On Not Counting the Cost:

Ad Hocness and Disconfirmation

Abstract: I offer an account of ad hocness that explains why the adoption of an *ad hoc* auxiliary is accompanied by the disconfirmation of an hypothesis H. H must be conjoined with an auxiliary (or set of auxiliaries) a' which is improbable antecedently given H, while ~H does not have this disability. This account renders it unnecessary to require, for identifying (bad) ad hocness, that either a' or H have a posterior probability less than or equal to .5; there are also other reasons for abandoning that condition. I distinguish between formal ad hocness, which is bad in the probabilistic sense that it results in disconfirmation of H, and argumentative ad hocness, which actually involves bad reasoning on the part of a subject. The latter is what I call "not counting the cost." This distinction allows us to see why the .5 condition appeared attractive in the first place. The concept of not counting the cost also has implications for other areas of research, including both a Bayesian concept of unfalsifiability and the classic epistemological question of the problem of the external world.

Keywords: Ad hocness, ad hoc hypotheses, Bayesianism, disconfirmation

Disconfirmation and the Bayesian account of ad hocness

In the first edition of *Scientific Reasoning*, Colin Howson and Peter Urbach give an account of ad hocness which goes approximately like this: Take some hypothesis H, some piece of evidence E, and some auxiliary hypothesis a'. Then, the adoption of the conjunction Ha' by the advocates of H in response to E is *ad hoc* just in case the probability of a' is low (that is, less than or equal to .5) after conditioning on E. (Howson and Urbach 1989, p. 110)

Paul Horwich points out in addition that the implausibility of Ha' will be made especially evident by the existence of some specific alternative theory R that could accommodate the data equally well but is less implausible than Ha', but he argues that it is not strictly necessary that such an alternative be available. (Horwich 1982, pp. 107-108)

In the second edition of *Scientific Reasoning* (1993, pp. 146-58), Howson and Urbach apparently eschew the intention of offering a Bayesian account of ad hocness generally. Instead they appear to accept from the existing literature the much broader idea that an auxiliary hypothesis may be called *ad hoc* if adopted in response to evidence requiring a revision in one's beliefs, without the presence of independent evidence for that auxiliary. Not surprisingly, given this broad concept, they argue that, from a Bayesian perspective, independent evidence is not necessary for one to be reasonable in adopting such an hypothesis and that theories which are *ad hoc* in this sense may be scientifically acceptable.

The .5 criterion given above still has a role to play, however. In the second edition, a theory a' which is *ad hoc* in the broad sense is also scientifically "unacceptable" just in case, on the basis of both background evidence and the new evidence E, it has a probability below .5. Similarly, the new conjunction Ha' is a "competent explanation" of E only if Ha' has a probability greater than .5, which is possible only if a' itself is "acceptable." Hence, what was before a "Bayesian account of ad hocness" is in the second edition what one might call a Bayesian account of *bad* ad hocness.

In this paper, I shall always use the term *ad hoc* in a negative sense, occasionally adding the word "bad" to emphasize this. I shall also distinguish between formal ad hocness--that is, ad hocness which has a negative formal effect on a theory H--and argumentative ad hocness, which involves, in addition, some degree of irrationality or failure on the part of a reasoner to acknowledge the true probabilistic state of affairs.

Whether we treat Howson and Urbach's .5 criterion as providing an explanation of ad hocness generally or only of bad ad hocness, there is something *prima facie* unsatisfyingly stark about the account. One is tempted to wonder if a low posterior probability can possibly be all there is to so rich a concept as ad hocness. One criticism to this effect comes from Michael Strevens, who argues that, while we expect the probability of H to decrease in cases of ad hocness, the standard Bayesian account of what Strevens calls a "desperate rescue," as given by Howson and Urbach, gives us no explanation for a decrease in the probability of H.[1] (Strevens 2001, p. 534)

> Why, in a desperate rescue, does the probability of h decrease? On Howson and Urbach's story about a desperate rescue, the low posterior of ha' would appear to be explained entirely by the low posterior probability of a'; the low probability of ha', then, cannot explain the decrease in the probability of h.[2]

Strevens's question about disconfirmation can be answered, but only by significantly fleshing out and refining the initial Bayesian account. By explicitly stating additional conditions that must obtain in order for the adoption of a' to cause a decrease in the probability of H when conditioning on evidence E, we will gain insight into the nature of *ad hoc* theory-making. But in the process, we will see that Howson and Urbach's .5 criterion of (bad) ad hocness turns out to be superfluous, so that, step by step, the original Bayesian account will actually be replaced by a different, though still Bayesian, account.

Consider a paradigm case of ad hocness given by Howson and Urbach.

> One triumphant success [of the psychological theory of Dianetics], a young student, was...announced by the inventor of Dianetics, L. Ron Hubbard, and in 1950 he exhibited

[1]Strevens (2001, p. 535) holds that bad ad hocness occurs when the prior probability of the main hypothesis H is much lower than the prior probability of an auxiliary a which is later replaced by a'. My own account here makes no reference to a previous auxiliary a, and I use the typographical convention of referring to an auxiliary which may be *ad hoc* as a' only because that convention is so widespread in the literature. It seems to me that the common assumption that there is a replaced auxiliary a which previously had an important role to play in conjunction with H is frequently artificial and unhelpful. Sometimes (indeed, often) the alleged replaced auxiliary is merely the negation of an auxiliary a' which is historically postulated later on in response to new evidence. See also footnote 8.

[2] Neither Howson and Urbach nor Horwich explicitly states that, in paradigm cases of (bad) ad hocness, H decreases in probability, though that conclusion is implied by Horwich's discussion.

> this person to a large audience, claiming that she had a "full and perfect recall of every moment of her life". However, questions from the floor ("What did you have for breakfast on October 3, 1942?"; "What colour is Mr. Hubbard's tie?", and the like) soon demonstrated that the hapless girl had a most imperfect memory. Hubbard accounted for this to what remained of the assembly by saying that when the girl first appeared on the stage and was asked to come forward "now", the word "now" had frozen her in "present time" and paralyzed her ability to recall the past. (Howson and Urbach 1989, p. 107)

It seems evident that the young woman's inability to answer the questions disconfirms a proposition like

> D: The application of Dianetics therapy greatly enhances the human memory.

Moreover, the adoption of

> a': Giving a command to a person to do something "now" freezes him in the present and makes him unable to remember things that he otherwise would be able to remember

and the conjunction of a' with D by Hubbard seems intuitively to be an *ad hoc* move.

On the Bayesian view given by Howson and Urbach, the "desperate rescue" of adopting a' should be understood in terms of the low posterior probability of a' and, consequently, the low posterior probability of the conjunction Da'. Certainly it does seem intuitively that the .5 criterion holds here. Even after conditioning on E (the woman's inability to answer the questions put to her), a' seems enormously improbable, a good deal less probable than .5. So, too, does the conjunction Da'. Moreover, the low posterior probability of D is not solely a result of the fact that D was quite improbable to begin with (a point I shall return to later). D seems definitely to have been disconfirmed by the dramatic failure of Hubbard's attempted demonstration.

The implausibility of a' can explain the decrease in the probability of D, but only if additional constraints are spelled out explicitly. Suppose, for the sake of simplicity, that the only way in which Hubbard could make shift to explain his protégé's failure while not admitting the falsehood of Dianetics was by adopting the auxiliary a', which had a low prior probability. In fact, we do find that the auxiliaries adopted in *ad hoc* theory rescues are usually either the only auxiliaries that will help or at any rate the "best shot," the only "good shot," at a fix that will rescue the initially postulated theory. What this means is that the only way for Dianetics to be true after conditioning on E is if a' is also true.[3] Hubbard must now advocate not D *simpliciter* but the elaborated Da'. After the failure of the demonstration, the hypothesis D, which previously had no need of a', is confined only to a small portion of its prior probability space. The need to wed D to the low-probability a' means that D is disconfirmed by the evidence that prompted the theory rescue.

It is important here not to slip into an error. The mere fact that D does not entail a' does not mean by itself that E disconfirms D. Nor does it follow from that together with the fact that a' has a low posterior probability. E disconfirms D if and only if E is less probable given D than given ~D. Here, then, we need to state an implication that Howson and Urbach do not spell out when they talk about the auxiliary a'. What is important is that a' has a low prior probability *given the hypothesis H.* This implication that the *ad hoc* auxiliary is previously improbable given H is clearer in Horwich, who focuses not only on the posterior but also on the prior probabilities.[4]

> Literally, something is *ad hoc* just in case it is specifically designed for some particular purpose. When applied to hypotheses, the relevant purpose is the accommodation of evidence. Thus an element of a theory is said to be *ad hoc*

[3] An anonymous reviewer has suggested that the Hubbard case is funny because there were other and better explanations Hubbard should have tried instead such as memory loss due to stage fright. However, if the girl was really a "Clear" in Dianetics, as this demonstration was to show, she would not have been susceptible to mundane problems like stage fright, according to the theory of Dianetics.
[4] In the first edition, Howson and Urbach (1989, p. 110) imply that their account is the same as Howson's.

> when it is included solely in order that the theory will entail certain statements. This implies that there is no ulterior reason for the inclusion of that element. It is not a natural component, does not harmonise with the other elements and produces a whole which is contrived, complex, and assigned a low credibility in our inductive practice. In other words, a theory is *ad hoc* if and only if its formulation is motivated by nothing more than a desire to accommodate certain facts. Such a theory cannot have a high prior probability or else that feature would constitute a further reason for its postulation. Therefore, *ad hoc* theories are implausible. (Horwich 1982, p. 108)

The advocate of H, or in Hubbard's case D, cannot give even the *appearance* of explaining E unless he adopts a', a hypothesis that he himself had not previously taken seriously. Although a low P(E|H) in the prior distribution does not by any means always mean that E disconfirms H (for E might be equally improbable or more improbable given ~H), we are often quite right to explain the disconfirmation of H by way, *inter alia*, of the fact that E has a low probability, especially a *very* low probability, given H. This is all the more true when we can see quite easily that ~H suffers from no such difficulty. In the case of D, the obvious alternative hypothesis is that Dianetics is false, that there are no such things as "Clears," and that the young woman had never had perfect recall of her past in the first place. This skeptical hypothesis is well able to explain her inability to remember what she had for breakfast on October 3, 1942.

The fact that some needed auxiliary has low probability given H in the prior distribution is an especially useful point for Bayesian analysis. It is all too easy, when we think of theories and evidence, merely to ask whether there is *some way* in which H can accommodate the evidence, without asking about the interaction of that theory rescue with Bayes factors. But the Bayes factor

$$\frac{P(E|H)}{P(E|\sim H)}$$

is not assisted much by our discovery that some auxiliary a' can be cojoined with H to give a high probability to E, or even to entail E, if a' itself has a very low probability given H in the prior distribution. For example, suppose that P(E|H & a') = 1 but that P(a'|H) = .0001. We then know only that the P(E|H) is not less than .0001. And if a' is the only "good shot" to account for E, then P(E|H & a') ≈ .0001. In that case, it could easily be the case that E strongly disconfirms H. All that is required for significant disconfirmation of H is that P(E|~H) is significantly higher than .0001. (This point is made in McGrew and McGrew 2009, p. 619)

Here, then, are the features of the analysis of ad hocness I propose for some auxiliary a' and some overarching theory H.

1) H is significantly disconfirmed by some new evidence E.

2) Either the only way or the only "decent" way for H to account for E is for H to be conjoined with a'. (Note that a' can be a disjunction of candidate auxiliaries, as illustrated in the Santa Claus example below.[5])

3) But a' has very low probability given H in the distribution prior to the acquisition of E and therefore would not have been considered were it not for the need to account for E.

4) ~H does not have to be conjoined with a' nor with another auxiliary which is similarly improbable given ~H in order to account for E.

This analysis applies quite well to various paradigm cases of ad hocness. In the Dianetics case, Dianetics itself gives us no reason to believe that subjects are "trapped in the present" by asking them to "come up here now." If Hubbard had thought that his theory had any such implication, he (of course) never would have used such a phrase to call his protégé to the stage in the first place. In other words, a' was not just extremely improbable prior to the experiment but was extremely improbable given D. Since ~D does not require a' nor anything similarly

[5] It is not necessary for there to be any particular connection among the disjuncts if a' is a disjunction, nor between a' and H. In fact, the more jumbled and disharmonious Ha' is, when ~H can easily explain E without resorting to such a messy combination, the more obvious is the reason for the disconfirmation of H.

implausible in order to explain the outcome of the experiment, it does not suffer from a similar problem, and the evidence heavily favors ~D over D.

Horwich (1982, p. 106) discusses another putative case of *ad hoc* reasoning--the phlogiston theory of combustion. According to this theory, phlogiston escapes in the process of combustion. Unfortunately for the theory, Lavoisier's experiments showed that the products of combustion are heavier, not lighter, than the metal that has been burned. Advocates of the phlogiston theory developed the auxiliary hypothesis that phlogiston has "negative weight" as an attempted theory rescue. If we take Howson and Urbach's approach, we will focus on the low posterior probability of this auxiliary. But we can do even better than that by noting that, even if we were to believe that phlogiston exists and that phlogiston escapes in the process of combustion, we would have no previous reason to believe that phlogiston has negative weight. In other words, the negative weight proposition, which is a' in this example, has extremely low probability in the prior probability distribution *given the main theory it is meant to rescue*, and it would therefore not have been considered at all by the theory's proponents if they were not attempting to rescue the theory in the light of seriously disconfirming evidence.

Here we may notice that, as Horwich envisages may sometimes happen, it isn't entirely clear what the alternative would have been to the phlogiston theory of combustion. What, in clear and precise terms, is ~P--the assertion that the phlogiston theory of combustion is false? However, this does not seem to be a case where, for all that we can tell, the experimental evidence is just as improbable or even more improbable given ~P as it is given P. Here we are skirting the extremely difficult question of the catchall hypothesis, a problem noted by Wesley Salmon (1990, p. 191). But, although the judgement that the evidence of Lavoisier's experiments disconfirms the phlogiston theory may seem a bit subjective, there is this to be said in support of that judgement: If the phlogiston theory of combustion is false, we are free to consider the possibility that combustion involves *taking something on* rather than giving something off. That, of course, turns out to be exactly what is happening in combustion. Nothing like the negative weight hypothesis is *required* given the falsity of the phlogiston theory. The negation of the phlogiston theory therefore seems to be less constrained and to have more resources at its disposal for explaining the experimental results.

Howson and Urbach also give, as a paradigm example of (bad) *ad hoc* reasoning, this case from psychology.

> A number of so-called environmentalists put a low [IQ] score down primarily to poor social and educational conditions. However, this explanation ran into trouble when it was discovered that a large group of Eskimos, leading a feckless, poor, and drunken existence, scored very highly on IQ tests. The distinguished biologist Peter Medawar..., in an effort to deflect the difficulty away from the environmentalist thesis, explained the observation by saying that an "upbringing in an igloo gives just the right degree of cosiness, security and mutual contact to conduce to a good performance in intelligence tests." (Howson and Urbach 1989, p. 108)

On Howson and Urbach's analysis of ad hocness in the first edition, the most salient point here is the extreme posterior implausibility of Medawar's remarkable auxiliary about the intellectual value of growing up in an igloo. But what seems even more pertinent to the disconfirmation of Medawar's environmentalist approach is the fact that the proposition about the cosiness of igloos and IQ tests was initially extremely improbable given the overarching environmental thesis Medawar was apparently trying to rescue. Consider this version of the environmental hypothesis:

> EN: Performance on IQ tests is primarily or entirely a result of external conditions in a person's life and upbringing rather than a result of any innate or biological properties of the person.

It is fairly obvious that, if EN is true, factors like poverty and poor education should be expected to be far more relevant to IQ than growing up in an igloo. There is no reason whatsoever *prima facie* to believe Medawar's auxiliary if one believes EN to be true. Very much to the contrary: The proponent of EN definitely seems to have reason to expect that the Eskimos, given their poor environmental conditions, would perform poorly on the tests. If people coming from such a background perform well on IQ tests, this seems much more probable given ~EN than given EN.

The negation of EN, of course, has no need to postulate the extremely implausible auxiliary that growing up in a cosy igloo makes one able to do well on IQ tests, nor does it need to postulate any similarly implausible auxiliary. ~EN is the hypothesis (roughly) that innate or biological factors play more than an insignificant role in IQ. The evidence of the high performance of the Eskimos on the IQ tests straightforwardly supports a greater role for innate factors in IQ as measured by such tests than the role allowed by EN; hence, it disconfirms

My suggested modification of the Bayesian account can also account well for garden-variety cases of ad hocness in ordinary life. Suppose that a child has been taught by his parents to believe in Santa Claus. One year, the observant child notices that the handwriting on the tags on the Christmas presents resembles his mother's. He points this out to his parents, who, thinking fast, tell him that Santa has mimicked his mother's handwriting in order to "throw him off the scent" and hide his, Santa's, involvement. This auxiliary is obviously quite improbable given that Santa exists. Why would Santa deliberately hide his involvement? And why, if he did so, would he specifically try to make the handwriting look like that of the child's mother? Since everyone already "knows" (as far as the child can tell) about Santa, even encouraging children to write letters to him, Santa doesn't seem to be attempting to hide his presence otherwise. Yet it's difficult to see what else the parents could say if they wanted to maintain the Santa myth. Perhaps they might dismiss the resemblance as the sheerest coincidence. But then the child is likely to wonder: Of all the ways that Santa's handwriting could have appeared, why should it just coincidentally resemble his mother's? Neither of these auxiliaries (the "throw you off the scent" auxiliary nor the "Santa sometimes writes like your mother by chance" auxiliary), nor even their disjunction, is remotely probable in the prior distribution given the hypothesis that Santa exists and is the source of the presents. On the other hand, the negation of the Santa hypothesis is well able to account for the handwriting; if Santa does not exist, it is not at all unlikely that the child's parents are giving him Christmas presents and writing the labels.

The perturbations of the orbit of Uranus

The account I am advocating here can thus make sense of many paradigmatic cases in which we want to give the pejorative label "*ad hoc*" to the postulation of some auxiliary hypothesis. But what if the account is too strong? It would be a problem for this account if cases that seem intuitively not to have negative features (either formal or argumentative) were also labeled *ad hoc* using its criteria.

The most famous case for testing an account of ad hocness to see whether it is too strong is the perturbations of Uranus and the discovery of Neptune, which Strevens (2001, p. 533-4) calls an instance of a "glorious [theory] rescue."

The auxiliary hypothesis a' in this case is the proposition that a new planet exists at a particular location and with particular properties sufficient to produce the observed perturbations in the orbit of Uranus. The hypothesis of interest H is the theory of Newtonian mechanics with its various constants and laws.

It is true that, being quite specific, a' had a rather low prior probability given H, but, as I have already noted, that is not a problem all by itself. In fact, specific evidence will often require a fairly specific hypothesis to explain it on either H or ~H. A low probability of a' given H (in the prior distribution) results in significant disconfirmation of H only if ~H is significantly better able to explain the evidence. In the above examples, we can make an argument (though as noted, this argument is less clear in the case of the phlogiston theory) that ~H can account for E without using either a' or some other similarly implausible auxiliary.

What, then, were the resources of ~H--the falsehood of Newton's theory--in the case of the orbit of Uranus? In point of fact, almost no distinctly non-Newtonian theories were proposed to explain the discrepancies between the expected and the observed orbit.[6] The one non-Newtonian explanation considered seriously by someone at the time was that suggested by George Airy, the Astronomer Royal. Airy thought that the "law of force" might differ slightly, *contra* Newton, from the inverse square of the distance (Adams 1896, pp. xxx-xxxi). Apparently only Airy saw much to commend this theory; it was not widely investigated by scientists interested in the problem.[7]

Airy's theory should not be confused with ~H, of which it was only a narrow subhypothesis. Airy's theory itself was an auxiliary put forward in response to the evidence about the orbit of Uranus, and it was quite improbable given ~H. In order for it to account for the

[6] See Smart (1947, p. 302). Theories other than Airy's non-Newtonian proposal included the existence of a previously unobserved satellite of Uranus, the impact of a comet on Uranus around 1781, and the existence of a resisting medium. All of these are as compatible with Newtonian theory generally as the hypothesis of a new planet.
[7] It is possible that Airy insisted so vociferously that a correction of Newton had been a reasonable hypothesis partly because he was in a defensive frame of mind. His dilatoriness in following up on John Adams's suggested position of a new planet had resulted in LeVerrier's being credited with the discovery of Neptune, and Airy was sharply criticized by his fellow Englishmen for mishandling the whole sequence of events (Smart 1947, pp. 307ff).

actual perturbations in the orbit of Uranus as well as the hypothesis of a new planet could account for them, the "law of force" must be hypothesized to differ by a highly specific amount, just as the hypothesis of a new planet had to attribute a specific location and attributes to the planet. This is often the case: Specific new evidence may require a fairly specific explanation given either H or ~H. There is no reason to believe that Newtonian physics was significantly disconfirmed by the perturbations of Uranus, because there is no reason to think that Newtonian physics lacked resources for explaining the evidence which were available to its negation.

A related point is that the presumption that there were no more planets was by no means an integral part of Newtonian theory. It was, rather, a highly defeasible empirical assumption arising from the mere fact that thus far there had been no reason to hypothesize more planets. In the absence of empirical evidence for another planet, Occam's Razor, not Newton's laws, led scientists not to multiply planets without necessity. (See footnote 1.) The low probability of a' given H in the prior distribution was a result entirely of the needed specificity of a', not a result of a theoretical tension between E and H or between a' and H. It is when there is a tension arising *from the theory H* itself that we naturally think that E significantly disconfirms H, since presumably ~H does not have the same disability.

We can contrast here the *ad hoc* attempt, discussed above, to save the strong environmentalist theory regarding IQ. There the environmentalist should, because of his theoretical commitments, consider poor and ill-educated Eskimos to be *prima facie* disfavored for good performance on IQ tests, contrary to the "cosiness of igloos" auxiliary adopted in response to the actual evidence of the tests. Because the low prior probability of E given H (and hence the low probability of a' given H) is a result of the actual theoretical implications of H itself, the Bayes factor for E strongly favors ~H, which is not burdened by those theoretical commitments.

My account of ad hocness therefore does not lead us either to think that the perturbations of Uranus significantly disconfirmed Newtonian physics nor to treat the hypothesis of a new planet as *ad hoc*. Since the account also handles well those cases in which we are pre-theoretically inclined to think that it is *ad hoc* to postulate the auxiliary, I conclude that my elaborated Bayesian account is neither too weak nor too strong in relation to paradigmatic cases.

The unnecessary condition in the standard Bayesian account

At this point, however, what has become of Howson and Urbach's central condition for bad ad hocness--namely, that the posterior probability of a' is less than or equal to .5? Once we have elaborated the account as I have suggested, is it still important or helpful to include that condition in our account?

The .5 condition does have some counterintuitive consequences. With that condition in place, H is not only disconfirmed, even significantly disconfirmed, by E in a case where a' is *ad hoc*; it is disconfirmed by E to such an extent that it has no greater posterior probability than .5. As Howson and Urbach explain,

> On this view, a' will be judged adversely and pejoratively labelled ad hoc, if $P(a'|e' \& b) \leq 0.5$, where e' is the new evidence that refuted the predecessor of a' and b is any other relevant information....Scientists are also interested in whether t in the presence of the newly-thought-up a' provides a competent explanation of the previously anomalous e'. It would do so only if t & a' was a sufficiently credible theory; since $P(t \& a'|e' \& b) \leq P(a'|e' \& b)$, this would be the case only if a' were not ad hoc. (Howson and Urbach 1989, p. 110)

In other words, if a' is judged *ad hoc* using the .5 condition, it follows that H (which has now been modified by being conjoined with a') also has a posterior probability less than or equal to .5. But whether or not H has a posterior probability less than or equal to .5 cannot in the nature of Bayesian analysis depend solely on the evidence for or against a'. Why could it not be the case that the postulation of a' is intuitively *ad hoc* but that the negative effect of E is insufficient to bring the posterior of H (even conjoined with a') down to .5 or below? If we assume that H must be conjoined with a' in the posterior distribution, and if the posterior probability of H is greater than .5, it does follow that the posterior probability of a' must also be greater than .5. Since a' was improbable in the prior distribution, this means that E confirms a'. But that might in fact be the case. Just as the conjunction (Ha') can be no more probable than its conjunct a', H can can also "pull a' up" in the transition to a new distribution induced by conditionalizing on E. If H itself had an enormously high prior probability on independent grounds, and if a' were the only way for H to account for E, that very fact could significantly raise the probability of a', however antecedently implausible it may have been.

What this means is that if we adopt the .5 condition we cannot fully evaluate even a paradigm case such as the Dianetics story without taking into account the prior probability of H. If Dianetics were enormously well-attested otherwise, then even the previously vastly implausible a' might have a posterior probability greater than .5 in virtue of the fact that it is the only way in which Dianetics can account for the adverse evidence. For reasons we have already discussed, the core theory of Dianetics was significantly disconfirmed by the need to wed it to a'. But "significant" disconfirmation can be defined reasonably enough in terms of orders of magnitude. If H has a prior probability of .999, is disconfirmed by E, and has a posterior probability of .9, it has suffered significant disconfirmation while remaining comfortably probable in absolute terms in the posterior distribution. We would be blocked from calling the postulation of a' *ad hoc* by the .5 criterion if only the prior probability of D, on independent grounds, happened to be high enough.

An analysis of ad hocness seems problematic if it entails that our evaluation of the ad hocness in Hubbard's move in the Dianetics case depends on the question, "How much other evidence did Hubbard have for the truth of Dianetics?" To put it differently, it does not seem correct for us to base our evaluation of his move as *ad hoc* or of the "frozen in time" auxiliary as *ad hoc* on our tacit assumption that Dianetics was an extremely silly theory to begin with, prior to the failure of the memory demonstration. Hubbard's presentation of the "frozen in the present" auxiliary seems *ad hoc* in and of itself, regardless of the prior probability of Dianetics.[8]

If we retain the .5 condition that Howson and Urbach suggest, this also has a counterintuitive consequence regarding cumulative disconfirmation. Suppose that some theory H must be modified by an auxiliary a', which has low probability given H in the prior distribution, to account for some piece of adverse evidence E1, and that conditioning on E1 results in the disconfirmation of H as discussed above. But suppose that the resulting conjunction Ha' is more probable than .5 after conditioning on E1, because H had a very high prior probability. Now suppose that some other piece of evidence E2 can be accounted for only by a similar move using another implausible auxiliary a", which results in further disconfirmation, and so forth, until H is no more probable than .5 as a result of conditioning on an entire set of pieces of evidence. It seems counterintuitive to insist that neither a' nor a" nor a"' can be considered *ad hoc* at the time

[8] This criticism is also relevant to Strevens's account (2001, p. 535), which ties the evaluation of a "desperate rescue" to the prior probability of H and its relation to the prior probability of an auxiliary a. (See footnote 1.) It seems possible that H might happen to have sufficient independent evidence in its favor to have as high a prior as an auxiliary a (not necessarily a sub-hypothesis of H), yet still suffer significant disconfirmation if $P(E|\sim H) >> P(E|H)$.

that they are first used, but that we can immediately see a'''' to be *ad hoc* because it happens to be the last in the series that drags everything down below .5. It seems that we should be able to evaluate each of these auxiliaries immediately as *ad hoc* in the same sense--namely, in the sense that Horwich describes of not being a "natural element," not "harmonis[ing] well" with the core theory and with the other elements, and producing a whole which is "contrived and complex." In fact, some of the auxiliaries adopted earlier might be even *more* obviously *ad hoc* than a''''. The need to join H to them to account for the evidence might have resulted in a greater degree of disconfirmation for H, even if H (and, hence, that particular auxiliary) happened to be more probable than .5 after that particular piece of evidence was taken into account.

In such a case it is the cumulative effect of joining H to various antecedently implausible auxiliaries that eventually drags the probability of H (with its gaggle of auxiliaries) down to the point that it cannot any longer be rationally believed. But we should be able to see, and to say, that the auxiliaries are *ad hoc* at earlier stages in the process. There should not be something special or different, in terms of bad ad hocness, about just that point where H finally succumbs and sinks below the crucial .5 line.

The .5 condition in Howson and Urbach's concept of bad ad hocness is therefore unnecessary. Paradigm cases of ad hocness can be better captured by abandoning that requirement and emphasizing instead, in formal terms, the set of conditions given above.

<u>Not counting the cost</u>

The purpose of introducing an *ad hoc* hypothesis is often said to be "saving" a theory. (See, e.g., Hempel 1966, pp. 29-30 and Horwich 1982, pp. 105-6.) Strevens (2001 p. 533) says that the central hypothesis H can be "rescued from falsification" by the introduction of a new auxiliary a'. Scott (2007, p. 155) goes so far as to say that the acceptance of the potentially *ad hoc* hypothesis renders the evidence "no longer disconfirming."

If, however, we take it to be of the essence of desperate *ad hoc* theory rescues that evidence E *does* disconfirm H, talk of "saving" and "rescuing" the theory becomes rather puzzling. We can make some sense of such language if we understand "falsification" in a Popperian sense to mean, strictly speaking, that the falsehood of some proposition or conjunction (H, or H in conjunction with its "old" set of auxiliaries) is *entailed* by the evidence. H may be rescued from strict falsification in this sense if we can show that there exists some auxiliary a' that, in conjunction with H, would give the evidence high probability. Still, this seems a rather pyrrhic victory if H is nonetheless severely disconfirmed by the evidence. It seems that the "rescue" of H has not been very successful.

It is at this point that we need to distinguish the concept of ad hocness as a formal, probabilistic notion applied to an auxiliary hypothesis or to the conjunction of an auxiliary with a main theory from ad hocness as a concept that applies to an *argumentative* move made by a reasoner or theory advocate. The formal or probabilistic notion of ad hocness can be spelled out as I have suggested above. The designation of the auxiliary as *ad hoc* in this case is pejorative entirely for probabilistic reasons--because the need for H to be conjoined with a' is probabilistically unfortunate for H. The resulting conjunction lacks internal cohesion and is significantly less probable than H was when it had no need for a'.

If we think of ad hocness in terms of "theory rescue," however, we gain insight into the sort of *argumentative* move that we should designate pejoratively as *ad hoc*. In this sense, an *ad* *hoc* move reflects negatively on either the reasoning powers or the intellectual honesty of the person making that move. Argumentative ad hocness should be understood as either the reasoner's being irrational or his attempt to induce others to be irrational, and possibly both. The irrationality, in the case of *ad hoc* argumentation, takes the form of not acknowledging the disconfirmation suffered by the theory.

A theory advocate who makes an *ad hoc* move appears to think that he actually has rescued his theory. When L. Ron Hubbard postulated his a' regarding being "frozen in the present," he was *not* saying, "I admit that Dianetics has suffered serious disconfirmation from the failure of this demonstration. However, I suggest that you not entirely give up on Dianetics just yet. Let us consider the admittedly antecedently implausible hypothesis that I accidentally froze my protégé in the present by using the word 'now' in my introduction. Since Dianetics has so much other evidence going for it, perhaps this is what actually happened." On the contrary, the idea he wished to convey was that he had taken care of the negative evidence merely by putting forward the auxiliary, that the evidence was now no longer a serious problem, and that anyone who had previously been inclined to believe in Dianetics should go on doing so as strongly, or very nearly as strongly, as before. The same seems to be true whenever we say pejoratively of a reasoner that he has "made an *ad hoc* argument." The problem is that the reasoner seems to be implying, merely on the grounds that he has come up with *some* auxiliary that can account for the evidence, that the evidence is not a serious problem for his theory We are left with the dissatisfying impression that the reasoner has not really allowed his theory to "take the hit" which it objectively should take from the disconfirmatory evidence.

Argumentative ad hocness, then, is the use of an auxiliary hypothesis a' to assist a significantly disconfirmed central hypothesis H in such a way that one *avoids recognizing or admitting* the disconfirmation or the degree of disconfirmation to the central hypothesis H. The *ad hoc* reasoner does not properly conditionalize on the evidence, where correct conditionalization would result in H's having a significantly lower posterior than its prior. Instead, the *ad hoc* reasoner incorrectly retains the same or virtually the same probability for H as its prior. I call this *ad hoc* move "not counting the cost."[9]

Once we have firmly in place both the distinction between formal ad hocness and argumentative ad hocness and the concept of not counting the cost, we can understand the attraction of Howson and Urbach's .5 condition without actually adopting it. Howson and Urbach, particularly in the second edition of *Scientific Reasoning*, appear to be concerned not automatically to taint reasoners with the accusation of irrationality if they adopt an auxiliary a' in the face of some problematic evidence; after all, both H and a' might be more probable than .5 after E is taken into account. This appears to be why they chose to use the broader definition of "*ad hoc*" in the second edition.

> We have argued, contrary to the standard view, that a theory could be scientific and plausible even if it is ad hoc. An acceptable ad hoc theory is a possibility allowed for by the Bayesian principle that theories should be evaluated according to their probabilities. To illustrate, consider the ad hoc theory a', which we have supposed was put forward in response to some refuting evidence e'. The probability of this theory must be reckoned relative to e' and any other available relevant information, b. The probability calculus places no restrictions on the value of P(a'|e' & b); it might, for example, be below 0.5, so that a' would be more likely false than true, or greater than 0.5, when the reverse would be the case.

[9] Not counting the cost does not necessarily result in an inconsistent posterior probability distribution. The *ad hoc* reasoner may adjust all of his probabilities so that the new distribution is consistent, though it did not arise from conditionalizing on E using the probabilities in his prior distribution. It is not always rationally necessary to change one's probabilities using conditionalization or even Jeffrey conditioning, but it is necessary to have an evidential reason for not conditionalizing, such as the sudden disappearance of foundational evidence (L. McGrew 2010, pp. 96, 103 n.7). Such a rational motivation for not conditionalizing is lacking in the case of argumentative ad hocness.

> Hence a' does not need the support of new independent predictions in order to be quite plausible and acceptable. (Howson and Urbach 1993, p. 157).

Howson and Urbach here imply that ad hocness, defined very broadly, is *bad* ad hocness only if the probability of the auxiliary falls to or below .5 in the posterior distribution, making a' no longer "plausible and acceptable." I have already discussed above the problems with restricting our understanding even of formal bad ad hocness in this way, and those problems can be readily extended to the treatment of argumentative ad hocness. Even if a theory has a posterior probability above .5, a reasoner can misuse an auxiliary (whether knowingly or in genuine confusion) to cover or mask the degree to which E has disconfirmed H. The problem lies in the failure to count the cost and in the contribution of the *ad hoc* auxiliary to this failure, not *per se* in the fact that either H or a' has a posterior probability of less than or equal to .5. As with formal ad hocness, discussed above, argumentative ad hocness can arise at each of a number of steps as H is repeatedly disconfirmed, even if the first step does not cause H to fall to .5 or below.

Strevens (2001, p. 536) appears to acknowledge a kind of argumentative ad hocness when he refers to researchers who "cling to the central hypothesis" when this is obviously unreasonable. Even if one adopts Howson and Urbach's .5 criterion, one would presumably never accuse *anyone* of (bad) *ad hoc* reasoning if all theories that fall below a probability of .5 on total evidence were promptly abandoned and if all auxiliaries a' with a posterior probability below .5 were treated as unacceptable. *Ad hoc* reasoning actually occurs only if someone or other *adopts* the auxiliary a' and *hangs onto* the theory H when he should rationally no longer do so. It seems impossible, then, to get along without a distinct notion of argumentative ad hocness according to which some truth or other about the actual probabilistic situation is not acknowledged by a reasoner or a researcher who continues to act as though his preferred theory is better off than it really is.

Suppose that a' is formally *ad hoc* in the sense defined above and that H is therefore significantly disconfirmed by E. If a reasoner considers H to be so strongly supported by independent evidence that it can afford to "take the hit" of being forced into conjunction with the antecedently implausible a', and if he therefore adopts a' in response to E, he is admittedly adopting a theory that is formally *ad hoc*, but he is not *necessarily* behaving irrationally nor encouraging others to do so. As long as he fully realizes and admits the disconfirmation H has suffered, he may be doing something entirely reasonable in adopting a', though of course (if he wants to believe H true) he would prefer not to have to do it. The adoption of a' is in the formal sense a "bad thing" for theory H, because it means that H has been significantly disconfirmed by E. But it need not be a "bad thing" in terms of rational theory-making. We can therefore say that the adoption of a formally *ad hoc* theory is always "bad" in *some sense*--that is, that it is always unfortunate from the perspective of an advocate of a theory H--without saying that it is always unreasonable.

Conclusion

The concept of not counting the cost has a number of implications for further research. For example, it allows us to recast in Bayesian terms a generally (or perhaps only vaguely) Popperian concern about unfalsifiability. If a reasoner is not counting the cost, not admitting or perhaps just not realizing the significant disconfirmation of his theory, and if this occurs because of his own facility in coming up with auxiliary hypotheses, there is reason to fear that he will do this repeatedly in the face of additional negative evidence. There is no reason in principle why he should not continue to spin out *ad hoc* auxiliaries and not count the cost in response to further negative evidence if he considers it epistemically acceptable to do it once. In that case, of course H could eventually (after the accumulation of more and more disconfirming evidence) actually be far less probable than .5 on the subject's total evidence, if the subject were evaluating matters rationally, while the subject fails to realize or admit that his theory is no longer remotely tenable. In this scenario we could say that the subject has, in a Bayesian sense, made his theory unfalsifiable.

The introduction of a kind of Bayesian unfalsifiability (the iterated refusal to recognize disconfirmation) allows us to see the importance of counting the cost not only in science but also in philosophy. Consider, for example, the Cartesian deceiver scenario. Typically, the scenario which the realist has to meet is said to make things "just as if" external-world realism were true.[10] But in that case, it seems that the narrow focus on such a "piggy-back" version of deceiverism makes deceiverism unfalsifiable in the Bayesian sense that its advocate will never admit disconfirmation even when it occurs. Since our attention is kept entirely on the empirical equivalence between what one might call "perfect deceiverism" and realism, prior probabilities never come into the picture and the question of disconfirmation never arises. The "perfect deceiver" scenario could be a severely *ad hoc* version of deceiverism (more generally conceived), and we would never notice so long as the entire issue is seen as an exercise in the problem of empirical equivalence. The problem of the external world is, of course, too vast a subject to be dealt with within the scope of this paper, but it is interesting to note that the concept

[10] See, for example, Timothy McGrew's argument for realism against Cartesian skepticism based on simplicity considerations. McGrew takes it for granted that the realist must answer an argument that treats deceiverism as

of not counting the cost can lead to further research in classical epistemology as well as in science and philosophy of science.[11]

Disconfirmation is the common theme between formal ad hocness as defined here and argumentative ad hocness. The account of formal ad hocness given here both defines formal ad hocness as including the disconfirmation of H and explains why H is disconfirmed. In argumentative ad hocness we see the circumstances in which formal ad hocness is accompanied by unreasonable argument on the part of an actual subject. Both concepts can help to focus further discussion in cases where ad hocness is alleged. The pair of concepts together illustrates the importance of having an epistemic approach that allows us to assess accurately the impact of disconfirming evidence for favored theories. If we are to keep our epistemic vision clear, we must always be willing and able to count the cost.[12]

empirically equivalent to realism. (McGrew 1995, p. 135)

[11] A similar question can be raised about the multiverse hypothesis. Apropos of (among other things) the multiverse, John Leslie (1989, p. 194) says that "strong evidence for something" is "whatever causes a puzzlement which the existence of that something would reduce or remove." But that description could be satisfied by an *ad hoc* hypothesis. See L. McGrew (2005).

[12] I am indebted to Timothy McGrew for comments on an earlier version of this paper and for help in my research on the perturbations of the orbit of Neptune.

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