Rational Insight and The Apriori in Science.

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Abstract

In this paper I argue for the rationalist idea that scientific reasoning sometimes involves a substantive apriori component. I focus on cases of scientific discovery of the sort presented in Kuhn and Hanson, in which scientific discovery involves something like a Gestalt switch. I argue that in such cases certain substantive realizations associated with Gestalt switches are best understood as entirely rational in character. The argument involves reconceiving Hansonian and Kuhnian Gestalt switches as a special type of purely rational process in which one comes to see the existence of a justified piece of reasoning from the data to a given conclusion. This also gives a different and less mysterious way of thinking of Gestalt switches in science.

Keywords: Rationalism, Apriori, Scientific Inference, Justification, Hanson, Kuhn.

1 Introduction

The idea that the acquisition of scientific knowledge might sometimes rest on apriori considerations or rational insight in any really substantive way has fallen out of fashion in modern philosophy of science. Even if, following a modern rationalist such as Bonjour (2010), it is conceded that something like rational insight is needed to recognize the truth of propositions such as 'nothing can be red all over and green all over at the same time', philosophers of science seem to have largely given up on the idea that apriori considerations or rational insight can also generate substantive knowledge of genuine interest to real scientists.

The rejection of any substantive role for apriori considerations in science is perhaps partially due to the dominance of an old-fashioned but still widely held image of science, according to which our scientific knowledge is constructed from observation data in a somewhat mechanical way using rules of inference such as induction or inference to the best explanation (henceforth IBE). On this sort of image of science, beyond the performing of fairly trivial bookkeeping tasks that are necessarily involved in any mechanical rule-governed practice, there does not appear (at least on first glance) to be much room for apriori considerations or the use of rational insight. Alternatively, on a subjective Bayesian picture in which science is nothing other than the updating of one's brute credences in accordance with Bayes' rule, there seems to be even less room for apriori considerations or rational insight to play any sort of non-trivial role in the acquisition of scientific knowledge.

In this paper, I would like to argue that contrary to these images of science, apriori considerations and rational insight do indeed play a non-trivial and substantive role in the generation of scientific knowledge.

Before arguing for this, there are several things I must say in advance to temper the reader's expectations. First, I will not be claiming that there are special assertions of science that are known *entirely* apriori. Rather, I will only be arguing that there are scientific assertions such that apriori considerations and rational insight are *involved* in coming to believe them with justification. The justification of such assertions will therefore only have an apriori component. Empirical considerations will also be involved in the justification of these assertions. So I will not be trying to argue that we come to believe Einstein's relativity principle or the basic posits of quantum mechanics in a wholly apriori manner, but rather only partly so. Second, my focus is going to be on apriori justification and justified belief, rather than apriori knowledge. So stated more carefully, my claim will be that the *justification* of scientific belief sometimes contains a non-trivial apriori component, or to put it differently, a non-trivial application of rational insight.

In §2 I discuss the way of thinking about non-deductive inference on which my argument relies, and in §3, I discuss the notion of the apriori and rational insight that I will be using. At this point I will be able to outline the kind of role that I take apriori considerations and rational insight to play in science. In §4 I look at some examples, arguing that Hansonian and Kuhnian cases of coming to see things differently in science are best seen as examples of rational insight. In §5–6 I make some further remarks and draw some conclusions.

2 Non-Deductive Inference

In this section, I make some points about non-deductive inference on which the rest of the paper will rely.

Upon conducting a detailed study of several thousand patients, we might conclude that we have an argument that a certain drug is effective against a certain type of diabetes. By an argument, I simply mean a sequence of considerations that shows that if we are justified in believing certain premises, then we are also justified in believing a certain conclusion. In the case of the drug study our argument might spell out a plausible mechanism by which the drug in question seems to work, then point out the evidence that the drug does indeed work in this way, and then finally point out how in a controlled study the drug actually appeared to reduce symptoms of the type of diabetes in question in a large sample of patients. This argument relies on premises such as that the human body functions in the way we ordinarily take it to, that our data is reliable, and so on. So long as this sequence of considerations conforms with the usual standards of good medical science, it will presumably count as an argument, but if makes some sort of mistake and thus does not justify belief in its conclusion, it will not count as an argument by our terminology.

In the case of the drug study, even the best possible argument will almost certainly not be a deductive one, but rather a non-deductive one. Perhaps for instance it will be an inductive argument, an inference to the best explanation (IBE), some combination thereof, or perhaps even something else.

On a certain way of thinking about non-deductive inference (that I will go on to reject), one might expect there to be something trivial about recognizing that something with the shape of a piece of non-deductive reasoning is indeed an argument. For it might be thought that if what we have is really an argument, it can be broken down into a sequence of successful inductions or inferences to the best explanation (IBEs) that ultimately justify belief in the hypothesis in question. Now in the case of *deductive* inference, there is something trivial about checking that something is a genuine argument - one simply checks that in every purported application of modus ponens, the premises have the form $A \to B$ and A and the conclusion has the form B, and similarly for any other rules used in the argument. One might expect that once the rules of induction and IBE are spelt out correctly, we will be largely in the same situation. For presumably to spell out the rule of induction or IBE adequately is to articulate all the conditions associated with these rules in such a way that these conditions can be verified more or less straightforwardly whenever these rules are correctly invoked. (In the case of induction, perhaps one condition is that the evidence is sufficiently diverse according to some reasonably precise standard, for example.) If this is right, then no great creativity or insight will be involved in recognizing that that something with the shape of a piece of non-deductive reasoning is indeed an argument. For it simply suffices to verify that each of the applications of the rule of induction or IBE contained in the argument in question are indeed successful, and it is reasonable to think that such a verification will be straightforward if we really do have enough evidence to justify belief in the effectiveness of the drug via the argument in question.

Of course, just as in the case of deductive logic, *constructing* an argument might require creativity and insight. But once one has an argument, one might expect that verifying that it is an argument will be entirely straightforward, requiring only basic and largely trivial uses of our rational capacities.

While there is something tempting about this picture of non-deductive inference, I do not think it is correct. It is surely relevant to note that in spite of great efforts, philosophers not have yet managed to formulate general versions of induction or IBE whose associated lists of conditions can be straightforwardly verified in typical situations. (For a synoptic discussion of problems with various formulations of induction and IBE, see Norton (2021)[1].) Instead, when scientists give non-deductive arguments, there is typically no obvious and uncontroversial way to rewrite their arguments as a sequence of precise but general rules whose applicability is trivially obvious. (I think this is true of the kind of argument sketched above in the case of the drug study, for example.) The problem here is of course not with the scientists. Insofar as there is a problem, it lies with a kind of philosophical impulse to model our understanding of induction or IBE too closely on our understanding of rules like modus ponens.

I would like to suggest that when it comes to non-deductive arguments, recognizing that something is indeed a genuine argument is a very different sort of thing from recognizing of a successful application of modus ponens that it is indeed a successful application of modus ponens. While recognizing that something is a genuine application of modus ponens (or a genuine deductive argument) is an entirely mechanical procedure that can be outsourced to a simple computer, recognizing that a putative non-deductive argument is indeed a genuine argument is a much more substantive act.

Now, whether we are talking about the highly trivial case of a single modus ponens or the more non-trivial case of our argument for the efficacy of our drug, I claim that it is with the faculty of reason (that is, with the use of our rational capacities) that we determine whether something is a genuine argument. Reasoning, after all, is just the process of assembling beliefs and hypothesis into arguments. This presupposes an ability to recognize of an argument that it is in argument; for without such an ability, it is not clear that reasoning is possible at all. Thus I claim that it is with the faculty of reason that we determine whether something is a genuine argument.

This claim might sound innocent enough, but the claim I want to make is that it is *purely* with the use of reason that we determine whether something is a genuine argument. This claim is perhaps more controversial. For instance, a good inductive argument for the efficacy of a diabetes drug based on a typical trial might call for thousands of patients, but in the right situation in astrophysics a much smaller number of data points might suffice for a convincing argument for a given hypothesis. Whether a non-deductive argument has enough data of the right sort to be persuasive thus depends on the field, and so looks like it is not the sort of thing that can be determined by purely rational considerations. The ability to distinguish genuine arguments from non-arguments in a given field instead requires deep experience with the field in question. On such a view, experience must then be involved in the judgement of a putative non-deductive argument that it is indeed a genuine argument.

However, I think this argument is misleading. Consider a case in which an argument for the efficacy of a diabetes medication depends on some fact F that can only be known by experience. Perhaps the fact F is that to account for variations in the population, diabetes drug studies must generally examine thousands of patients in order to lead to reliable results. Consider then the following argument A:

[1]While I agree with Norton's claim that existing ways of formulating a general rule of induction or IBE do not work, I do not agree with Norton that local principles do better.

> A carefully conducted double-blind study involving 2000 patients showed that drug X significantly reduced the symptoms of diabetes. Therefore diabetes drug X is likely to significantly reduce the symptoms of diabetes in the general population.

This argument does not really represent the full reasoning for its conclusion. The fact F that in diabetes drug studies one must generally examine thousands of patients in order for the results to be reliable is presumably something known to us on the basis of evidence E. Thus a fuller representation of our reasoning is something like the following argument A':

> On the basis of evidence E, it appears that diabetes drug studies must generally involve thousands of patients in order to yield reliable conclusions. A carefully conducted double-blind study involving 2000 patients showed that drug X significantly reduced the symptoms of diabetes. Therefore diabetes drug X is likely to significantly reduce the symptoms of diabetes in the general population.

This argument, rather than implicitly assuming fact F, shows how F follows (non-deductively) from the evidence that we already have, and then proceeds from there. More generally, insofar as non-deductive arguments depend on field-dependent facts that are known on the basis of certain evidence, these field-dependent facts along with the evidence supporting them can be inserted into an argument to show its full structure. Thus one ends up with a (schematic) argument of the following form A'':

> On the basis of all available evidence E (including (1.) the details of a carefully conducted double-blind study involving 2000 patients showing that drug X significantly reduced the symptoms of diabetes, (2.) general evidence pertaining to the nature of the human body and the discipline of medicine, and so on), such-and-such an argument shows that diabetes drug X is likely to significantly reduce the symptoms of diabetes in the general population.

In practice when everyone in a discipline knows many of the facts cited in this sort of argument it is pedantic to keep mentioning them, and so they are typically omitted and arguments like A rather than A' or A'' tend to be written down. But it is an error to think that arguments like A then show that deciding whether a non-deductive argument is good or not depends on empirical knowledge, such as knowledge of F. This is because A is really just an abbreviation for a fuller set of considerations A' or A'' which does not contain F as a kind of tacit premise the way that A does.

I take this to dispel the worry that it is only with empirical knowledge that we can recognize ourselves to have an argument that our diabetes drug X is likely to significantly reduce the symptom of diabetes in the general population. I claim instead that when a set of non-deductive considerations for a conclusion is fully fleshed out, recognizing that what one has is a genuine argument is something that is done purely with the use of our rational capacities. (Note that something at least related to the view I am advancing here has been defended in chapter 7 of Bonjour (2010).)

I have claimed (and will go on to argue in more detail) that it is in general a non-trivial, substantive thing to recognize of a non-deductive argument that it is indeed an argument. It thus follows that substantive uses of our purely rational faculties are required in recognizing of a non-deductive argument that it is indeed an argument. In this way, I claim that substantive uses of reason are involved in the acquisition of scientific knowledge. In the following sections, I will argue that it is in precisely this way that substantive apriori considerations and rational insight end up playing a non-trivial role in the generation of real scientific knowledge.

One might imagine at this point the quite separate objection that unless we have unambiguous, mechanically decidable criteria with which we can distinguish arguments from non-arguments, any faculty that distinguishes arguments from non-arguments is simply mysterious. *How* does reason distinguish arguments from non-arguments if not by unambiguous, mechanically decidable criteria? To this, two things need to be said. First of all, it seems close to a brute fact that we simply do find ourselves able, albeit with varying reliability, to distinguish arguments from non-arguments without precise, mechanically decidable criteria directing us how to do so. This capacity includes some sense of what to do when faced with doubts, uncertainties or disagreements about any particular putative argument. It is a capacity which is fallible but can also be cultivated, and as a result we think of some people as experts when it comes to distinguishing arguments from non-arguments, at least in certain domains. In this sense it seems no more mysterious than many other unremarkable skills that humans possess, like understanding the unspoken messages in our friends' words or deciding the best way to make certain sorts of practical decisions in the face of uncertainty and risk.

Second of all, I think that there is something question-begging about this objection. In particular, this objection simply assumes that any capacity to distinguish arguments from non-arguments must be reducible in some way or other to our ability to perform purely mechanical tasks. In this way, it simply presupposes the incoherence of the idea that our rational capacities could allow us to discern arguments from non-arguments in even a rough way without unambiguous, mechanically decidable criteria for doing so. We need not, however, go along with this. The idea that there is something mysterious about a brute faculty or ability to distinguish arguments from non-arguments rests on a kind of closedness to the idea that reason could ever gift us with even unperfected forms of such abilities. As we start to take the idea that reason gives us such capacities more and more seriously, the sense of mystery in question disappears. I thus think that in these ways the objection in question can be resisted.

3 The Defeasible Apriori.

In this section, I make some remarks about rational insight and the apriori.

Through much of the history of philosophy, apriori justification has been regarded as infallible or unrevisable. However, a growing number of contemporary thinkers have resisted this understanding of the apriori. A more recent notion that separates the apriori from this old understanding is that of an *apriori defeasibly justified belief*. For our purposes, this idea emerges most clearly in the work of Plantinga and Burge (Plantinga, 1993; Burge, 1995, 1997, 1998, 2003), though (Bigelow, 1992; Bonjour, 1993; Katz, 1998) are also relevant and important.[2]

To explain this notion, I begin by giving my working definition of apriori justification:

> An apriori justification for believing some ϕ is a purely rational justification for believing ϕ.

This definition of the apriori puts the focus on rationality rather than independence from experience. Still, I take it to largely coincide in extension with many traditional definitions of the apriori. I acknowledge that the question of what counts as a purely rational justification is a difficult one, though I do not attempt to directly tackle this question here. I am willing to conceive the notion of purely rational justification quite broadly, so that it includes what mathematicians might do when they use their memories, blackboards, diagrams, notes, computers, and so on, though nothing in the present paper will really hang on this. Although there are questions that can be raised about this working definition of apriori justification, I take it to be clear enough for present purposes.

The reader is presumably familiar with the phenomenon of defeat in epistemology. Although a belief is justified, further considerations can sometimes arise that defeat (in one way or another) the original justification for believing it. For example, I might be justified on perceptual grounds in thinking that an object before me is green. Upon learning that the lighting conditions are unusual, the perceptual grounds I have for believing that the object is green might be defeated, with the result that I am no longer justified in believing that the object is green.

On the conception of the apriori I wish to consider, this sort of defeat is possible even when the justification for believing something is apriori (that is to say, purely rational.) For example (Burge, 1997) argues that I am apriori defeasibly justified in believing what I seemingly remember. Being justified in believing what I seemingly remember need not be grounded in empirical beliefs about the reliability of my memory, and does not require knowledge about the statistics of how reliable my memory is. For Burge, believing what I seemingly remember is rather a default position I am justified to take on purely rational grounds. Nevertheless, it is compatible with

[2]It would be remiss not to mention the fact that other philosophers have cast suspicion on the notion of apriori justification more broadly (Hawthorne, 2007; Quine, 1951; Williamson, 2007).

this that empirical considerations can give me reason to doubt any of my memories. (I might have conflicting evidence, or realize that particularly old memories are not reliable, for example.) Thus the apriori justification I have for believing any seeming memory is defeasible. Apriori justification therefore in no way entails any sort of infallibility or unrevisability.[3]

For a different example, suppose that I carefully do a complex multiplication problem with pen and paper using the traditional method, and obtain a particular result. I am thus justified in believing a claim of the form $x \times y = z$ for some particular values of x, y and z. This justification invokes nothing other than my rational capacities, and so is an apriori justification; that is to say, as a result of carefully performing this calculation I am apriori justified in believing $x \times y = z$. However, this justification is defeasible – I might later spot a mistake, or get a different result when I check the result using a calculator. Because I have arrived at the result $x \times y = z$ purely by using my rational capacities I have an apriori justification for believing that $x \times y = z$, but this apriori justification is clearly defeasible.

A similar example can be constructed with arguments rather than calculations. Suppose that using only my rational capacities I carefully construct what I take to be an argument that some complex sentence X is a logical truth. I then have an apriori justification for believing that X is a logical truth. This too is defeasible, as I may have made an error that someone could point out. In this case, in spite of my apriori warranted belief that I have an argument that X is a logical truth, I in fact do not have such an argument at all.

It must be stressed that in spite of their defeasible status, the grounds we have for relying on our rational capacities are different from the grounds we have for relying on things like thermometers. If I see a thermometer with the reading 30°C on it, I am not even defeasibly justified in believing that the surrounding temperature is 30°C, unless I have empirical grounds for regarding thermometers as reliable. However, if an exercise of my rational capacities gives me a claim of the form $x \times y = z$, then I am justified in believing this claim; not because of empirical grounds I have for trusting my rational capacities, but rather merely because I have arrived at this claim through an exercise of my rational capacities. We revisit this point in the final section of the paper.

Finally, by rational insight I will not try to capture the very broad sense in which this term has been used by others. Instead, I will just mean the (defeasible) recognition that something is an argument (i.e., the recognition that a sequence of considerations succeeds in justifying belief in a conclusion given justified belief in its premises) through the use of only rational capacities. Unlike others, I will not take sides on whether rational insight involves some sort of 'immediate' grasping of a truth. I am quite happy to say that coming to see that there are an infinitude of primes based on the usual mathematical argument is an act of rational insight, even though there is

[3]Burge remarks for example that *'we could be apriori entitled to false beliefs'* (Burge, 1995), p. 285.

probably nothing particularly immediate about this.

Although there is much more that could be said about the apriori, this much will suffice for our purposes. Combining the considerations of this section with those of the previous section, my claim is that because it is purely with the use of reason that we recognize that a putative non-deductive argument is indeed an argument, the fact that something is a non-deductive argument is something that can be known apriori, though defeasibly. It is through this sort of defeasible rational insight that we recognize that some putative non-deductive argument is indeed an argument. My claim will be that such applications of rational insight are substantive, and that it is in this way that substantive rational insight comes to play an indispensable role in non-trivial science.

4 Scientific Discovery and Gestalt Switches.

4.1 Gestalt Switches in Science.

Thus far, the reader might feel that in spite of my insistence to the contrary they have been given no real reason to think that the rational insight involved in coming to see that a non-deductive argument is indeed an argument is ever substantive in any interesting way. The goal of this section is thus to present cases in which there is indeed something substantive about this sort of rational insight. This will involve re-appropriating examples from Hanson and Kuhn and arguing that, perhaps contrary to how Kuhn and Hanson understood these examples, they are cases in which substantive rational insight reveals that some sort of non-deductive argument is indeed justified.

Both Hanson and Kuhn (Hanson, 1958, 1969; Kuhn, 1962) recognize that scientific discovery sometimes begins with a scientist feeling confused about what exactly lies before them. The scientist senses that there is an underlying order in what they see, but finds themselves at a loss to describe this order. As a result, the scientist lacks any sort of firm basis on which they can make predictions about how the system before them will evolve, or explain features of the system that seem to call out for explanation. However when scientific inquiry goes well, what sometimes happens is that the scientist notices something about the data before them that suddenly reveals a type of order in what previously looked chaotic and unordered. On this basis, the scientist finds themselves able to make reasonable predictions about how the system will evolve, and finds themselves able to explain features of the system that previously defied explanation. Importantly, all this can at least sometimes happen without acquiring any new empirical data, but merely by scrutinizing the data the scientist already has before them.

Hanson and Kuhn both thought that at least sometimes what happens in these cases is that reflection on the data induces a kind of *Gestalt switch*, revealing to the scientist an order that they had not noticed before. As an analogy, the figure below at first looks like a collection of random patches, and as such it is difficult to make any predictions about what lies outside the frame, or to explain anything about what lies within. However, careful examination of this collection of patches leads to a Gestalt switch in which an image of Jesus appears. Once we have noticed this new Gestalt, we find ourselves able to make reasonable predictions about what lies outside the frame and to explain features of this data. For example, it suddenly becomes reasonable to expect that if we look below the picture, we will see a torso and legs, perhaps enclosed in some sort of cloak. Kuhn and Hanson took progress in science to sometimes be the result of similar Gestalt switches that occur when reflecting on the data.

When discussing such examples within science, Hanson repeatedly used the language of 'visual aspects' that 'dawn' on the scientist. Hanson's idea was that in such moments, something literally becomes visible to the scientist that was not visible before, and the world thus comes to visually appear differently to the scientist. Kuhn too used visual language repeatedly, also using the idea of a visual Gestalt switch to famously describe what happens in a scientific revolution.[4]

While it is highly suggestive to describe certain moments in science with the visual language of Gestalt switches, it also brings with it many puzzles. Consider for example the famous rabbit-duck Gestalt switch of which Kuhn was very fond. It is not as if seeing the rabbit presents itself as the right way of seeing things. We rather think with some indifference of the rabbit-duck picture as something that presents itself as a rabbit at some times and something that presents itself as a duck at other

[4]In discussing the Gestalt switch involved in Kepler recognizing that the orbit of Mars is elliptical, Hanson says *'the affinities between seeing the hidden man in a cluster of dots and seeing the Martian ellipse in a cluster of data are profound.'* (p. 86). Kuhn says *'... during revolutions scientists see new and different things when looking with familiar instruments in places they have looked before. It is rather as if the professional community had been suddenly transported to another planet where familiar objects are seen in a different light and are joined by unfamiliar ones as well.'* (p. 111) Kuhn goes on to say *'what were ducks in the scientist's world before the revolution are rabbits afterward. The man who perceives them differently is responding to a different world.'* (p. 111-112), and *'therefore, at times of revolution, when the normal-scientific tradition changes, the scientist's perception of his environment must be re-educated – in some familiar situations he must learn to see a new gestalt.'* (p. 112)

times. Scientists do not however regard their new ways of looking at the world as mere options in this sort of way. Instead, when things go well the scientist who sees the world in a new way subsequently finds it either impossible or irrational to see things otherwise. (The Jesus figure Gestalt switch fares slightly better than the rabbit-duck in this respect, but how much so is not clear.) Another important difference is that in the rabbit-duck Gestalt switch, whether one sees a rabbit or a duck does not seem to be connected in any straightforward way with the exercise of our rational capacities. We simply see the rabbit and then the duck without any sort of explicitly rational process explaining the change. (Again, the Jesus figure Gestalt switch fares somewhat better here, but again how much so is not clear.) The worry that looms is that we are moving towards a non-rational conception of scientific change.

At this point one could defend Hanson and Kuhn by saying that talk of Gestalt switches is to be understood metaphorically. There is textual room for this in both Hanson and Kuhn.[5] But if talk of Gestalt switches is a metaphor, what is it a metaphor for? If we can get a grip on this, we can perhaps make whatever point it is that needs to be made about the relevant episodes in the history of science with less reliance on metaphors.

4.2 Kepler and Harvey.

In order to think more closely about all this, it will help to consider a case study. In his masterwork *Patterns of Discovery* (Hanson, 1958), one of the main examples Hanson gives of a Gestalt switch in science is Kepler's discovery that the orbit of Mars is elliptical. It will be useful to spell out this example in some detail. In order to understand the motion of Mars, Kepler worked primarily with data obtained by Tycho. Kepler initially tried in vain to fit this data to the hypothesis that the orbit of Mars was circular (perhaps with epicycles), with little success. He also tried to fit the data to the hypothesis that the orbit of Mars was a sort of ovoid. Kepler moved between these and other hypotheses over a number of years, even occasionally considering the elliptical hypothesis. However, even the accuracy of the elliptical hypothesis does not immediately jump out of the data. (It is not as if Kepler's discovery was simply a case of plotting some points and noticing an elliptical shape.) Furthermore, the idea that nature could choose an elliptical orbit also seemed weird and unmotivated. For these reasons, the elliptical hypothesis did not initially strike Kepler as especially promising.

At an early point in Kepler's investigations, all available hypotheses were thus problematic in various serious ways. When Kepler reflected on the data and his mass of inconclusive calculations, he was not sure what he was looking at. Among all available failed explanations, the elliptical hypothesis could not even be said to be the 'best explanation' in any clear sense.

[5]For example, at critical moments Hanson changes his focus and talks about *conceptual* Gestalt switches, as opposed to visual Gestalt switches.

However, as the result of a period of years of effort scrutinizing and struggling with the data, Kepler began to see the data differently. This is documented in some detail in his magnum opus *Astronomia Nova* (Kepler, 2015). Even though Newton had not yet formulated his law of universal gravitation, Kepler was convinced that the sun was in some sense responsible for the motion of the planets. Kepler took there to be a connection similar in some ways to a magnetic connection between the sun and the planets that explained their motion. The simplest idea would of course be that such a connection produced circular motion in the planets, but the data stubbornly refused to fit this hypothesis, continually deviating from circular motion. Kepler's struggle was largely one of understanding the pattern behind the ways in which the orbit of Mars deviated from circularity. He was convinced that there was some sort of 'reciprocation' (or 'libration') in action, such that deviations in the radius of the orbit of Mars had to be explained by proportional deviations in some other quantity that stood in some sort of 'reciprocal' relationship with the radius. The main obstacle here was that the deviations from circularity appeared chaotic and unordered. Eventually however Kepler came to recognize that '*the planet's body reciprocates according to the measure of the versed sine of the eccentric anomaly.*' (Kepler, 2015), p. 558. This at first seemed ad hoc, but Kepler came to see that precisely such reciprocation would be possible if the sun and Mars interacted in something like '*the manner of the steelyard or lever*' (Kepler, 2015), p. 378. Thus Kepler had a mechanism explaining precisely the sort of reciprocation he now saw in the data.[6] Kepler then realized that this sort of reciprocated motion produced several possible orbits including the ellipse, with the actual data clearly showing that in the case of Mars it was the elliptical orbit that nature had chosen. Kepler thus inferred the elliptical hypothesis.

It is important to emphasize that throughout this process Kepler did not acquire new observational data. All he had was the same data from Tycho with which he had been working for years. He simply came to see this data differently. Coming to see the correct relationship of reciprocation in the data was something Kepler described in his own words as a '*revelation*' (Kepler, 2015), (p. 576). Elsewhere he describes this realization by saying '*it was as if I were awakened from sleep to see a new light*' (Kepler, 2015), p. 543. It is presumably this language of revelation and awakening which leads Hanson to suggest that Kepler underwent a type of Gestalt switch in his investigations.

It is not exactly clear at which moment of Kepler's protracted process of discovery we should say that a Gestalt switch has occurred. Nor is it clear that there needs to be a unique such moment – perhaps there are several Gestalt switches happening

[6]It may be asked whether Kepler thought he had actually identified the true mechanism connecting the sun with the motion of Mars, or whether Kepler merely took himself to have shown that there are natural ways in which nature can produce the reciprocal relationship in question, without committing himself to any precise hypothesis as to how the sun determines the motion of the planets. Many historians seem to prefer something like the second option; see (Martens, 1999; Tonnessen, 2010) for discussions of this issue.

in sequence. Hanson does not express a firm opinion on this in his discussions of Kepler. For our purposes, it will suffice to speak somewhat loosely and say that Kepler recognizing the correct relationship of reciprocation and recognizing how it led to the elliptical hypothesis involved a Gestalt switch for him. Loosely speaking, it is in this process that Kepler moved from not knowing what he was looking at, to seeing a profound order that allowed him to make predictions and provide explanations about important aspects of the orbit of Mars.

Hanson elsewhere (Hanson, 1969) describes in similar ways Harvey's discovery that blood circulates evenly around the body with the heart acting as the pump. Harvey is initially faced with a mass of facts about blood and the body that do not appear to add up to any coherent picture. In this way, Harvey too finds himself not knowing what lies before him, and finds himself unable to make predictions or provide explanations about the motion of blood in the body. But as a result of what Hanson sees as a Gestalt switch, Harvey begins to see the data before him differently, eventually becoming convinced that the strange set of facts before him is nothing other than an expression of the fact that the blood circulates evenly around the body with the heart acting as a pump. He then becomes justified in making all sorts of predictions and providing all sorts of explanations about the motion of blood around the body that previously would not have been justified.

4.3 Gestalt Switches Revisited.

Rather than viewing Kepler's Gestalt switch as some kind of *je ne sais quoi* in which the visual world suddenly appeared differently to him, I think it is both more accurate and helpful to view it as a moment in which Kepler realized that what he had before him was an *argument* for the elliptical hypothesis. What Kepler had before him was of course not just the raw astronomical data, but many pages of calculations in which he had (largely inconclusively) tried to get various hypothesis to fit the data. The failure of so many attempts to get the circular or ovoid hypotheses to work; the fact that the relationship of reciprocation which comes from a simple mechanistic hypothesis yielded not circular or ovoid motion but elliptical motion – these facts together (perhaps along with others) Kepler was to suddenly realize gave an *argument* for the elliptical hypothesis that he had dismissed for so long. And so although at first looking at the the mass of chaotic data and inconclusive calculations before him Kepler found himself in a confused state of having no idea what he was really looking at, the realization that what he had before him was actually an argument for the elliptical hypothesis was to transform the disorder before him into an intelligible whole. This was Kepler's Gestalt switch.

I do not deny that undergoing such a Gestalt switch involves seeing things differently in some sense. However, what seeing things differently in such cases amounts to is *recognizing arguments* for certain conclusions where one previously only saw tangled morasses of observations and inconclusive considerations. Once the strength of these newly noticed arguments is grasped, it becomes impossible to ignore them. This applies equally in the case of Harvey, Einstein, or any of Hanson and Kuhn's main examples. For example, it was in recognizing that in the confusing mass of data before him lay an argument for the hypothesis that blood flows evenly around the body that Harvey underwent his Gestalt switch. Likewise, it was in Einstein's recognizing that in the confusing mass of data before him – including such things as the failure of the Michelson Morley experiment, the phenomenon of stellar aberration, Fizeau's experiments, and the Lorentz invariance of Maxwell's equations – there actually lay an argument for the relativity principle, that Einstein too underwent his Gestalt switch.

Kepler noticing that what he had before him was an argument for the elliptical hypothesis was not simply a case of him literally seeing something new in his visual field. It involved a significant move from thinking about planetary orbits in geometrical terms that by default privileged circular orbits, to thinking about planetary orbits in terms of physical causes and mechanisms. Kepler's methods also put weight on numerical considerations in a way that earlier science had not. The result of all this was a relationship of reciprocation quite unusual in form for astronomy at the time, which then led Kepler to the elliptical hypothesis. In at least these ways, Kepler's work embodied an entirely different way of thinking about both the orbit of Mars in particular and the methods of planetary astronomy in general. Recognizing that what Kepler had before him was an argument for the elliptical hypothesis was thus a substantive and highly non-trivial accomplishment.

The rabbit-duck Gestalt switch is quite different in critical ways from the main examples of Gestalt switches in science given by Kuhn and Hanson. The rabbit-duck Gestalt switch is not rooted in the recognition of the strength of an argument which once grasped becomes impossible to ignore. (Again, how much better the Gestalt switch in the picture of Jesus fares here is unclear.) Taken as rough analogies for the kind of process of discovery I have described, things like the rabbit-duck Gestalt switch can be instructive, but taken too literally they lead to a misleading picture of science. My main positive suggestion is that a better way to understand Gestalt switches in science is as something that occurs in certain cases when a scientist comes to see an argument for a conclusion that was not visible to them before.

4.4 Rational Insight in Science.

Kepler's coming to recognize the strength of the non-deductive argument he had for the elliptical hypothesis was not something mechanical or trivial. It was a substantive accomplishment that required the abandoning of old ways of thinking and the embracing of new ways of thinking about planetary motion. It illustrates well how recognizing the strength of a non-deductive argument is not a purely mechanical process. Because it is with the use of our rational capacities that we recognize the strength of an argument, it was only via a substantive use of his rational capacities –

that is, by a substantive act of rational insight – that Kepler arrived at the elliptical hypothesis.

Kepler's recognition of the strength of his argument for the elliptical hypothesis is of course defeasible. Even without acquiring new evidence, it may have been successfully critiqued. But this is entirely compatible with it being the result of a rational process, and thus apriori in character.

This of course does not mean that Kepler's belief that the orbit of Mars is elliptical is *purely* apriori in character. The recognition that from the data before him and everything he is justified in believing about astronomy there is an argument for the elliptical hypothesis *is* indeed purely apriori in character. But the data itself (which includes Tycho's measurements, among other things) had been given to Kepler empirically and thus was not itself the product of reason. My main claim is that in addition to having an empirical component, Kepler's justification for believing the elliptical hypothesis also had an indispensable and substantive purely rational component, insofar as Kepler had to recognize that what he had before him was indeed an argument for the elliptical hypothesis.

I now make three supplementary remarks about rational insight in the context of cases such as Kepler. First of all, I would like to suggest that the kinds of acts of rational insight I have described here involve an important type of *understanding* in science. A problem however with making this sort of claim is that the word understanding is used in many different ways in contemporary philosophy of science. For example, when understanding is discussed in works such as (Lipton, 1991; Khalifa, 2017; Trout, 2007; de Regt et al., 2009), although each author has slightly different things in mind, they all think of understanding as a type of knowledge or grasp of something like the *true* cause or *true* explanation for some phenomenon.[7] In this way, they all think of understanding as *factive* in one way or another.

I have no objections to this, but wish instead to carve out a different notion. I am interested in a notion of scientific understanding that centers around recognizing strong arguments for a given conclusion. Presumably Hippocrates defeasibly noticed such strong arguments for believing the theory of the four humors. Nevertheless, the theory of the four humors is false and thus not the object of knowledge. Additionally, not everything Harvey came to believe about the circulation of the blood was true. So the sort of understanding I am interested in need not be connected with true explanations or true causes or knowledge in any way. For this reason I continue to use the term 'rational insight' to describe it, rather than understanding. But the reader should think of it as a type of understanding, though different from other types of understanding discussed in the literature.

Second of all, writers on scientific understanding often stress that understanding should not be identified with a mere strong feeling of conviction. The same point applies to rational insight. A strong feeling of conviction is certainly present in the

[7]For example, (Trout, 2007) says 'scientific understanding is the state produced, and only produced, by grasping a true explanation'.

examples I gave, and indeed, it is hard to imagine it being absent. Nevertheless, rational insight requires the defeasible recognition of an argument for a given conclusion. It is thus quite different from a hunch or mere intuition that some hypothesis is correct.

Lastly, the claim that Kepler's coming to believe the elliptical hypothesis is an act of rational insight should not be misconstrued as a point merely about the context of discovery. Insofar as another scientist at the time could have become convinced by Kepler, it would be because in reading Kepler's texts, they too had come to notice what Kepler had noticed – namely, that there is an argument for the elliptical hypothesis in the data before them. In this way, they too would undergo the Gestalt switch, and arrive at justified belief in the elliptical hypothesis.

Of course, the modern student of physics comes to believe the elliptical hypothesis in a different way from Kepler. Their coming to believe the elliptical hypothesis typically does not involve them having to shake off medieval views about the nature of motion. In addition, the modern student simply accepts a great deal of information about planetary motion testimonially. The modern student's justification for believing the elliptical hypothesis is thus quite different from that of Kepler. I think it too involves a substantive recognition of the strength of an argument, though presumably a different sort of argument from that which persuaded Kepler. A full discussion of this would involve a much closer examination of the role of testimony and expertise in science, so we do not pursue this. For present purposes it suffices to focus attention on the claim that *Kepler* coming to believe the elliptical hypothesis involved a substantive act of rational insight.

5 More Remarks on Non-Deductive Inference

I have claimed that Kepler saw the data before him as constituting an argument for the elliptical hypothesis. But I have deliberately avoided speculating on what sort of argument this was. Was it an inductive argument that Kepler saw in his data? An IBE? Some mixture thereof?

It is less obvious than it might at first seem how to understand Kepler's argument as a mixture of inductions or IBEs in a way that stays faithful to Kepler's text. Perhaps the most natural temptation is to try to think of his overall argument as an IBE. However, reading Kepler's text, one is struck by the fact that Kepler does not seem to be taking himself to justified in inferring the elliptical hypothesis in virtue of the lack of good competitors. Kepler's overall project is not that of tallying and weighing the strengths and weakness of some larger set of hypotheses. Kepler rather seems convinced that the observational data *directly* justifies belief in the elliptical hypothesis on its own merits, quite independently of the fact that it has no serious competitors. Of course, Kepler is inferring a hypothesis that explains the data, and the hypothesis that he is inferring is presumably the best one. But to be an IBE it must the the case that the elliptical hypothesis being the best explanation of some set of candidate hypotheses is what justifies belief in it. It is far from clear that this is how Kepler sees things.

It is a widespread belief that just as any valid argument in deductive logic can be decomposed into a series of applications of basic rules (such as model ponens, universal instantiation and so on), so too any good non-deductive argument can also be decomposed into a series of applications of basic rules (such as induction, IBE and so on). If this holds true in general, then it must hold true of Kepler's argument for the elliptical hypothesis. Thus if we take Kepler's argument to be good, we are obligated to make some judgment as to precisely how this argument can be decomposed into a series of inductions and IBEs.

But it is important to realize that this overall view of non-deductive inference is a piece of philosophical doctrine that we are under no obligation to accept, and for which there it is not even clear that there is particularly strong evidence. To say that Kepler recognizes an argument for the elliptical hypothesis in the data before him does not require that we take any sides on the question of whether he notices a specific sequence of inductions and IBEs, or even to take sides on whether there is such a sequence of inductions and IBEs that may be taken as a precisification of his argument.

Indeed, it is not even obvious whether it is right to think of Kepler's argument as a larger structure consisting of smaller individual steps such that the justified nature of the smaller individual steps renders the larger argument justified. Is it obvious that we are justified in accepting Kepler's relation of reciprocation independently of the rest of Kepler's argument? Might it not be that it is only by participating in a larger justified argument for the elliptical hypothesis that such smaller steps are justified? I will not try to settle this here. My point is just that nothing that has been said thus far requires us to take a stance on these difficult questions.

The idea (to which I am opposed) that there is something non-substantive or trivial about Kepler seeing the data before him as constituting an argument for the elliptical hypothesis requires that we answer the questions posed in this section in very specific ways. It requires that we see Kepler's argument as composed of individually justified parts, with these parts having specific forms (perhaps inductions or IBEs) whose associated conditions can be seen to hold in some sort of mechanical or trivial way. In sum, it requires us thinking that non-deductive inference is very much like deductive inference in certain specific ways. But all of these assumptions can be challenged. I would in fact suggest that it is far from clear that *any* of these assumptions are right. A very detailed discussion of the issues cannot be pursued here. My goal is simply to note that the more we start to distance ourselves from this very particular (and I think, misleading) picture of non-deductive inference, the more room we create for substantive acts of rational insight in recognizing the goodness of non-deductive arguments.

6 Conclusions

Let us make clear the sense in which the claims of this paper express a modest kind of rationalism. Recalling that an argument has been defined as a sequence of considerations that justifies belief in some hypothesis (assuming justified belief in some set of premises), we have the following:

Principle: When someone through the exercise of their rational capacities comes to believe that there is an argument for a given hypothesis, they are then defeasibly apriori justified in inferring the conclusion of this argument from its premises.

This principle expresses a kind of special right we have to trust our rational faculties, and in this sense, it expresses a modest type of rationalism. More specifically, it tells us that we are defeasibly apriori justified in taking a seeming argument to be an actual argument. It is in virtue of this principle that Kepler is justified in inferring the elliptical hypothesis, for example.

This principle stands opposed to an extreme sort of empiricism according to which we only have a right to trust our rational capacities insofar as we have some sort of empirical grounds for taking them to be reliable. And so for example it stands opposed to the view that we can only trust arguments by induction or IBE if we have empirical grounds to think that these argument forms are reliable. Instead, this argument tells us that we have a default, defeasible right to infer from its premises the conclusion of an induction, IBE, or any other sort of seeming argument delivered to us by our rational capacities, for the simple reason that our rational capacities have given us what appears to be an argument (be it an inductive argument, an IBE, or something else) for inferring that conclusion from the premises in question.

Arguments are therefore not like thermometers. Only empirical considerations can give us grounds for trusting a thermometer, but an argument gives us defeasible grounds for believing its conclusion quite independently of what we do or do not know about the empirical reliability of the argument form in question.

One might worry that our principle gives flat-earthers grounds for believing that the earth is flat. Do not flat-earthers, after all, take their rational capacities to give them arguments for distrusting mainstream science, and arguments for accepting the view that the earth is flat? In reply to this worry, note that not anything counts as coming to believe that there is a good argument for a hypothesis through the use of one's rational capacities. Many people deeply convinced of eccentric views are such that we cannot describe them as even seemingly having arguments taking them from the observational data to their eccentric views.

Additionally, it must be emphasized that even if they do have seeming arguments for their eccentric hypotheses, the justified beliefs they have in their eccentric hypotheses are still defeasible. Insofar as there are anything like actual arguments for the flat-earth view, presumably the conclusions of such arguments are not just defeasible but actually defeated by other well-supported scientific considerations.

Similar considerations apply in cases in which a more well-meaning scientist might end up endorsing what is ultimately a bad argument. Perhaps this scientist puts too much weight on what turns out to be a spurious correlation, or a biased data set. Or perhaps they make a simple error in their calculations. Nevertheless, such a scientist has a defeasible apriori right to believe that they are possession of a good argument. When science goes well, the scientist will eventually come to acquire defeaters for the claim that their argument was good, at which point they will no longer be justified in believing that the argument in question is good. The skilled scientist will recognize this, and no longer endorse their earlier argument. The scientist who does not becomes the object of right criticism.

In conclusion, our principle tells us that rational insight – that is, our capacity to recognize and apprehend arguments – gives us defeasibly justified belief on apriori grounds. This use of rational insight is in general substantive, and goes beyond trivial or mechanical verification that the conditions for any relevant rules of inference are met. As a result, this paper should be understood as putting forward a view that is rationalist in character. Much (though certainly not all) contemporary philosophy of science tends towards empiricistic views of scientific reasoning. This paper should be thought of as a nudge in the direction of rethinking this prejudice, and a suggestion that philosophers re-examine the question of whether more rationalist ways of thinking might be useful in the philosophy of science.

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