Moving Too Fast: The Making of an Ad Hoc Hypothesis*

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Abstract

A scientific dispute may turn crucially upon whether or not a given hypothesis is ad hoc. So, it is extremely important to determine what makes a hypothesis ad hoc. Yet, previous accounts have failed, either because they have run afoul of the Quine-Duhem problem, or because of other major defects.

I develop a novel account of ad hocness. I propose that a hypothesis is ad hoc when disconfirming evidence leads scientists to accept that hypothesis into their theory even though the core of the theory, in combination with auxiliaries and the evidence, does not entail the hypothesis.

Keywords: theory - modification - evidence - methodology

Resumen

Una disputa científica puede girar crucialmente sobre si una hipótesis es ad hoc o no. Así, es extremadamente importante determinar qué hace ad hoc a una hipótesis. Sin embargo, los tratamientos previos han fracasado o bien debido a que se han visto entremezclados con el problema de Quine-Duhem o bien debido a otros defectos mayores.

Desarrollo un tratamiento novedoso de la ad hocidad. Propongo que una hipótesis es ad hoc cuando evidencia disconfirmatoria lleva a los científicos a aceptar esa hipótesis en su teoría, aun cuando el núcleo de la teoría, en combinación con las hipótesis auxiliares y la evidencia, no implican la hipótesis.

Palabras clave: teoría - modificación - evidencia - metodología

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Many disputes, especially those in science, turn crucially upon whether a given hypothesis is ad hoc. Yet there has been no adequate account of ad hocness. In this paper I will propose an account of ad hocness that does not fall prey to the same sorts of objections that previous proposals concerning the nature of ad hocness do. It does this while still retaining an intuition concerning ad hocness that underlies these previous accounts: that scientists or theorists do not have a good reason to adopt a hypothesis when they do so ad hocly.

Because the charge of ad hocness, in deed if not in word, is used to discredit hypotheses in science, courtrooms, and other important domains, it is crucial to have a theory of what ad hocness consists in. We need to be able to determine when the charge legitimately applies and why ad hocness is objectionable. When there is recalcitrant evidence and we want to maintain our theory in light of it, we shouldn't accept just any hypothesis in order to reconcile our theory with the evidence. This smells of improper theory modification.

In the next three sections, I will discuss in more detail the problem posed by ad hoc hypotheses and survey three failed accounts. These sections will set the stage for a fuller explanation of my account by highlighting traps that my account will avoid. In particular, there seem to be some desiderata that a good account of ad hocness should fulfill. A good account of ad hocness would capture the notion that ad hocness is always a vice, which is how the term is used in scientific practice. It would not appeal to the psychology of the individual scientists accepting the hypothesis, as ad hocness is a methodological vice, and not an epistemological one. A good account of ad hocness should also accurately diagnose canonical examples. And, finally, a good account should avoid things such as the Duhem testability problem. All of these desiderata will be discussed in more detail in the following sections.

1. Introduction to the Problem

According to the hypothetical deductive tradition,[1] hypothesis within a theory are examined for their empirical predictions. A theory is confirmed or disconfirmed depending on whether the observations are in accord with these predictions. However, a large problem arises for this tradition. A theory cannot typically be tested by itself. It needs auxiliary hypotheses in order to have predictive value. The auxiliary hypotheses enable something like, for example, Newton's Law of Universal Gravitation, to be tested. In order to see if, for example, the motion of the moon is predicted by the Law of Universal Gravitation, the scientists testing the theory need to figure out the distance between the moon and the sun, and the moon and the Earth, amongst other things. To determine these distances, the scientists also assume that, for example, light travels in straight lines. And all of these are auxiliary hypotheses: hypotheses needed

[1] Whose proponents were early Hempel and Popper, among others.

to be assumed so that the main theory or hypothesis may be tested. So when a theory seems to be disconfirmed by observation, the theory can still be saved in the sense that the evidence can always be made logically consistent with the theory. All that has to be done is to tinker with its auxiliaries, thus leaving the core of the theory intact. Duhem, among others, famously pointed out this problem (Duhem 1962, p. vi).

This is a problem for the hypothetico-deductive tradition because one *can* render any disconfirming evidence theory-consistent, but this is not always justified. One way for a modification to be unjustified is for it to be ad hoc. Here are three examples where ad hoc modifications clearly seem to occur:

- *Funiculus case*: One of the scientific theories of the 1600's was *plenism*. The *plenists* believed that there were no vacuums in nature. Observations on the spring and weight of air using mercury, made by the 1660's, seemed to conflict with the *plenists*' theory. In order to explain why a glass tube, filled with mercury and placed in a pool of mercury, appeared to have a space between the top of the mercury and the top of the tube, Franciscus Linus postulated the existence of an invisible material connecting the mercury with the top of the vessel, filling the space completely and suspending the mercury in the tube. Linus named this material the *funiculus*[2] (Boyle 1662, p. 22; Greene 1962, pp. 461-464).
- *Ptolemy case*:[3] Ptolemaic astronomy offered a geocentric model of the solar system that posited circular orbits for the heavenly bodies. In order to explain why the heavenly bodies appear not to travel in circular orbits, Ptolemaic astronomy posited for each of the heavenly bodies some combination of eccentrics and epicycles. Suppose that a proponent of Ptolemaic astronomy makes planetary observations that cannot be explained by the posited epicycles and eccentrics. In response, she adds some further epicycles to the model (Kuhn 1962, p. 68).
- *Continental drift case*: In the early 20th century, Wegener proposed that all the continents were originally one, and that they had drifted to their current locations over time. He proposed that the continents moved by plowing through the ocean floor, as a result of gravitational forces. However, it was demonstrated that these gravitational forces would not be nearly strong enough to make the continents move in this way. Du Toit, a proponent of Wegener's theory, proposed in response that the movement was a result of radioactive melting of the ocean floor at the edges of the conti-

[2] Boyle himself, in responding to this hypothesis, observes that the funiculus was created only so that Linus could avoid admitting the existence of a vacuum (Boyle 1662, p. 21).

[3] This example is not, in fact, historically accurate. However, I am going to use it anyway because it has become a canonical example of ad hocness. For Ptolemy's actual account, see The Almagest. In both Ptolemy's actual account and in the canonical example, there is a case to be made that the account is instrumental in nature, as distinct from an epistemological realist position. However, whether the account is instrumentalist or realist, it still falls prey to the vice of ad hocness, both in our intuitions and according to my account of ad hocness, amongst others.

nents. Later, after more evidence had been compiled concerning the geology of the ocean floor and possible mechanisms for movement of the plates, plate tectonics, a version of the continental drift theory, became generally accepted[4] (Wegener 1966; Stanley 1989, pp. 165-175; Hurley 1970).

In contrast, here are two examples where the modifications do not seem ad hoc:

- *Neptune case*: Observations of Uranus made by the mid-1800's seemed to conflict with the predictions of Newton's laws of motion and gravity coupled with auxiliary hypotheses specifying all of the massive bodies in the solar system. Adams and Leverrier modified the auxiliary hypotheses concerning the number of massive bodies to accommodate the observations, postulating more massive bodies and predicting Neptune's existence (See 1910).
- *Vulcan case*: Observations of Mercury made by the mid-1800's seemed to conflict with the predictions of Newton's laws of motion and gravity. Leverrier and others modified auxiliaries to accommodate the observations, predicting the existence of a large intra-Mercurial mass sometimes known as Vulcan.

And here is an example where the theory seems highly suspect but where its modification does not seem ad hoc:

- Pre-Adamists case: Certain Pre-Adamists believe that humans were created as the Bible said, when the Bible said. There arose evidence of human and human-like fossils whose carbon dating demonstrated that they predated any possible Bible genealogy. In response, the Pre-Adamists postulated the existence of pre-Adam hominids, which were not *real*, *souled*, humans (Ross 1998, Grigg 2002).

Philosophers have previously attempted to identify the difference between the Ptolemy, *funiculus* and continental drift-type cases, on the one hand, and the Neptune, Vulcan and Pre-Adamist-type cases on the other. In the next section, I will highlight two ways in which philosophers have tried, and failed, to meet this challenge.

2. Two Failed Views: No Other Motivations and No Other Evidence

It has been suggested that a given scientist's adoption of a hypothesis is ad hoc if and only if her only motives for adopting it are to save a cherished theory from apparently disconfirming evidence (Hempel 1966, p. 29; Strevens 2001, pp. 533-544). I will call this the No Other Motivations thesis, or NOM.

A main problem for NOM concerns the dubious connection between a scientist's reasonableness in adopting a given hypothesis, barring other major problems, and her thoughts or motivations at the time. A scientist who adopts

[4] Steven J. Gould labels this case ad hoc, as well, in Ever Since Darwin (Gould 1977, p. 160).

a hypothesis solely in order to save a cherished theory may still be justified in adopting the hypothesis. Here's an example to illustrate this point: scientists Kevin and Brian work together to create a new hypothesis. They create and then introduce this hypothesis because there has arisen new evidence that is inconsistent with the current theory T. They are committed to the same scientific background, have read the same articles, and are committed to the same theories. Everything about their respective situations is exactly the same with one exception: Brian is motivated to introduce this hypothesis because he knows that the President really likes theory T and will generously fund any project that *saves* theory T. Kevin, on the other hand, realizes that this new hypothesis will also predict other phenomena that will likely occur, based on the new evidence that has been found.

In this example, it looks like a proponent of NOM would be committed to one of the following claims: (a) the hypothesis is not ad hoc if we look at the motivations of Kevin but it is if we look at Brian's motivations; (b) the hypothesis is sort of ad hoc –because the two scientists' motivations were different–; (c) the hypothesis is ad hoc –at least, until Brian realizes that it does predict other phenomena over and beyond theory T and, thus, T-plus-hypothesis is a better predictor than T alone–; (c) does not seem to be a very satisfactory option because it more heavily weighs Brian's motivations without giving justification for this. Yet, options (a) and (b) both force us to give up the idea that something is either ad hoc or not.

There is, here, a dubious connection between the reasonableness of a scientist adopting a given hypothesis (barring other major problems) and what the scientist was actually thinking at the time. Appropriate theory modification is a matter of following the correct procedures. It is in following these procedures that the scientist will be likely to make the appropriate modifications. It does not matter why it is that the scientist follows these procedures, nor does it matter if the scientist understands how the procedure works.

A skeptical reader might view the Kevin and Brian example with suspicion. He might think that this story is being set up as if Kevin and Brian have identical situations except for one insignificant difference in their motivations. Yet, the objection goes, this difference ought to be seen as significant. The only way that this difference would be insignificant, the skeptic claims, is if the example presupposes a consequentialist methodology. Like consequentialists in ethics, the skeptic claims, there can be consequentialists in scientific methodology. And, as in ethics, there is also another way to view scientific methodology that is not recognized in the Kevin and Brian example. This would be a deontological view of scientific methodology, where the motivations of the scientists are the *most* important criterion for the acceptability of their actions.

The Kevin and Brian example is presupposing a consequentialist conception of the situation: that is, Kevin and Brian were committed to the same theory, were aware of the same recalcitrant evidence and came up with the same hypothesis to save the theory in the face of this evidence. Therefore, supposing consequentialism, Kevin and Brian's hypotheses ought to be evaluated the same. However, the skeptic continues, this conclusion only follows given the presupposed consequentialism. Yet, there is a very important difference between Kevin's situation and Brian's situation: they have different motivations to introduce this hypothesis. Having read all of the same literature and being committed to the same background, etc., gives both of them access to the same sorts of motivations. Kevin acted according to the correct motivation and, so, is justified to believe that the new hypothesis is true. Brian, on the other hand, did not have as his *actual* motivation the correct motivation to introduce the hypothesis. Yet, he, too, had the correct motivation at his disposal. Therefore, Brian is not justified in believing that the new hypothesis is true. Therefore, in Brian's case, the hypothesis is not on the same footing as in Kevin's case. So, in fact, scientist's motivations are very important in deciding whether a certain hypothesis is ad hoc or not.

This reply to my objection rests on a misunderstanding of what is going on when a hypothesis is labeled as *ad hoc*. This is because, in discussing which one of these scientists is justified in believing that the hypothesis is true, the reply is making ad hocness an epistemological matter. However, ad hocness is a methodological concern. Labeling a hypothesis *ad hoc* is to claim that the scientific endeavor went wrong in its procedures. An account of ad hocness will give scientists a rule for determining whether or not the acceptance of a hypothesis is a permissible act. It is a far different issue as to whether or not a scientist is *justified in believing* that a hypothesis is true. It will be up to the proponent of the methodological account to demonstrate that it is a good account that will cause scientists that follow it to get the right results. The scientists themselves don't need to be justified in their beliefs in order to use the methodology to prevent ad hoc acts.[5]

Here is an example in order to clarify my point. Suppose that there are several mechanics that work at a garage that is noted for its excellence in auto repair. There are several ways that the different mechanics in this shop determine what procedures they need to undertake in order to fix the cars that come into their shop. Some of the mechanics hook up the cars to a computer that has been pre-programmed to interface with the computer chips in the cars in order to determine what repairs need to be made. Some of the mechanics use the algorithms that the computer uses in order to determine the needed repairs. The rest have a detailed understanding of the components of the cars, how the computer chips work to make the car operate and how certain types of problems cause certain issues. The diagnostic computer does not understand what it is doing: it has been programmed to produce certain results given certain inputs. Those mechanics that are using the computers do not need to understand how the diagnostic computers work, either. And they could just be using the

[5] Reichenbach makes a related point in Experience and Prediction, when speaking about the distinction between the context of discovery and the context of justification. See, e.g., Reichenbach 1938, pp. 6-7, 382.

diagnostic computers because they know that the boss really wants them to do so and the mechanics that use the computers get more money from the boss. The story might be similar for those that use the algorithms. Those mechanics also don't need to have good reasons to use the algorithms and also don't need to understand how these algorithms work. In this garage, all the mechanics get the same results, even though they have different reasons for arriving at these results. And, therefore, the system by which they fix the cars is still good, even though not all of the mechanics understand how the system works and not all of them are using the system because of a motivation to fix the cars properly: some are using the system just because the boss will pay them more if they do.

The mechanic situation is analogous to the situation that a scientist faces when trying to determine whether or not a certain hypothesis introduced to save a theory in the face of recalcitrant evidence is ad hoc. Some scientists might well understand how the procedure for determining a non-ad hoc act works and some scientists might not. Some might have motivations other than the motivation for more predictions. However, none of this matters, as long as the system for determining ad hoc and non-ad hoc acts works well. Scientists might well have unsavory motives. We are just interested in giving them a good system –a good rulebook– to follow. This rulebook doesn't tell them what motivations to have. It just tells them what is acceptable to do, given a certain input. This would be like determining what moves are acceptable or not, given that you are playing a game of chess.

So, in the case of Kevin and Brian, it would be inappropriate for the rulebook –the system for determining whether or not a certain act is ad hoc– to give them two different rules to follow. This would be inappropriate because they have identical inputs. These inputs are identical because the only difference between Kevin's situation and Brian's situation is each person's motivation, which does not factor in to what is relevant according to the rulebook. Kevin might still be *justified* to believe that his hypothesis is true, and Brian might not be. However, the rulebook or the system, given the same inputs, must give them the same output at the same time.

In addition to the problem highlighted by the Kevin and Brian example, there is another worry facing NOM. It might well be the case that a certain hypothesis introduction ends up being a benefit for a given theory, even though the scientists introduced it NOM-ly. An example would be Planck's introduction of the constant, *h*, in order to solve the problems that arose for the then-current theories of blackbody radiation (Planck 1967, p. 83). The problem was that neither of the theories that explained blackbody radiation in terms of the wavelengths of the radiation could account for all wavelengths of emission. The addition of this constant made it possible to account for all wavelengths. Planck was introducing this constant just to save these theories.[6] Yet, Einstein

[6] The way that Planck introduces his constant, h, is quite suggestive. When discussing stationary energy distributions and how to determine them, Planck states: "after the stationary energy distribution is thus deter-

later validated this introduction. Would, then, we want to say that this hypothesis introduction was ad hoc but that it was a good introduction, nonetheless?

According to NOM, we would have to say that this was a good ad hoc introduction. For NOM, it was ad hoc because of Planck's motivations, as evidenced by his language and the way that he characterizes the constant. It was a *good* ad hoc modification, for NOM because of the consequences stated above. However, against NOM, we would *not* want to label this an ad hoc hypothesis introduction, for (at least) two reasons. Firstly, the label *ad hoc* is used exclusively in a pejorative manner in scientific practice. It is true that, in certain situations, there might be reasons to choose or to accept an ad hoc hypothesis. After all, there are other vices or virtues that a hypothesis acceptance might have. And, in certain cases, there might be a virtue that a hypothesis has, that would give us a reason to choose one hypothesis over another. And, of course, hypothesis acceptances can have all sorts of different virtues and vices. Of course these virtues and vices have to be weighed against each other in order to determine an appropriate course of action. However, just because some hypothesis has, say, a certain virtue, that does not mean that an already identified vice disappears.[7]

Secondly, the introduction of *h* by Planck just doesn't seem to be a case of ad hocness at all. In order for his Radiation Law to be compatible with Wien's Law –which it had to be, because Wein's Law was accurate in certain wavelengths– it was necessary for Planck to posit exactly the *h* that he did. There was no other choice, no other possibility. This does not seem ad hoc and a positive corroboration of my account is that my account deems the addition of *h* not ad hoc.[8]

So on views that rely on NOM, ad hoc hypotheses need to be divided into a *good ad hoc* category and a *bad ad hoc* category.[9] This goes against scientific usage, where ad hocness always has negative connotations.

mined using a constant, h," (Planck 1967, p. 82). The reason why this quote is suggestive is because Planck speaks of *h* as a constant, not *the* constant, and because he speaks of it only as a constant, not as the more fundamental number which it has come to be understood.

[7] For more on this point, see the last paragraphs of endnote 9, where I discuss additional reasons for thinking that ad hocness ought always to be considered a vice, even putting aside the fact that scientists and other disciplines use this term almost exclusively as a pejorative. Also, see the discussion in my positive account of what counts as ad hoc.

[8] My thanks to an anonymous referee for stressing that I needed to address this point and for explaining, in clear terms, why the introduction of *h* by Planck can scarcely be considered a case of ad hocness, due to the necessity of Planck's taking the energy element *e* to be equal to *hv*.
In fact, on my account, the Planck case is a bit more complicated than what I present above. I argue, in other places, that the addition of *h* changes the core of the theory. If the core of the theory is changed, then the addition cannot be ad hoc or not ad hoc: ad hocness is a methodological issue whereby the auxiliary hypotheses are changed/improperly added in order to reconcile a theory with recalcitrant evidence. If the core of the theory is changed, then it becomes a shift in theory, not a potential case of ad hocness. I do not have room to discuss this point in more detail here. However, even if the addition of *h* were not to be considered a change in the theory's core, it was necessary for Planck to introduce it and, therefore, it is not a case of ad hoc hypothesis acceptance, on my account.

[9] See, for example, Strevens's Bayesian account of glorious rescues and desperate rescues. In his account, the difference between a good ad hoc hypothesis (a glorious rescue) and a bad ad hoc hypothesis (a desperate rescue) depends on the standing of the theory in question. In a glorious rescue, the to-be-discarded auxiliary

It has also been suggested that the acceptance of a hypothesis is ad hoc if and only if the only evidence that supports this hypothesis is the recalcitrant evidence that the hypothesis is intended to explain (Hempel 1966, p. 29). I will call this view the No Other Evidence thesis, or NOE.[10]

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hypothesis gets disconfirmed the most by the evidence and this will happen when the prior probability of the main hypothesis is much higher than the prior probability of the auxiliary. In a desperate rescue, the main hypothesis gets disconfirmed the most by the evidence, because its prior probability was very low in relation to the prior probability of the to-be-discarded auxiliary, yet scientists keep the main hypothesis and get rid of the auxiliary, ad hocly introducing another.

There is, I feel, a huge problem in relying on the prior probabilities of the theories in question. However, I will focus on the problem of having an umbrella term for two very distinctive phenomena. The problem in so doing is twofold. The first problem is that ad hocness is used as a pejorative in science and in (most of) philosophy. People who claim that there is such a thing as a good ad hoc move will have to give a good reason as to why it should be considered ad hoc at all. Making ad hocness be heterogeneous runs strongly counter to how scientists use the term ad hoc. Having part of one's theory labeled ad hoc is considered a detriment to the theory, not a form of approbation. Something that philosophers of science must be sensitive to is to how terms are actually used in science. If we are to differ markedly in our use of terminology, we must give a good reason for so doing.

The second, related, problem is that, on occasions where certain types of acts can be clearly distinguished, and there is a reason to so distinguish them, it is a mistake to place such acts under the same umbrella term. For example, Strevens's label glorious rescue seems to be another way of labeling instances where it is rationally permissible (maybe even encouraged) for scientists to introduce or accept a hypothesis. To label some act a desperate rescue seems to be another way of labeling an instance where it was not rationally permissible for scientists to do so. To label the former as a good ad hoc move and the latter as a bad one seems to unnecessarily complicate the issue.

There may be instances where there isn't any principled way to make a distinction between, say, two different types of hypotheses. In contrast, there is a major, and clear, distinction between a move that is rationally permissible to make and one that is not rationally permissible to make. Why not respect this distinction, and the way in which scientists use the term ad hoc, and label the rationally impermissible moves ad hoc and the rationally permissible moves as good scientific methodology?

After all, an important part of scientific methodology is to modify one's theories in the light of disconfirming evidence. To do otherwise would be either to throw over a theory the instant disconfirming evidence arose, no matter the theory's virtues, or to maintain theories that will be empirically inadequate. These modifications are being made because of disconfirming evidence, which seems to point to the possibility of their being ad hoc. To label indiscriminately all (or most) of these types of theory modifications as ad hoc is either to misrepresent what is going on or to render the term ad hoc rather meaningless. It will become meaningless if it embodies too many actions, especially if the actions that it embodies are very heterogeneous, or if the term goes too far afield from scientific practice. If the term is meaningless, why use it at all? I don't find any of these consequences acceptable. (See Leplin 1982, pp. 240-241, for an elegant discussion of this problem.).

[10] Karl Popper's related view is that ad hoc hypotheses are unfalsifiable: that is, that there are no experiments different than the one that caused the hypothesis to be introduced into a given theory that can be performed where there is an opportunity for the hypothesis to be demonstrated false. These criteria –that of Popper and the NOM and NOE stated above– are related because both trade on the idea that an ad hoc hypothesis is one whose only implication is the accommodation of the recalcitrant evidence so that scientists may still uphold their theory.

Popper's account falls prey to the same flaw to which NOE falls prey: the problems of how to identify testability. Claiming that a hypothesis has no additional test implications is similar to claiming that a hypothesis is unfalsifiable in that these are both, at heart, claims about auxiliary hypotheses. And the worry is the same, as well: how to determine what count as acceptable auxiliary hypotheses and what count as unacceptable? See the above discussion for more details.

Another key figure in this literature, Imre Lakatos, posited something similar to NOE, though with elements of the automatic recipe I discuss, below. On Lakatos's account, there is a hard core of a theory, which contains its key or basic assumptions, and a protective belt of hypotheses that are produced in order to reconcile the core of the theory to the observations. The key, here, is in how many new hypotheses are being added in comparison to how many new predictions that theory (the core plus the belt) is making. On his account, the theory is regressive if it must keep adding additional hypotheses without the compensation of making new and true predictions. The regressiveness of a theory best matches up to what I have been talking about, previously, when discussing ad hocness. However, as Lakatos's account involves the idea of new predictions, it will fall prey to the problem faced by NOE, as I have outlined, concerning predictions.

A main problem for NOE concerns that connection between a scientist's reasonableness in adopting a given hypothesis and the amount, or type, of evidence that supports it. This problem should sound familiar, as it is a version of the first problem that faces NOM. A scientist who adopts a hypothesis that is supported solely by the disconfirming evidence may still be justified in adopting this hypothesis. Planck's introducing his constant to save theories of black body radiation exemplifies this problem, as well. The sole support for the introduced constant was the disconfirming evidence. It was only later that other evidence was found to support it. So, on views that rely on NOE, the same division between bad and good ad hoc hypothesis will have to occur.

NOE has a further problem, in that it has a problem related to one of the hypothetico-deductive tradition's problems. This problem has the form of a dilemma. Typically, in mature sciences, we cannot test a single hypothesis in isolation. So, if the requirement for non-ad hocness is that the hypothesis *by itself* leads to additional test implications, all hypotheses will be ad hoc. On the other hand, suppose that the requirement for non-ad-hocness is just that the hypothesis together with some set of auxiliary hypotheses leads to some additional test implications. If we allow a given hypothesis to be tested *in conjunction with* auxiliaries, we can engineer the auxiliaries in such a way that no hypothesis will be ad hoc because all hypotheses would lead to additional test implications.

Clearly, we would need restrictions to be placed on the auxiliary hypotheses. Yet, this, too, will lead us to trouble. A way in which we might restrict the auxiliaries so as not to allow for such trouble might be to limit them to the ones that we have already accepted, or to the ones that we think are true, or something like that.

If we were to limit the auxiliaries to ones that we think are true, we might be able to make some distinction between hypotheses that are legitimately testable and those that are not. Yet, limiting the auxiliaries to those that we believe to be true will not be enough to fully overcome the testability problem. We still can add hypotheses that we believe to be true to some other hypothesis in such a way that this hypothesis will be rendered testable, no matter what it posits. For example, I might believe that the hypothesis "the Earth is round" is true. All I need to do is to add this hypothesis to any hypothesis whatsoever and I will make that hypothesis testable. And changing the restriction to include only those auxiliaries that we have already accepted seems to have the same problem.

Instead, we might claim that a hypothesis is testable only if, when combined with one or more auxiliary hypotheses that we already believe to be true, we can infer some observation statement not previously inferable from the auxiliary hypotheses alone. This formulation would get around the previous objection because the additional testability in the previous objection will arise from the auxiliary hypotheses alone: in that case, "the Earth is round" hypothesis. However, this does not get rid of all objections to this formulation of testability. Suppose you have a hypothesis H, and suppose A is "either $\sim$H or else R", where R is some testable hypothesis that you already believe. A will be true, according to you, and A & H entail observation statements that are not entailed by H alone. Yet, on purpose, R is (deliberately) theoretically irrelevant, in this particular case, and should not make the difference between an ad-hoc hypothesis acceptance and a non-ad-hoc one. And this sort of situation can be constructed for any such hypothesis. So, even limiting the auxiliaries in this way will not be enough to fully overcome the testability problem.[11] Therefore, NOE is not a desirable theory of ad hocness.

There is a final failed account of ad hocness that I will review, before turning to my positive thesis. This is the characterization of ad hocness as an automatic recipe for modifications.

3. Ad Hocness as an Automatic Recipe

On the automatic recipe view, which is related to some comments of Kuhn's (Kuhn 1962, p. 78), it is the historical background that determines whether the adoption of a hypothesis is ad hoc. It might be helpful to think about the Ptolemy case, where the scientists add an epicycle when, say, Mercury isn't where it is supposed to be at a certain time. They then add another epicycle when more disconfirming evidence concerning Mercury arises, and so on. What is ad hoc, according to this view, is the adoption of a hypothesis as a result of recalcitrant evidence when the only additional test implications of the hypothesis are such that, if those implications turn out to be false, then the same kind of modification would again bring the hypothesis into line with the evidence. Thus, we have an *automatic recipe* in the cases of ad hoc modifications and we do not in other cases of non-ad hoc (repeated) modifications. We can keep adding epicycles upon epicycles, as it were.

This view is preferable to both NOM and NOE because the scientist's motivations are no longer essential in determining a hypothesis's ad hocness. It is additionally compelling because it seems to identify why the canonical example of adding epicycles to epicycles in Ptolemaic theory is not reasonable. It will not be reasonable, according to this view, because we could always add another epicycle if more recalcitrant evidence arises.

However initially compelling, this view fails for two reasons. Firstly, there is the test implications problem that I discussed concerning both the NOE account and the hypothetico-deductive tradition. If we are able to add additional hypotheses to the one that is in question, it looks like additional test implications can be manufactured for all hypotheses. Secondly, there does not seem to be a clear way to make the distinction between repeated modifications that qualify as ad hoc and those that don't. So, there does not seem to be a way to determine whether some theory's historical background is such that the theo-

[11] The problem that I just discussed is almost identical to the devastating problem faced by Ayer's verificationism in Ayer (1946). This problem is clearly discussed in Wright (1993).

ry is vulnerable to charges of ad hocness. I have already identified the Neptune case as an example of the non-ad hoc adoption of a hypothesis. When there arose a similar kind of recalcitrant evidence concerning Mercury, proponents of Newtonian theory postulated the existence of Vulcan to make their theory consistent with the evidence. Both Neptune and Vulcan are modifications *of the same kind*. Thus, the automatic recipe account would label the adoptions of at least one of these hypotheses as *ad hoc*. The problem is that there doesn't seem to be a way to distinguish between the Neptune and Vulcan cases, on the one hand, and the Ptolemy case on the other, according to this view.

Supporters of the automatic recipe view might say that there *is* a way to distinguish between the Newtonian case and the Ptolemy case. They might claim that the Newtonian case leads to additional test implications while the Ptolemy case does not. However, this response will not work because of the problems associated with demanding additional test implications that I talked about for NOE and the hypothetico-deductive tradition.[12]

NOM, NOE and the repeated modifications accounts fail to adequately identify ad hocness. I turn now to my own view, which does not have the failings of these views. To set up my account, I must first define what sorts of acts can be ad hoc.

4. What Can Be Ad Hoc?

A scientist's theory is always set against a background consisting of currently accepted scientific theories and assumptions relevant to this theory. If disconfirming evidence arises, a scientist will always have three possible courses of action, should she accept the disconfirming evidence. Option A: the scientist can discard her theory. Option B: the scientist can accept a hypothesis that makes the evidence consistent, or relieves the tension between the evidence and the theory. Option C: the scientist can recognize that there is a tension, or an inconsistency, between the evidence and her theory plus the background but decide that there is not enough information to modify her theory for the time being. A scientist must take option B in order to commit a potentially ad hoc act.

In cases where a charge of ad hocness is possible, then, a scientist must keep the core of her theory intact. By the core, I mean the parts that, if eliminated, would cause the scientific community to consider it a new theory, instead of a version of the original. Throwing out part of the core would constitute rejecting the theory, and so would not be liable to the charge of ad hocness (Leplin

[12] There is another avenue open for automatic recipe advocates. They might claim that what counts is the number of modifications. In the Newtonian case, there were only two modifications. In the Ptolemy case, however, there were many more. This response does not get to the heart of the matter, either. It's true that there were only two modifications in the Newtonian case. However, there could have been more. And, it's true that in the idealized Ptolemy case, there were many more modifications. But, there didn't have to be. Pinning ad hocness on the number of modifications is missing the mark. Linus, e.g., postulated only one modification in the funiculus case. Yet, it still seems awfully ad hoc.

1975). An ad hoc move purports to save the theory's core by changing some part of the set of auxiliary hypotheses needed to test the main hypothesis or hypotheses that constitute the theory's core. In the Ptolemaic case, for example, a change to heliocentricity in order to explain the planets being in different positions at a given time than was postulated by the theory would constitute rejecting Ptolemaic theory in favor of a new theory. Part of the essence of Ptolemaic theory is its geocentricity. Therefore, such a change would not be liable to the charge of ad hocness. Adding epicycles to explain the planets' positions, on the other hand, keeps the core of Ptolemaic theory intact and, so, *is* liable to the charge of ad hocness.[13]

It will not always be appropriate for the scientist to take option B. Suppose she holds a particular theory to be true. Following Linus's lead in the *funiculus* case, she maintains her theory and attempts to reconcile it with the evidence. Accordingly, she will accept a new hypothesis into her theory plus auxiliaries. Something in the theory plus auxiliaries must have been modified because, if it were not, this set of hypotheses would entail the falsity (or the very low probability) of the disconfirming evidence statement. However, she still accepts the core of her theory, which is consistent with this evidence. The intuition behind the charge of ad hocness is that our scientist ought not take option B if her situation is appropriately analogous to Linus's. In the *funiculus* case, Linus addresses the problem in a particular way, when the situation is such that he should instead take one of the other two options: A or C.

Linus accepted the hypothesis that there was an invisible substance –a *funiculus*– that connects the mercury with the top of the tube. Yet, there are other ways that Linus could have reconciled the new evidence with his theory and it is underdetermined which, if any, of these ways should be accepted. The tube might have been perforated with tiny holes. Certain types of gases might be able to permeate glass. Mercury might emit vapors when in glass tubes, where these vapors rise to the top and separate the mercury from the top of the tube. Because of this underdetermination, Linus should not have taken option B.

Similarly, Du Toit accepted the hypothesis that the movement of the continents was a result of radioactive melting of the ocean floor at their edges. Yet, there are other ways that Du Toit could have reconciled the new evidence with the theory of continental drift. Almost nothing was known at the time about the geology of the sea floor. Therefore, Du Toit should not have accepted this hypothesis into continental drift theory. He could have tried to test it, or could have entertained it as a possibility, but he should not have included it as part of the theory. If some other scientist's situation is appropriately like Linus's or Du

[13] Another example: if a Newtonian theorist dealt with a problem that arose for the theory by rejecting absolute simultaneity, it is highly probable that the scientific community at the time would not consider the modified theory to be Newtonian theory at all. This modification occurred when relativity theory became accepted. When we discuss relativity theory, we do not claim that it is a new version of Newtonian theory, one that no longer contains the notion of absolute simultaneity. Instead, we claim that relativity theory proponents discarded Newtonian theory in favor of the one they currently hold. This is because the commitment of absolute simultaneity is one of the core elements of Newtonian theory.

Toit's, she, too, will unreasonably take option B by accepting one hypothesis when there are insufficient grounds for doing so.

In sum, I think that the fundamental mistake committed by the scientists in the Ptolemy, *funiculus* and continental drift cases is that a hypothesis was accepted when it was underdetermined which, if any, hypothesis should have been accepted. This is the core idea behind the theory I want to propose. Now lets make this more precise.

5. The Formal Account

Let M be an agent, T be a theory, B be a body of background knowledge, and t be a time, such that M accepts both T and B at t. Let T_c be the core of T and let T include a set of auxiliary hypotheses, used in tests of T_c, in addition to T_c itself. Let E be an evidence statement that is recalcitrant for T, given the background B. Let M accept E. Let H be a hypothesis that is inconsistent with some set of auxiliaries in T. Let X be the set of (relevant) auxiliary hypotheses in T that remains after (1) shrinking the set to make E be consistent with T_c and the remaining auxiliaries, and (2) then shrinking the set to make H be consistent with T_c and the remaining auxiliaries. Let A ($= \langle M, T, B, E, H, t\rangle$) be the act in which agent M, at time t, encounters the problem to T posed by E, and attempts to solve it by accepting the hypothesis H, while retaining his commitment to T_c, B and X.

A is ad hoc iff $\{T_c, X, E\}$ do not entail H and $\{T_c, X, E\}$ do not entail that H has a high objective probability.

A is not ad hoc iff either $\{T_c, X, E\}$ entail H, or $\{T_c, X, E\}$ entail H has a high objective probability.

If A does not meet all the conditions on it imposed in the above definition, then A is neither ad hoc nor non-ad hoc. That is, "A is not ad hoc" is neither true nor false.

The following will be an explanation of the formal account. Suppose our scientist has decided to modify her theory in the face of recalcitrant evidence. She removes the auxiliary hypotheses that conflict with the hypothesis that is going to be accepted. She will be left with the remaining auxiliary hypotheses and the core of her theory. Imagine the theory T as a list of hypotheses that are individually accepted by our scientist at the time that she is faced with disconfirming evidence. X, the set of auxiliaries remaining after *crossing off* the conflicting hypotheses, is consistent with the hypothesis to be accepted and, in conjunction with the new evidence, E, is also consistent with the core of our scientist's theory, T_c.

We are at the point where our scientist is accepting the hypothesis in order to solve the problem facing her theory, while still retaining her commitment to the core of her theory, the background, and the set of auxiliary hypothesis left after the appropriate ones have been removed.

Our scientist has committed an ad hoc act if, and only if, the core of her theory, in conjunction with the pared-down set of auxiliaries and the (previously) disconfirming evidence, does not entail (the high objective probability of) the hypothesis.[14]

On the other hand, this act is not ad hoc if, and only if, the core of our scientist's theory, the pared-down auxiliaries and the evidence together entail (the high objective probability of) the hypothesis itself.

When this entailment does not occur, there is a sense of arbitrariness in our scientist's decision to accept the hypothesis that she did. The same question arises in response to her choice as it did in response to Linus's choice, or Du Toit's choice: why accept *this* hypothesis and not some other? Ad hocness, then, is a type of arbitrary act that seems methodologically suspect. The scientist decides to accept one response to a problem, when the available evidence and the scientist's theoretical commitments do not warrant this choice over other possible responses. And this assessment holds true *even if* our scientist has thought of only this one response–the one she is accepting–and has not thought of any other ones. As long as the theory, with the appropriate auxiliaries and the evidence, admits of other responses, she is acting arbitrarily in accepting the one that she does. In these cases, it is better to remain in a state of living with the tension, or to reject the theory, than to adopt the hypothesis being considered.

My view is in accord with scientific practice in that the charge of ad hocness, on my account, carries negative connotations. Yet, while ad hocness is always a vice, it is not an indefeasible vice. The hypothesis might possess other virtues that justify our accepting the hypothesis *despite* its being ad hoc. I wish to stress the *despite* because the acceptance is still ad hoc: the hypothesis was not appropriately entailed. Of course, hypothesis acceptances can have all sorts of different virtues and vices. Of course these virtues and vices have to be weighed against each other in order to determine an appropriate course of action. However, just because some hypothesis has, say, the virtue of simplicity, this does not mean that an already identified vice disappears. To say that would be similar to claiming that, since I picked up the check and engaged in scintillating conversation, I magically was no longer late to our dinner engagement. You might *overlook* my lateness because of my generosity and great conversational skills. However, my great conversational skills cannot reverse time so that I actually

[14] Notice, here, that "high objective probability" is in parentheses. This is because, in the cases that I examine for the paper, this clause of my positive account may be left out. I did not discuss any cases where the core of the theory, the auxiliaries and the evidence entailed the high probability of a hypothesis. Yet I wish to allow for a hypothesis acceptance to be non-ad hoc if its high objective probability is appropriately entailed. I wish to allow for this because I recognize that not all cases will give us a strict entailment yet we might wish to claim that it is (almost) strictly entailed, or something like that. The defense of a plausible, comprehensive theory of objective probabilities is beyond the scope of this paper. Even if there were no such comprehensive theory, however, there are still individual cases where theories do entail objective probabilities, such as certain interpretations of quantum mechanics that assign objective probabilities to events. And this sort of objective probability is fairly unproblematic. The reason why I allow the entailment of the high objective probability of a hypothesis is to allow for ad hocness in the case of a non-deterministic theory. Take the GRW interpretation of quantum mechanics, for example. There is, for macroscopic systems, a high objective probability of a nearly immediate collapse at any given time. The collapses are essentially non-deterministic and, so, cannot be entailed simpliciter. However, their high probability can be.

arrived on time. Likewise, an ad hoc hypothesis that has the virtue of simplicity is still ad hoc. It does *not* become magically not ad hoc just because of another virtue that it possesses.

Additionally, it might be rationally permissible to use an ad hoc hypothesis as a placeholder –to act *as if* the hypothesis were a part of the theory– without accepting it, just as we sometimes do with a hypothesis that is known to be false. An ad hoc hypothesis may still be entertained as a possible modification, scientists may engage in research to see if more evidence arises to support it, etc. It is just that a scientist would commit a methodological error in accepting it at that time, with that particular formulation of the theory.

Now, I need to show that the vice of arbitrariness that I have pointed out actually is the vice of ad hocness.

6. Why Arbitrariness is the Vice of Ad Hocness

In order to motivate my claim that the unjustified, arbitrary choice between hypotheses is unjustified because it is ad hoc, I will turn back to the traditional view of ad hocness. On the traditional view, a hypothesis is ad hoc if it is introduced solely to save a theory from recalcitrant evidence and has no additional testable implications.[15]

Of course, this formulation sounds paradoxical: what could be wrong with modifying our theoretical commitments to fit our evidence? Would it be better to adopt theories that do not fit the evidence? My view removes the air of paradox from the notion of ad hocness. Although an ad hoc hypothesis modifies a theory to fit the evidence, its adoption is unwarranted because there is no reason to prefer this way of modifying the theory to fit the evidence over a variety of alternative schemes. To adopt one over the others would be arbitrary at this point.

Moreover, my view removes the air of paradox while avoiding the traps that the traditional view does not: so, for example, my view does not involve additional test implications (like in NOE) and does not require knowledge of the psychology of individual scientists (like in NOM) in order to determine whether the acceptance of a hypothesis is ad hoc.

Having espoused the virtues of my account, I shall now demonstrate how two sorts of objections miss their mark.

7. How Can Scientists Ever Modify New Theories Non-Ad Hocly?

My account might appear too profligate in labeling acts *ad hoc*. Suppose, for example, a scientist accepts a very general statement into his theory in the

[15] This is how Hempel, e.g., formulates the definition of ad hocness in Hempel (1966, pp. 29-30). Basically, it is a combination of NOM and NOE. Having the two in combination does not alleviate any of the problems highlighted for the individual theories, however. It is out of the scope of this paper to present the reasons as to why this is the case: I leave this to the reader.

face of disconfirming evidence. It seems impossible that his theory entail this statement. Therefore, whenever such a statement is accepted into a theory under the right circumstances, this acceptance would seem to be ad hoc. But, of course, we would not want to label all of these acts as ad hoc.

This objection misunderstands my account. Either the acceptance of these really general, law-like statements requires discarding part of the core of the theory, or it does not. Suppose that it does require this. If so, accepting this general statement is to move to a new theory, an act that is not a candidate for ad hocness. Suppose Isaiah's theory consists in two commitments. 1. All violets are purple. 2. All violets grow only in Tanzania. Suppose Isaiah, facing evidence of violets growing elsewhere, accepts the law-like statement, "In all tropical zones there grow a species of violet". Given some relevant background knowledge, such as the fact that Tanzania is not the only land in tropical zones, Isaiah must discard (2) in order to accept the statement. Therefore, Isaiah is really moving to a new theory.

If the acceptance of a law-like statement does not require discarding part of the core, then it becomes a matter of determining whether or not the proper entailment is there. It is true that we would need a very strong set of postulates on the side of the theory to entail a general, law-like statement. The proper entailment will not always exist. However, the theory itself contains law-like statements and law-like statements are strong enough to entail others. So, neither will it be the case that the proper entailment *never* occurs or too rarely occurs. Think about the Neptune case, for example. It is the laws of motion and gravity that entail the existence of a specific center of mass where Neptune is located.

There is another way in which my account might appear too profligate. New or vague theories are not committed to much. Therefore, they will entail little or nothing. It looks like the acceptance of all hypotheses into such theories will be ad hoc, on my account. Yet, we would not wish to label them all *ad hoc*.

I have a straightforward answer to this objection, as well. Given such a vague theory, it is highly unlikely that any evidence will be recalcitrant for the theory. Suppose that the theory consists in the following: "the compositions of all organic bodies are somehow similar". There is not much that would count as recalcitrant evidence for this theory. If an apple, say, is dissimilar in composition in one way to an orange, it is likely to be similar in another. Thus, the evidence does not disconfirm the theory: the theory is not committed to enough. A charge of ad hocness can be appropriate only where a hypothesis is accepted as a result of *recalcitrant* evidence. In this sort of case, as well, the act is not one that can be charged with ad hocness.

It is clear that these objections miss the mark. However, I need to address exactly what I mean by the pared-down set of auxiliaries that remain after making the theory, evidence and new hypothesis consistent with the auxiliaries. To keep silent on this subject is to allow for misinterpretation of my account.

8. A More Detailed Definition of X (The Pared-Down Set of Auxiliaries)

In my formal account, I claim that X is the set of auxiliary hypothesis that remains after first shrinking the set to make the remaining auxiliaries consistent with the core of the theory and evidence, and then shrinking the set to make the new hypothesis be consistent with these. It is important that I make explicit what constitutes X. If I did not, my view might seem to fall prey to a similar objection to the test implications that the hypothetico-deductive tradition faced: that is, whether an entailment occurs or not is entirely dependent on what we allow into X, and there is no principled way of determining what we should allow into X. In addition, various objections to my definition of ad hocness attempt to illicitly manufacture X. To set up these objections, let me explain more fully how X is supposed to be constructed.

X is a pared-down version of some theory T's auxiliaries, so I need to say something about how I see T. I am not considering theories as deductively closed sets of propositions. I cannot do so and maintain my theory of ad hocness because there would not be any way to determine which hypotheses to remove in order to relieve the tension between the theory and the disconfirming evidence. This is because there are infinitely many ways to remove part of a deductively closed set of propositions to make it consistent with the recalcitrant evidence.

Instead, I conceive of the theory as a list of discrete hypotheses accepted by the scientist at the time that the disconfirming evidence comes to light. X will be a pared-down list of propositions created by starting with T and many hypotheses. First, the theory's core will be removed. Then, those hypotheses that, in conjunction with the theory's core, are in conflict with the evidence itself will be removed. Then, those hypotheses that, in conjunction with the theory's core, are in conflict with the hypothesis to be adopted will be removed.

Suppose we are considering the "fertilize every week" theory of rose growing. This theory claims that, if you give your roses two tablespoons of fertilizer every week, they will produce a greater amount of higher-quality flowers. For a certain batch of roses, we fertilize them every week for a month and find that, contrary to what the theory claims, these roses did not produce a greater amount of flowers. We want to reconcile our theory with the evidence by adding the hypothesis "the more acidic the soil, the greater the amount of time needed to produce a greater amount of flowers". To make sure that the adoption of this hypothesis is not ad hoc, we need to determine what constitutes X (the pared-down set of auxiliaries). We will have to remove the core of our theory: that it is best for your roses to fertilize them every week. We will have to remove such hypotheses as "there are no circumstances under which roses are fertilized for a month yet do not produce a greater amount of flowers", as these hypotheses, in conjunction with the theory's core, are in conflict with the evidence itself. If we haven't already, we then need to remove such hypotheses as "the acidity of the soil plays no factor at all in the production of flowers", as these, in conjunction with the theory's core, are in conflict with the hypothesis we wish to adopt. The rose example is intended to illustrate how, generally, to create X. I now want to discuss each step of creating X in more detail.

All hypotheses that, in conjunction with the core of the theory, conflict with the evidence must be eliminated. I will be discussing the hypotheses to be eliminated in terms of minimal sets of auxiliaries in conflict with the evidence, given the core of the theory. Here is what I mean by a minimal set of auxiliaries inconsistent with the core of the theory and the evidence. Let $\Gamma = \{A_1, A_2, \ldots, A_n\}$ be a set of auxiliaries in some theory T. Γ is a minimal set inconsistent with theory's core in conjunction with the evidence if and only if Γ is inconsistent with this conjunction and removing some A from Γ will enable the conjunction of Γ, the theory's core, and the evidence to be consistent. These minimal sets must be eliminated from X: we cannot just pick certain hypotheses to eliminate from these sets and not others. To do so would be arbitrary in just the way that I define ad hocness as being. Instead, all such minimal sets must be crossed off the list of the theory's auxiliaries in order to create X (the pared-down set of auxiliaries).

Finally, any hypothesis that, in conjunction with the theory's core, is in conflict with the new hypothesis, H, must be removed. Not-H needs to be removed (if it hasn't been already), as well as any other set of hypotheses that are in conflict with H. This is to ensure that the theory's core, the pared-down set of auxiliaries, the evidence and the new hypothesis, considered together, will be consistent. Then, X will be properly constructed. The following section will address an objection in which a scientist *illicitly* constructs X. The objection loses its force after we reveal this illicit construction.

9. A Potential Problem with X (The Set of Pared-Down Auxiliaries)

Suppose Terry is a scientist who wants to maintain her theory T in the face of disconfirming evidence, so she considers accepting a certain hypothesis H. She also wants to avoid a charge of ad hocness and her theory's core, in conjunction with the pared-down set of auxiliaries X and the disconfirming evidence E, does not entail the hypothesis that she wishes to accept. So, Terry constructs X to include "if R, then H" ($R \supset H$), where she believes (or knows) R to be false. Terry can construct the conditional such that its placement in X will (wrongly) categorize the acceptance of her hypothesis as not ad hoc: she will just make her proposed hypothesis to consequent of the conditional. R is false, so "if R, then H" will be true. Then, since this truth will be a part of X, Terry can accept H without making an ad hoc move because her theory's core, in conjunction with X and the evidence, will now entail H. This seems to be a big problem for my account because, if X can be constructed in just this manner, no hypothesis acceptance need ever be ad hoc.

In fact, if Terry had believed not-E before receiving the disconfirming evidence E, then Terry believed "if E, then H" and so could simply use it as the material conditional in X. Then, after E is discovered, her theory's core, in conjunction with X and E, entails H and, so, H is automatically not ad hoc by my analysis.

Think of the *funiculus* case as an example. Suppose Linus builds "if there appears to be a vacuum in a tube filled with mercury, then there is an invisible substance suspending the mercury from the top of the tube and completely filling the space between the mercury and the tube" into X in the manner just described. It then looks like Linus's hypothesis will pass through my ad-hocness filter when he accepts it after discovering that there is this space between the mercury and the top of the tube. And this is certainly not the conclusion that I wish to draw from this example.

However, Linus cannot construct X in this manner. The conditional is trivially true, based on the fact that Linus believed the antecedent false. Yet, in constructing X, Linus must discard every hypothesis that (or some combination that), with the theory's core and the background, entails not-E. These auxiliaries cannot be included in X because X includes only those that do not conflict with the disconfirming evidence. So, not-E needs to be discarded. But note that "if E, then H" is in the set of auxiliaries in the first place only because it was believed that not-E was true and this conditional was a trivial consequence of it. Therefore, "if E, then H" will not be a part of X and his acceptance of the *funiculus* hypothesis is ad hoc.

Suppose Terry really does accept the proposed hypothesis, while keeping her theory's core and keeping an illicitly constructed X of the type just discussed. Although Terry constructed her X so that she might avoid the charge of ad hocness, she has not succeeded in her attempt. Her acceptance of this hypothesis is still ad hoc because, had she constructed X appropriately, her theory's core in conjunction with X and the evidence would not have entailed her hypothesis.[16]

To give further support to my account, I will turn to the *funiculus* case and analyze what it is that causes us to think it a clear example of an ad hoc theory modification.

10. Re-Examining the *Funiculus* Case

In the *funiculus* case, consider what *else* Linus might have done instead and contrast it with what he *did* do. Instead of postulating the existence of this invisible material that filled up the space between the mercury and the top of the

[16] There are two objections that might arise which I have not addressed, due to space constraints. One objection is that my account relies on classical logic. However, if a given theory assumes, say, paraconsistent logic, my account of ad hocness won't work. The quick response is to say that paraconsistent logic and its use is not uncontroversial. The second objection is that my account will not be able to diagnose inconsistent theories. Again, I do not have space to discuss this point in more detail. However, this does not seem to be a problem just for my account alone.

tube, Linus might have claimed that "*something* happened" or that there was some sort of unspecified phenomena that caused the appearance of a vacuum in the tube. How is this different than accepting the *funiculus* hypothesis? In the latter case, Linus is accepting the *funiculus* hypothesis in order to explain the anomalous evidence. In the former, there is a conspicuous failure to explain anything at all. Linus, in accepting the hypothesis that "something happened", is really just accepting that he does not know what happened: that is, he has decided, for now, to live with the tension between the evidence and his theory. To decide not to act, for the time being, might be a problem in and of itself. However, since he doesn't act, he can't be acting ad hocly. And our intuitions, I would argue, would point to this reading of the *funiculus* case as a situation where nothing ad hoc occurs.

In the actual *funiculus* case, Linus accepted some particular hypothesis –the *funiculus* hypothesis– when some other hypothesis is just as likely. Why is he insisting on the funicular hypothesis when it could just as well have been something totally different? Linus's theory, in conjunction with the pared-down set of auxiliaries and the evidence, does not entail even the high probability of a *funiculus*. In the actual *funiculus* case, where Linus accepted the *funiculus* hypothesis, it is clear that an ad hoc modification occurred. My account will give the same judgment. Similarly, my account gives the same judgment to the continental drift case and the Ptolemy case, properly fleshed out.

In order to demonstrate that my account will not label as *ad hoc* clear cases of appropriate modifications in the face of recalcitrant evidence, I will examine the pre-Adamists case in more detail.

Pre-Adamists believe in theistic evolution. They believe that the Bible is correct in its timeframe concerning human creation and the age of human society. Yet, they also believe in the accomplishments of archeology, such as carbon dating. There arose evidence of human and human-like fossils whose carbon dating demonstrated that they pre-dated any possible Bible genealogy. In response, the Pre-Adamists postulated the existence of pre-Adam hominids, which were not *real*, *souled*, humans. When faced with this disconfirming evidence, the Pre-Adamists have the same three options available to any scientific (or pseudoscientific) theorist. They could throw away their theory, or modify it so that the new evidence is no longer in tension with their theory, or live with the tension between their theory and the evidence, for the time being. Since they wanted to keep their theory, Pre-Adamists choose the second option.

Is this an appropriate modification to their theory? In order to answer this question, we need to give the pre-Adamists' theoretical commitments in more detail. For the pre-Adamists, it is essential for humans to have souls; the first man lived at time t; fossils are the remains of organisms; carbon dating works; these fossils are the remains of human-shaped animals and carbon-dating dates them before time t. Therefore, it looks like these fossils MUST be the fossils of human-like animals that lacked souls. Thus, the acceptance of this hypothesis is not ad hoc.

You might wonder why I am highlighting this particular case. The Pre-Adamist theory seems to be a perfect example of a wildly problematic theory: exactly the sort of theory that should be abandoned. And I completely agree with you. Due to the large amounts of disconfirming evidence for this theory, and due to the extremely negative social implications that arose from theories such as these, the Pre-Adamist theory looks like a very good candidate for being abandoned as a viable scientific theory.

This is precisely why I have discussed this theory. I wish to emphasize that there is a difference between making a methodologically acceptable modification to one's theory, and having a really bad theory. To borrow some language from Kuhn, there is a major difference between paradigm shifts and correctly or incorrectly working within normal science. And, although the Pre-Adamist theory looks to be a prime candidate for being abandoned, the hypothesis that the scientists accepted into their theory looks to be the only possible hypothesis that they could have accepted, given their theoretical commitments and the evidence. The Neptune and the Vulcan cases are similar to this case in that the hypotheses that were accepted in each of these cases were entailed, given the evidence and the background. Where they might differ is in our endorsement of them as good or viable theories.

11. Conclusion

In conclusion, I have briefly discussed why other, initially plausible accounts of ad hocness fail. I then presented an account of ad hocness that, I argue, correctly captures the essence of ad hocness. Hypotheses accepted in the face of recalcitrant evidence are ad hoc if their acceptance is arbitrary. The benefits of this view are several fold: it captures the notion that ad hocness is always a vice, which is how the term is used in scientific practice; it does not appeal to the psychology of the individual scientists accepting the hypothesis; it accurately diagnoses canonical examples of ad hocness. This account also avoids the problems to which these other accounts have succumbed.

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