позиция наименее убедительна.

Ключевые слова: Кун, Пирс, Хэнсон, парадигма, предположение, гештальт

1. Introduction

Of the claims made in *The Structure of Scientific Revolutions* [Kuhn, 1970] (henceforth SSR), Kuhn's claim that there is something non-rational about paradigm changes in science is arguably the most controversial. As I shall discuss in the following sections, this controversial claim is intimately connected with Kuhn's idea that paradigm changes in science are best thought of as Gestalt shifts. It is noteworthy however that even before SSR the role of Gestalt shifts in science had been discussed by Hanson [Hanson, 1958]. Unfortunately, much of the subsequent literature comparing Kuhn and Hanson (e.g., [Kordig, 1971; Sleinis, 1973; Malone, 1978; Tibbets, 1975; Reisch, 1991; Hintikka, 1992; Brewer; Lambert, 2000], and [Estany, 2001]) has focused on their views on the theory-ladenness of observation, generally lumping Kuhn and Hanson together. The contribution of this paper is to argue that this running together of Hanson with Kuhn misses what is most interesting about Hanson, namely, his idea that coming to see something differently in science is a *rational* process. While this idea is somewhat buried in Hanson's work, bringing it out shows a way in which we can avoid being forced to think of scientific paradigm shifts as non-rational in the way Kuhn does. My broader suggestion is that contemporary philosophy of science would benefit from viewing Kuhnian ideas through a Hansonian lens.

2. Two Images

Central to Kuhn's philosophy is a problem he takes himself to be unable to solve. This problem is presented in compressed form in chapter 10 of SSR, when Kuhn notoriously states that '…*though the world does not change with a change of paradigm, the scientist afterward works in a different world*' (p. 121). Kuhn makes this bold claim fully recognizing its paradoxical nature, and I think we fail to understand Kuhn's thought correctly if we do not recognize its paradoxical nature.

The problem is generated by two conflicting images of the scientist. On the first image, the scientist is someone engaged in the project of trying to understand the mind-independent world they find themselves presented with. The scientist carefully observes, measures and collects data on this mind-independent reality, after which they take their task to be that of producing a theory that is in some way appropriately supported by this data. According to this first image, the scientist is then an observer and rational interpreter of a mind-independent world that does not change its character even when the scientist changes their views about the world. This image is familiar and intuitive.

But Kuhn also takes his research to give us a second, quite different image of the scientist. According to this second image, the scientist always observes the world in a particular way, informed by the paradigm in which they work. This is true of all scientific observation, measurement, and data-collection. Presented with streaks in a cloud chamber, the modern scientist cannot help but *see* this as the effect of a stream of electrons. Looking at a sunrise, the Copernican astronomer *sees* the effects of the earth revolving around a stationary sun, while the Ptolemaic astronomer *sees* the sun literally rising over a stationary earth. On this second image of the scientist, although the job of science is the progressively more detailed articulation of the world given to the scientist through observation, what is given in observation – what the scientist 'sees' – depends on the paradigm of the scientist. Because paradigms change over time, the scientist is then an observer and rational interpreter of a paradigm-dependent world that *does* change its character when the scientist changes their most basic beliefs.

On this second image, something like the move from Ptolemaic to Copernican astronomy should *not* be understood as the result of objectively recognizing that some sort of paradigm-independent evidence supports the Copernican view and refutes the Ptolemaic view. Instead, according to this image we must first see the world through heliocentrist eyes to become convinced that the data before us is poorly explained by Ptolemaic geocentrism, and much better explained by Copernican heliocentrism. According to Kuhn, this process of coming to see the world through Copernican eyes requires a type of 'gestalt switch', the *result* of which is that we come to recognize the geocentric view as untenable. We are thus left with a picture of science according to which the scientist's beliefs are (their best attempts at) rational responses to what is given in observation, though what is given in observation depends on the paradigm they occupy, which may change over time.

These two images of the scientist are at odds with one another. Is the scientist engaged in the project of rationally interpreting a paradigm-independent world, or rationally interpreting a paradigm-dependent world? In SSR, I think Kuhn wants us to feel the strong appeal of both answers. He knows this leaves us with a problem, and thinks that an '*alternate to the traditional epistemological paradigm*' (SSR, p. 121) must be found in order to resolve this tension. While he is clear that '*the traditional [Cartesian] paradigm is somehow askew*' (SSR, p. 121), he is also clear that no alternative to this paradigm has yet been produced, and that SSR does nothing more than suggest a few broad features this paradigm must have. SSR thus leaves us with two conflicting images of the scientist, a recognition that they are conflicting, and the call for a resolution with a new paradigm that Kuhn himself does not provide. For the duration of this paper, let us call the question of how to reconcile these two conflicting images of the scientist *Kuhn's problem*.

It is perhaps tempting to think that in SSR Kuhn is outright rejecting the first image of the scientist and telling us to endorse the second. One might even support this by drawing from Kuhn's later writing. In [Conant, Haugeland 2000, p. 264] for example, Kuhn states that '*I go around explaining my own position saying I am a Kantian with moveable categories*'. This seems to suggest that Kuhn knows precisely what sort of non-Cartesian paradigm is needed to resolve his problem – namely, a neo-Kantian one. Such a neo-Kantian view would be one according to which in order for the world to be the sort of thing that is objectively real, it must be structured by the basic concepts that appear in our scientific paradigm, which now play a role similar to the categories of thought in Kant's theoretical philosophy. On this view, the objective world really does change when our basic scientific paradigm changes and new categories of thought are adopted. Indeed, in his discussion of Taylor in [Ibid., p. 220] Kuhn doubles down on this view, re-iterating that '*the heavens of the Greeks were irreducibly different from ours*.' This suggests that Kuhn's ultimate view is that the first image of the scientist offered above is simply wrong, and the second is right.

There may well then be reason to think that what I have described as Kuhn's problem is not a problem for the later Kuhn. But I am not convinced that this is how the Kuhn of SSR sees things, and in this respect it is the Kuhn of SSR in which I am interested. If when writing SSR Kuhn thought that neo-Kantianism (which had already been in vogue in philosophy of science decades before SSR was written) was sufficient to resolve his problem, surely he would have written chapter 10 of SSR very differently. And so although SSR is largely devoted to articulating the second image of the scientist, in SSR Kuhn does *not* take this to amount to an argument for a neo-Kantian view of science; or at least, he does not present it that way. Rather, in SSR Kuhn sees himself as articulating the second image of the scientist described earlier, and noting how it conflicts with the otherwise compelling first image of the scientist given earlier. This leaves Kuhn with a problem that he admits he cannot resolve. Kuhn *might* have later come to think that he could solve this problem, but that will not be the focus of this paper. The focus of this paper will rather be on Kuhn's problem as he sees it in SSR, and which I think is a genuinely compelling problem for philosophy of science. Perhaps contrary to the later Kuhn, I think that abandoning the first image of the scientist and rushing headlong into neo-Kantianism fails to do justice to the intuitions behind the problem that Kuhn himself carefully sets up in SSR. The focus of this paper will therefore be on articulating ways of thinking that take both images of the scientist as seriously as possible. I argue that a plausible direction in which to try to find a solution to Kuhn's problem involves bringing on board some non-Kuhnian ideas, but this should not be understood as rejecting the largely Kuhnian framework in which what I say should be understood.

3. Coming to See Things Differently

Kuhn is right that in science what is *typically* given to us is already structured by a paradigm, and therefore in part a product of how we 'see' the world. In this way, much of science takes place in a world that changes when our most basic beliefs change. But I will argue that it is not correct to say that all science has this character.

The focus of this paper will be on the process by which we make a transition from one paradigm to the next. I think that Kuhn is right to characterize this process as one in which we 'come to see things differently'. However, I am not convinced that this process is at all similar to the way in which we come to see things differently in the kinds of duck/rabbit picture cases on which Kuhn dwells. In the duck/rabbit case, the transition from seeing the picture as a duck to seeing it as a rabbit typically happens suddenly and definitively, and does not involve deliberation (let alone painstaking deliberation) of any sort. We simply see the picture as a rabbit at one moment, then as a duck the next. We typically do not engage in deliberation about what we are seeing, and then come to see things accordingly. Rather, we see the picture as a duck or a rabbit first, and *then* reason about the fine points of what we are seeing. Perhaps Kuhn's attachment to the duck/rabbit picture comes from the view that creativity in science involves some sort of sudden and largely unanalysable act of seeing things differently, though we will not pursue this matter here. (For a good defense of this claim about Kuhn as well as some comparisons with Wittgenstein, see [Kindi, 2021].)

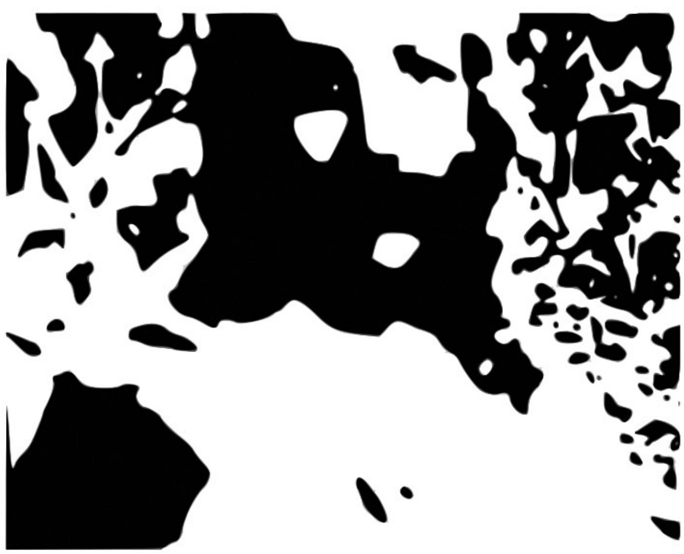
Inspired by this way of thinking of the duck/rabbit picture, Kuhn sees deliberation in science as the *result* of seeing things a certain way, rather than its cause. In Kuhnian science, there is no neutral space in which we rationally deliberate about how to see the world, and then come to see things accordingly. This is because on the Kuhnian picture, rational deliberation on any scientific matter *presupposes* acceptance of a paradigm. Thus for Kuhn, there is no rational scientific process of inquiry that takes us from one paradigm to the next. This is stated forcefully in Chapter XII of SSR, when Kuhn tells us that '*the transition between competing paradigms cannot be made a step at a time, forced by logic and neutral experience. Like the gestalt switch, it must occur all at once (though not necessarily in an instant) or not at all*.' (SSR, p. 150). So even though certain concepts must be developed and set in place in order to change paradigms (and in this sense, the process of changing paradigms extends over time), at any moment of scientific deliberation the scientist occupies one paradigm or another. There is no intermediate space in which the scientist stands at a critical distance from any paradigm, and nevertheless manages to conduct genuine scientific inquiry. This much is central to Kuhn's picture of science.

Viewed in fast-motion, a change of scientific paradigm might indeed look something like a sudden gestalt switch. But slowing things down and looking more carefully, the comparison with duck/rabbit pictures becomes much less compelling. The construction of a new scientific paradigm – or new way of 'seeing' things – is instead a process of extensive deliberation and critical examination of prior assumptions, in which the revolutionary scientist finds themselves unclear about what they are seeing at all, and slowly comes to see new ways of looking at the data as not just possible but *reasonable*. To put it differently, the revolutionary scientist does not simply see things in a new way, but rather through painstaking deliberation works through their confusion about what they are seeing, arriving at a new way of seeing things that, in virtue of this deliberation, they are entitled to call reasonable. One of the main tasks of this paper is to urge this point of view, and argue that it may be used to free us from some of the problematic strains of thought that trap us in Kuhn's problem.

The kind of process I have in mind by which new ways of seeing emerge in science has to some extent already been articulated by Hanson and to a lesser extent by Pierce, though the way in which the work of these thinkers paints a non-Kuhnian picture of science has not been widely appreciated, and therefore requires some elucidation. Hanson's book [Hanson, 1958] (which actually appeared before SSR), is particularly relevant. Most well-known in this book is Hanson's description of Kepler coming to reject Aristotelian conceptions of motion and coming to see that the planets orbit the sun in elliptical paths. Also noteworthy is Hanson's description in [Hanson, 1969] of Harvey coming to see that the heart pumps blood rapidly and basically evenly around the whole body, contrary to the dominant views of the time. In each case, Kepler or Harvey felt an inability of the science of the time to adequately accommodate the data (not unlike Kuhn's talk of 'crisis'). If we must use metaphors of sight, we should say that faced with such a situation, Kepler and Harvey did not continue to simply 'see' things in accordance with the existing paradigms of their times, nor yet in accordance with some new paradigm. Rather, Kepler and Harvey found themselves unable to see things clearly at all – it was as if a fog covered things so that they did not know *what* they were seeing any more when they contemplated their respective phenomena. In an attempt to regain their lost vision, what followed in each case was a deliberative process in which a series of conjectures or hypotheses were individually examined, and the extent to which such hypotheses allowed a restoration of explanatory order in the data before them was critically assessed. To focus on the case of Kepler, Kepler first carefully considered the hypothesis that the orbit of Mars might really be circular, and noted intractable problems with it. He then considered various hypotheses according to which the orbit of Mars might be one of various ovoid shapes, and noted deep problems with each of them. After much moving back and forth between new and old hypotheses, Kepler was led to the hypothesis that the orbit of Mars might be elliptical, which after much deliberation he came to see as a hypothesis of great explanatory virtue. Hanson describes in great detail how '*[Brahe's] observations were given, and they set the problem – they were Johannes Kepler's starting point. He struggled back from these, first to one hypothesis, then to another, then to another, and ultimately to the hypothesis of the elliptical orbit.*' [Hanson, 1958, p. 72]. After examining at length and in painstaking detail the question of whether the elliptical hypothesis was really plausible, '*the enormous heap of calculations, velocities, positions and distances which had set Kepler his problem now pulled together into a geometrically intelligible pattern*.' [Ibid., p. 83]. Hanson describes this as a process in which through great effort Kepler slowly not only came to see the data before him clearly again, but to see it in a new way altogether.

Most noteworthy in Hanson's account is the rationality of this whole process. By the time Kepler was happy with the elliptical hypothesis, it was to him much more than a wild guess. It had become a guess that his masses of painstaking calculations over a period of years had led him to see as entirely *reasonable*. Kepler seemed to first have the idea of elliptical orbits quite early, but rejected it on various grounds, favoring other hypotheses that at the time seemed (perhaps completely reasonably) more worthy of attention. The accomplishment of Kepler was not in merely having the idea of elliptical orbits – that idea could have occurred to anyone with sufficient mathematical education, and presumably did – but rather in coming to see this idea as *reasonable*. Coming to see such an idea as reasonable is not something that happens in a single moment of insight, but is rather a deliberative process extending over a great deal of time, requiring much effort. In the case of Kepler, this process involved several distinct parts. First, Kepler had to be convinced that simpler hypotheses ultimately left him with no clear vision of what he was seeing. Next, Kepler had to come to see how the elliptical hypothesis succeeded where these simpler hypotheses failed in creating a new and clear way of seeing the mass of data and calculations before him. The second part is impossible without the first. It would not have been enough for Kepler to begin with the elliptical hypothesis. Part of Kepler seeing the order that the elliptical hypothesis created was fully recognizing the disorder that other hypotheses left him with. In this way, a rational trajectory was created that *led* Kepler to see the world differently through the eyes of the elliptical hypothesis. It is not as if Kepler simply and suddenly saw a rabbit – rather, his prolonged investigations gave him *good reason* for conjecturing that there could be nothing other than a rabbit before him.

We must then be careful in not taking too literally various simple visual metaphors for the process that led Kepler to the elliptical hypothesis. Part of what made Kepler's elliptical hypothesis a rational one is the way in which he was led to it by studying the failures of rival hypotheses. This is entirely lost in analogies with the duck/rabbit gestalt switch. A better comparison might be the example Hanson gives of coming to see the image of Christ in the blur below:



For some, seeing the image here is a struggle. It was for me at first. Being told that there was an image of Christ here but unable to make sense of the patches of black and white before me, I first wondered whether some part might be an eye, some other part a nose, and even wondered whether a prank had been played and that it was really just a montage of random splotches. I moved between making conjectures (perhaps this is the left eye), abandoning conjectures (but then there is nothing that could plausibly be the nose, so that must be wrong), and making new conjectures (so maybe this other bit is the eye and that other bit is a nose), until the image of Christ emerged. If one just focuses on the final accomplishment of seeing, it is easy to lose sight of the way in which the clarity that is eventually achieved is the result of a process of *deliberation.* In this process of deliberation, there is something reasonable at various moments about taking something to be a nose and something else to be an eye. Imagine after much effort and many false starts saying to oneself, '*Surely then this must be the eye…*' A claim like this, even if ultimately abandoned, is something that can be reasonable in virtue of the deliberative process that leads up to it.

Focusing on simple duck/rabbit examples, the idea of the *reasonableness* of a new way of seeing things is impossible to see. In the duck/rabbit example, we typically simply see things one way or another without effort. It is thus easy to be tempted into the view that seeing things in a new way is a kind of non-rational process. This temptation is very hard to resist if we take simple visual analogies too seriously, and therein lies their danger. Kuhn, for example, was unable to resist this temptation.

If we feel compelled to use a visual analogy to capture the process of scientific discovery, I think we would have to consider a picture vastly more complicated even than the image of Christ above and so great in size that we can only see a small area at a time. It would have to be the case that in trying to make visual sense of this massively complex image, our most intensive deliberations give us clarity on only a small corner of the picture and perhaps a rough area surrounding it, and in which we eventually find ourselves also able to make sense of these and perhaps a few other small regions of the picture. It would also have to be the case that our conviction that our way of making visual sense of this image is reasonable is tied up with the failure of other ways of making visual sense of what lies before us. None of the usual examples of gestalt switches given by Kuhn have anything like this character, and I doubt there is a reasonably simple example that does. (There are even limitations with the image of Christ example, though I think it is better than Kuhn's examples.) This is not to deny that there is a genuine gestalt switch going on in scientific revolutions. My point is simply that we must not hastily identify these gestalt switches too closely with the visual gestalt switches of simple examples in which rational deliberation is entirely absent or minimal.

A further point must be stressed here. I have described Kepler's accomplishment as that of making the elliptical hypothesis *reasonable*. This is different from confirming the elliptical hypothesis, or even having justified belief in the elliptical hypothesis. Kepler's hypothesis still required empirical confirmation after it was advanced. Prior to obtaining such empirical confirmation, it was simply a reasonable conjecture. Indeed, it may even have ended up not being empirically confirmed, Kepler's deep convictions notwithstanding.

To say that Kepler merely produced a reasonable conjecture is however no sort of criticism. [Popper, 1959] famously argued that there were no logical or epistemic distinctions to be drawn between conjectures that go beyond the evidence – the only distinctions that exist are purely psychological. In Popper's eyes, there might be interesting or exciting conjectures – where this terminology is to be understood purely psychologically – but not *rational* or *insightful* conjectures in any substantive epistemic sense. Even though philosophers have largely rejected Popper's overall way of thinking of science, there is still today much lingering attachment to this Popperian attitude towards conjectures. As a result, one might be tempted to think that there is nothing intrinsically reasonable about Kepler's elliptical hypothesis – it is just a brilliant guess, and language of 'reasonableness' or even 'insight' is entirely misplaced. But this Popperian view about conjectures is I think entirely incorrect. As a corrective, it is useful to recall the work of Pierce, who divided scientific inquiry into an abductive process in which a hypothesis was generated, followed by an inductive process in which the various consequences of the hypothesis were empirically confirmed. Importantly, both the inductive and abductive processes of inquiry were rational for Pierce. (This view Pierce emphasized over many decades, but see especially [Pierce, 1955] and [Pierce, 1998].) Although it is not entirely easy to pin down Pierce on this matter, rational conjectures are rational in virtue of the way they provide more satisfying, powerful, and verifiable explanations of the phenomena that strike us as calling out for explanation. Nevertheless, no matter how reasonable a conjecture may be, it still requires empirical verification in order for the relevant scientific inquiry to be complete. The accomplishment of Kepler that Hanson brings to our attention is thus the production of an extraordinarily insightful and *rational* conjecture that created a new way of seeing the data and calculations that Kepler found before him.

Acknowledging that conjectures themselves are the results of a process of deliberation is helpful in freeing ourselves from some of the unattractive strictures of the Kuhnian viewpoint. If we ignore the idea that the formation of new scientific ways of seeing and the associated gestalt switches are the results of rational processes, we will then be tempted to think that what happens in a scientific revolution is non-rational. This leads to an uncomfortable picture of science for those who want to hold science as something like a paragon of rational inquiry. The unattractive view of scientific revolutions as non-rational can I think be traced back to the Popperian conception of conjecturing in science as a non-rational process, which ironically Kuhn was not able to liberate himself from, despite his otherwise trenchant criticisms of Popper.

4. Returning to Kuhn's Puzzle

Let us then return to Kuhn's puzzle. This puzzle revolves around the question of whether the scientist is a rational interpreter of a paradigm-dependent world or a paradigm-independent world. I think Kuhn is right to suggest that once a paradigm is fully articulated it structures the activity of much subsequent science in such a way that it takes place within a paradigm-dependent world. But I also think that the process of inquiry by which new paradigms are created is not an activity occurring in a paradigm-dependent world, and should instead be understood as the activity of rationally interpreting a paradigm-independent or even mind-independent world.

One objection to the idea that the process of creating new paradigms is somehow paradigm-independent is that this process typically involves the acceptance of much of the previous paradigm. Kepler, after all, continued to embrace much of the physics of his time in the deliberations that led him to the elliptical hypothesis. Harvey too did not completely reject everything that was believed at the time about the circulation of the blood around the body. The kinds of arguments each of these scientists offered that made their new ways of seeing the world reasonable explicitly presupposed much of the previous paradigm. Indeed, it is difficult to see how they could have proceeded in any other way. And so it might be thought that even in the deliberations that lead scientists to see things in radically new ways, scientists are still guided by significant fragments of the paradigms of the time in such a way that what they are doing should be understood as taking place in a paradigm-dependent world.

But we must be careful here. It may well be true that in any scientific revolution, a significant fragment of the previous paradigm is carried over into the new paradigm. (Let us put aside Kuhnian questions as to whether the meaning of relevant terminology is nevertheless transformed.) At the start of a revolution, however, it is impossible to say which fragment that will be. When Kepler and Harvey were no longer sure what they were seeing and were trying to make sense of the world before them, no principle was sacred. Had their deliberations demanded it, they might have ended up abandoning different aspects of the paradigm of the time. It is only after the fact that we can point to the part of the paradigm that survived. We must not think of the surviving part of the paradigm as having had some privileged status all along that made its survival inevitable.

Let us consider this point some more. If deep in a cave in a remote forest some bizarre physical phenomenon was found in which matter appeared and disappeared in an inexplicable manner, we would at first try to explain it in a way that required us to abandon as little of our present science as possible. If this failed, we might consider progressively more radical departures from present science. In the right circumstances, we might even go so far as regarding the phenomenon as evidence that we are living in a computer simulation and that reality as we know it is an illusion. There is no way of knowing in advance how far we will have to go in rendering the phenomenon in question intelligible. In our deliberations we would constantly be trying to gauge the extent to which certain fragments of our existing paradigm could be maintained while rendering the phenomenon in question intelligible. A moment of speculative deliberation in which some fragment of our existing paradigm remains intact is not, however, a moment in which that fragment remains uncritically accepted and endorsed as some sort of a-priori fact. Rather, in such speculative deliberations one of the things we are interrogating is precisely whether we really can continue to accept and endorse the fragment of our existing paradigm in question. Presumably at some level we are open to discovering that we cannot. The fragment of the paradigm in question, far from being endorsed, is being interrogated for its suitability to survive into the next paradigm. When a detective speculates about what might have happened had a particular suspect really been innocent, their attitude towards the claim that the suspect is innocent is *not* one of endorsement. It is rather the attitude of provisionally accepting a hypothesis for the sake of seeing whether it leads to a compelling way of seeing the evidence before them. Only if the hypothesis that the suspect is innocent turns out to be fruitful in the right ways will the detective end up accepting it as a genuinely reasonable hypothesis. Even this, of course, is less than acceptance in the sense of full belief (which is in turn still less than justified belief.) To what extent this sort of thing happens even in Kuhn's *normal* science is a question worth pursuing, though I will not do so here.

Hanson's description of Kepler paints a picture of someone methodically *testing* (and not merely uncritically accepting) old ways of thinking to help lead them to a new way of seeing the mass of data and calculations before them. Insofar as this process of deliberation involves continuing to embrace a certain conception of matter and motion, it does so with critical distance, involving a willingness to abandon old ways of thinking should that be the only way to form a coherent image of the data before them. This process of challenging old assumptions and constructing new yet reasonable 'visions' of the data cannot therefore be described as paradigm-dependent in Kuhn's sense, as in any paradigm-dependent inquiry, the paradigm itself must be taken as beyond question. This is not what we find when we look at the way in which Kepler and Harvey built new paradigms out of the confusion and visual chaos in which they found themselves.

These considerations help salvage the idea of the scientist as an observer and rational interpreter of a mind-independent world that does not change its character even when the scientist changes their views. Ways of seeing the world may change over time, but Kepler and Harvey are faced with the problem of a world that they do not know how to see anymore. The process of inquiry they then find themselves engaged in cannot be described as paradigm-dependent. Arguing that human thought genuinely reaches a stable world beyond the mind touches on some of the most perennial questions of philosophy, and it is not the job of this paper to somehow settle these issues. Suffice it to say that Kuhnian considerations of the paradigm-dependence of much of scientific activity do not provide effective arguments against the view of science as a project of rationally interpreting a world outside our minds.

This then helps to dissolve Kuhn's puzzle. Even if in much 'normal science' we find ourselves interpreting a paradigm-dependent world, the moments in which we interrogate and perhaps even revise our scientific paradigms should not be understood in this way. In this way, both images of the scientist presented at the beginning of the paper correctly describe a certain sort of scientific activity, though neither correctly describes all aspects of scientific activity on its own.

5. Conclusions

Kuhn's articulation of the notion of a scientific paradigm and his defense of the idea that much science should be understood as the observation and interpretation of a paradigm-dependent world is a profound contribution to the philosophy of science. I nevertheless think it should be separated from other Kuhnian claims about paradigms that are much less plausible, and that rest on an inaccurate picture of the generation of hypotheses and conjectures in science. Rather than thinking of a change of paradigm as a sudden gestalt shift, we should think instead of coming to see the world differently as a protracted and rational struggle in which new foundational conjectures come to be seen as *reasonable*. This creates space for a kind of rational scientific inquiry that cannot be described as the straightforward observation and interpretation of a paradigm-dependent world. I think something like this has been the reaction of many philosophers of science to the Kuhnian position. A main claim of this paper is that this reaction is best understood as rooted in Piercian/Hansonian intuitions about the nature of scientific discovery. Because of this, I suggest that many critics of Kuhn are really Piercians or Hansonians without knowing it.

One way forward in the philosophy of science is then to flesh out these Piercian/Hansonian intuitions more carefully. What makes a conjecture reasonable? Although Pierce took seriously the idea that a conjecture could be reasonable, he ultimately did not give a satisfactory account of how this is possible. How in more detail do conjectures allow us to see the world differently? Although Hanson provides many compelling historical sketches, he fails to give the kind of detailed philosophical account of this that we might like. How should we understand the role of creativity in science, if not via the duck/rabbit image metaphor? To generate an account of the methodology and epistemology of science that does justice to Kuhn's most important ideas while avoiding the traps into which he fell, these are the sorts of questions we must address.

A final topic worthy of mention is that of scientific realism. A traditional challenge to scientific realism is the pessimistic meta-induction, and this has generated an enormous literature. There is also the question of whether Kuhnian considerations generate a quite distinct challenge to scientific realism. Kuhn himself spoke in highly ambiguous terms on this issue, and there is much division in the literature as to what impact the work of Kuhn has on scientific realism. A good deal of this debate is summarized nicely in [Wray, 2021]. Giere [2013] argues that Kuhn's position is compatible with realism, albeit so-called 'perspectival realism'. Massimi [2015] questions whether Giere's perspectival realist reading of Kuhn is genuinely realist, and proposes her own realist interpretation of Kuhn in terms of 'naturalized Kantian kinds'. Sankey [2018] rejects such realist readings of Kuhn, claiming that '*Kuhn's account of science affords little scope for a realist conception of truth.*' [Sankey, 2018, p. 77]. The question of whether SSR poses new challenges for realism is thus a vexed one. My view is that a more Piercian/Hansonian way of articulating some of Kuhn's main ideas allows us to resist the idea that Kuhnian considerations require us to either abandon or mutilate the doctrine of scientific realism. The question of whether there are other grounds for worrying about scientific realism remains, but that is not the topic of this paper.

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