Ad hoc hypotheses: a historico-philosophical analysis

Abstract

What does it mean for a hypothesis to be *ad hoc*? Although seemingly straightforward, *ad hoc* judgements by scientists exhibit a surprising complexity. This paper will critically review extant accounts of *ad hoc*ness and advance a new account that better accommodates the discussed historical cases.

**Keywords:** *ad hoc*, *ad hoc*ness, empirical success, coherence, unification, method

1 Introduction

It is widely agreed—amongst scientists and philosophers alike—that a good hypothesis ought not to be *ad hoc*. Consequently a theory that requires *ad hoc* hypotheses in order to overcome empirical tests is less likely to be adopted by rational agents than a theory that does without (or with fewer) *ad hoc* amendments. Put differently, whether or not a hypothesis is *ad hoc* will affect the degree of confirmation of the theory it is invoked to save: a theory that is amended with *ad hoc* hypotheses will receive less confirmation from evidence that it thereby accommodates than a theory that does without any such amendments. But what does it exactly mean in *epistemic* terms for a theory to be *ad hoc*? What is it that is somehow epistemically defective about *ad hoc* hypotheses? This is what this article is about. Whilst the main focus of the article will be on science, and particularly physics, the conclusions of this paper should be interesting to anyone interested in the meaning of *ad hoc*ness and confirmation more generally.¹

Intuitively, a hypothesis is ‘*ad hoc*’, minimally, when it was introduced for the sole purpose of ‘saving’ a theory which faces observational anomalies. Whilst this definition may suffice for all practical purposes, it is not enough for anybody looking for an *epistemic* account. What is it about a hypothesis that somehow makes it less deserving of confirmation than a hypothesis that

¹ The nominalisation of the adjective *ad hoc* is a little awkward. Alternatively *ad hoc*kery has also been used, which, I think, is more appropriate for the *process* of concocting *ad hoc* hypotheses.
accommodates the same evidence in a non-\textit{ad hoc} fashion? The above intuitive definition gives us no answer to this question. It only tells us something about \textit{why} \textit{ad hoc} hypotheses are used; it tells us about motivations.

Philosophers of science were once very interested in specifying the epistemic meaning of \textit{ad hoc}-ness; mostly in the 1960-1980s. The philosopher with arguably the biggest impact outside the discipline, Karl Popper, was very much driven by the attempt to formulate conditions that would disqualify as unscientific the \textit{ad hoc} maneuvers of psychoanalysis and other alleged pseudosciences.\textsuperscript{2} His student Imre Lakatos deemed research programmes degenerative when they kept making \textit{ad hoc} modifications whilst ceasing to successfully predict new phenomena. According to Lakatos’s student John Worrall \textit{ad hoc}ness and novel success are just two sides of the same coin: “it is wrong to regard the downgrading of \textit{ad hoc} explanations and the apparent upgrading of genuine predictions as two separate methodological phenomena—they are at root the same phenomena” (148). Curiously, although novel success plays a major role in the contemporary realism debate (e.g. Psillos 1999), there is little, if any engagement with the meaning of \textit{ad hoc}ness.

Traditionally, discussions of \textit{ad hoc}ness have been informed by historical case studies. The present article will follow this tradition. There might be other ways of investigating the meaning of \textit{ad hoc}ness by descriptive means (interviews with living scientists?), but the history of science offers one established way of seeking descriptive accuracy of philosophical accounts. And seeking such descriptive accuracy, we shall see, does offer problems and insights which one might not expect when philosophizing in the armchair only.\textsuperscript{3} At the same time pure historical description of \textit{ad hoc} judgements as made by scientists will not suffice either. Scientists generally do not explicate what grounds their judgements; they just make them. Also, they may not always be very explicit when condemning hypotheses as \textit{ad hoc}. They may not use the word ‘\textit{ad hoc}’ but other normative language condemning hypotheses as ‘artificial’, ‘concocted’, and ‘contrived’. Thus, some substantive philosophical

\textsuperscript{2} See e.g. Popper (1959a, 42). Lakatos (1978) once characterised Popper’s falsificationism as
“introducing new, non-justificationist criteria for appraising scientific theories based on anti-\textit{ad hoc}ness.” (39)

\textsuperscript{3} This paper is written in the tradition that seeks to refine pre-analytic scientific concepts by means of the history of science [cf. Schickore, 2011 #303].
work is indispensable in the project of determining the meaning of *ad hoc*ness by descriptive means.

The structure of this paper is as follows. In Section 2 I will lay out the state of the art of regarding philosophical accounts of *ad hoc*ness. In Section 3 I propose my own view of *ad hoc*ness and explain how it accommodates the historical cases better than alternative accounts. Section 4 concludes this article.

## 2 *Ad hoc*ness: the state of the art

This section discusses four accounts: *ad hoc*ness as a lack of independent testability/support (2.1), as lack of unifiedness (2.2), as parameter-fixing (2.3), and as subjectivist / aesthetic judgements (2.4).

### 2.1 *Ad hoc*ness and independent testability / support

Popper believed that *ad hoc* hypotheses would decrease the falsifiability of the theory they were introduced to save (Popper 1959a, 82f.) and that “degrees of *ad hoc*ness are related (inversely) to degrees of testability and significance” (Popper 1959b). If a hypothesis which is added to a theory entails only the evidence that is troublesome to this theory, then the falsifiability of the theory is decreased: there is one state of affairs less that could threaten the theory in question. On the other hand, the falsifiability of the theory is increased when the added hypothesis makes new predictions. This is why Popper also claimed that *ad hoc* hypotheses “cannot be tested independently” (1974, 986), i.e., they cannot be tested other than on the basis of the evidence they were introduced to account for. At least this is the idea. Barnes (2008, 11) has, I think rightly, questioned Popper’s alleged connection between *ad hoc* hypotheses and decreased testability. For example, it is for example not plausible that the hypothesis “bread nourishes” when amended to “all bread nourishes except that grown in a particular region of France” after the relevant discovery, would result in a decrease in testability.

Independent testability also seems not the right way of thinking about one of the most emblematic *ad hoc* hypotheses. As Grünbaum (1959) pointed out, the Lorentz-FitzGerald contraction hypothesis (LFC), introduced to save the aether theory from refutation in the face of the famous Michelson and Morley aether drift null result, contra Popper, did indeed make an
independently testable prediction: it entailed a positive aether drift result that was different from the positive result implied by the unmodified aether theory. There even was an experiment, the so-called Kennedy-Thorndyke experiment, which was apt to falsify this consequence of the LFC. Still, there is strong evidence that contemporary scientists regarded the LFC as an ad hoc hypothesis. Einstein, for instance, wrote about LFC that “[t]his manner of theoretically trying to do justice to experiments with negative results through ad hoc contrived hypotheses is highly unsatisfactory” (Einstein in Warburg 1915, 707). To Michelson, who was initially actually quite keen to explain away his null result in favor of the ether theory felt that "such a hypothesis seems rather artificial" (cited in Holton 1969, 139). Even Lorentz, who had developed the LFC, admitted that it first appeared ‘far-fetched’ and that it depended on an assumption (namely that the same laws apply to intermolecular forces that apply to electrical ones) which ‘there is no reason to make’ (Lorentz 1895 in Einstein et al. 1952, 6). In response to criticism by Poincare in 1900, Lorentz also admitted that “[surely] this course of inventing special hypotheses for each new experimental result is somewhat artificial” (Lorentz 1904, ibid., 12-13). Given that the LFC was independently testable, but nevertheless, clearly judged ad hoc even by those proposing it, there are thus good grounds for the lack of independent testability not being a mark of ad hoc hypotheses. Even Popper himself was willing to admit that much (Popper 1959b). The conclusion Grünbaum (1976) drew was this: an ad hoc hypothesis can have independent consequences, but when it does, these consequences are neither supported nor unsupported (because no relevant evidence has been gathered), or they have been shown to be false at the time of the hypothesis is judged ad hoc.

The idea that a hypothesis needs independent support for it to not be judged ad hoc is extremely popular amongst philosophers (e.g. Zahar 1973; Schaffner 1974; Leplin 1975; Lakatos et al. 1978; Scerri and Worrall 2001; Worrall 2002; Sober 2008). But it is riddled with problems. How are we supposed to

---

4 The LFC had it that matter moving through the ether contracts in the direction of motion by the same laws governing the contraction of interference patterns. See for example (Janssen 2002b), footnote on p. 433, for the difference between the Michelson-Morley and the Kennedy-Thorndyke experiments and the respective ether theory predictions.

5 Lakatos (1978) distinguished between three senses of ad hoc hypotheses (added to a research programme): $ad\ hoc_1 =$ the hypothesis makes no new predictions; $ad\ hoc_2 =$ the predictions a made by a hypothesis are not confirmed; $ad\ hoc_3 =$ the hypothesis does not is “not form an integral part of the positive heuristics” (fn 1 on p. 112). Ad hoc$_3$ for Lakatos was ‘synonymous’ with ‘empirical’ or ‘formal’
think about hypotheses that are introduced to save a theory, make independent predictions, but whose predictions are not successfully refuted? Does a hypothesis come *ad hoc* only at the moment we find refuting evidence? That seems barely plausible. What about hypotheses for which it is hard or even impossible to find refuting evidence? Can those not be *ad hoc*? Is the hypothesis *ad hoc* even before we find refutation (because we introduced it to save the theory)? If so, then what role does the refutation play at all for the meaning of *ad hoc*ness? Or is the status of the hypothesis in question then not determinable for the time we need to wait until we manage to refute the hypothesis? That is strange as well: the introduction of a hypothesis to save a theory is clearly part of what we mean when we say a hypothesis is *ad hoc*. Incidentally, with regards to our example of the contraction hypothesis, the historian Holton remarks in his wonderful analysis of the impact of the Michelson-Morley experiment that “[t]hese other applications [i.e., predictions] of the *ad hoc* hypothesis [i.e., the contraction hypothesis] are not of real interest in any case; they were not urged as tests that would decide on its acceptability” (Holton 1969, 177). So perhaps there is something else about *ad hoc* hypotheses that is deficient, epistemologically speaking.

The independent support view of *ad hoc*ness leaves open the possibility that *ad hoc* hypotheses make independent predictions that are not refuted but confirmed. Indeed there is one example that often gets cited as illustration, namely the stipulation and discovery of the planet Neptune in 1846. Neptune was stipulated after an irregularity in the orbit of Uranus was observed; Uranus did not behave in exact the same way predicted by Newtonian mechanics. So, seemingly, the hypothesis of there being another planet (Neptune) in the vicinity of the planet Uranus was invoked to save Newtonian mechanics from refutation. Proponents of the independent support view of *ad hoc*ness have taken this to confirm their view (Worrall 2002): the Neptune hypothesis was introduced *ad hoc*, but was rendered non-*ad hoc* when it received independent confirmation. As Leplin (1982) has pointed out, however, that there is no evidence whatsoever that the postulation of Uranus was considered *ad hoc*, or

---

(fn 2 on p. 88). The first and the second sense are covered by my discussion in this section. A discussion of the third sense would lead us too far astray.

6 Some believe that the postulation of dark energy is *ad hoc*. It’s extremely hard to come by.
somehow methodologically unsound, by the scientific community. If the postulation and discovery of Uranus really is no example for an *ad hoc* hypothesis, as determined by the judgements of the scientific community, then it cannot lend credence to the independent support condition of *ad hoc*ness, or to any account of *ad hoc*ness for that matter. Moreover, it can then also not be the case that introducing a hypothesis to save a theory from refutation is sufficient for it that hypothesis to be *ad hoc*. Something else is needed.

The independent support view of *ad hoc*ness is very persistent though. In the most recent contribution on the topic of *ad hoc*ness, Friederich et al. (2014) discuss several judgments by physicists (and some philosophers) about the Higgs mechanism (HM) of the standard model in particle physics being *ad hoc*. Friederich at el. list the following features of the HM that arguably underlie physicists' *ad hoc* judgments: (i) the HM leads to a large number of free parameters for the particle masses that are not determined in a principled fashion but need to be put in 'by hand' on the basis of experimental results, (ii) there are no known fundamental scalar particles in physics apart from the Higgs boson, (iii) the symmetry breaking of the HM, contrary to all other known cases of symmetry breaking, is implemented not dynamically but by fiat, and (iv) the fact that the bare mass of the Higgs particle is ‘fine-tuned’ to its interaction mass (by 34 orders of magnitude) is “unexplained”. This is known as the ‘fine tuning’ or ‘naturalness’ problem and, according to Friederich et al, “is widely regarded as the most severe” argument against the “fundamental scalar character” of the Higgs field. In other words, the HM has a number of

---

7 See for example Grant (1852, 164-201) and Grosser (1962). Bamford (1996), similarly, notes that “[t]here was no theoretical objection, however, to a planet located at some intermediate distance beyond Uranus” (216).

8 Incidentally, it is interesting to note with Leplin (1975, 314, fn. 17) that Lorentz appears to have regarded the question of whether or not the LFC was *ad hoc* independently of the question of whether or not there ever could be (positive) experimental tests for it Lorentz (1885 in Einstein et al. 1952, 6). We shall return to this point later in the paper (cf. Section 3.1.2).

9 Symmetry breaking normally is due to composite rather than fundamental fields. Only in the former case can it be dynamical.

10 In quantum field theory any experimentally determined particle mass is understood as the sum of the ‘bare’ mass and the ‘interaction mass’, due to the interaction of the particle with vacuum fluctuations.

11 Friederich et al. mention two further points: that a non-zero Higgs field in a vacuum is conceptually problematic and ‘triviality’. The first of these points, Friederich et al. argue, makes a false presupposition: the Higgs field in the vacuum actually is zero. The second concerns the position of the so-called “Landau pole”, basically a limit of the standard model to certain energy scales. The Landau pole for the standard model depends on the observed Higgs mass. For the currently observed Higgs mass, the Landau pole turns out to be entirely absent. This result, Friederich et al. point out, can be generated only “if the self-coupling of the Higgs boson is assumed to be vanishing, i.e. trivial, while this self-coupling must be non-
arbitrary features. Interestingly this is also what Weinberg, in his original paper, expressed when he conceded that “[…] our model has too many arbitrary features for these predictions [regarding the electroweak “mixing angle”]" to be taken very seriously” (Weinberg 1967, 1265-66). It is this aspect of arbitrariness that appears to underlie physicists’ judgment that HM is ad hoc.

Friederich et al. conclude their analysis by saying that the Higgs mechanism is no longer to be considered an ad hoc hypothesis because “the most crucial characteristic of an ad hoc hypothesis [namely the condition of independent support] […] is no longer obeyed”; “the ad hoc character of the [HM] therefore has ended after the recent discovery of a Higgs-like particle” (p. 3913). As evidence for this claim Friederich et al. cite the fact that “many physicists […] now seem to be ready to accept the [HM] as part of physical reality” (ibid.). We note that that this conclusion presupposes what Friederich et al. set out to argue for: that an ad hoc hypothesis is one whose independent predictions are not supported. Indeed, as Friederich et al. astutely point out themselves none of these issues is resolved by the recent discovery of the Higgs particle, which is why “most of them [i.e., most physicists] seem to be not ready to conclude that the criticisms of the [HM] were unfounded” (ibid.). But then, it seems that the ad hoc judgements regarding the Higgs mechanism have simply nothing to do with independent support; they can thus lend no credence to the independent support account of ad hocness.

2.2 Ad hocness as lack of unifiedness

One of the most systematic and perhaps most influential accounts of ad hocness is the one by Leplin (1975). Leplin’s account is complex. It contains no less than five detailed individually necessary and jointly sufficient conditions for ad hocness (336-7):

1. An hypothesis H introduced into a theory T in response to an experimental result E is ad hoc if and only if:
   a. E is anomalous for T but not for T as supplemented by H.
   2. E is evidence for H but

vanishing for the HM to generate non-vanishing particle masses”. Unfortunately it remains a little unclear in Friederich et al. ‘s discussion, why physicists consider this ad hoc (if at all).

12 The mixing angle of the Weinberg model, which also became known as Weinberg angle, indicates the relative strengths of the neutral and charged weak interactions and the masses of the Z and W bosons.

13 Instead of HM Friederich et al. use the abbreviation SMHM for Standard Model Higgs Mechanism.
a. no available experimental results other than E support H,
b. H has no application to the domain of T apart from E,
c. H has no independent theoretical support.
3. There are sufficient grounds neither for holding that H is true nor for holding that H is false.
4. H is consistent with accepted theory and with the essential propositions of T.
5. There are problems other than E confronting T which there is good reason to hold are connected with E in the following respects:
   a. these problems together with E indicate that T is non-fundamental,
   b. none of these problems including E can be satisfactorily solved unless this non-fundamentality is removed,
   c. a satisfactory solution to any of these problems including E must contribute to the solution of the others.

Leplin believes that all ad hoc hypotheses in science are introduced in response to an experimental anomaly and therefore restricts his analysis accordingly with condition (1), i.e., the condition of experimental anomaly. As we’ve seen in the previous section, this condition has been challenged. Interestingly Leplin does allow for this condition to be removed should cases of ad hocness be found that suggest the inadequacy of this condition (336). Condition (2), Leplin’s condition of justification, captures the intuition that ad hoc hypotheses have no other support than the one they are designed to accommodate. Condition (3) is Leplin’s condition of tentativeness. Presumably, more evidence than E is needed for judging whether H is correct. The condition of tentativeness is thus closely related to the condition of justification: if there were more evidence than E, we wouldn’t have to be tentative about H. Condition (4), the condition of consistency, must be satisfied because otherwise the hypothesis in question would presumably simply be dismissed and not considered a serious candidate for amending T in the first place. Condition (5) is Leplin’s condition of non-fundamentality. It is probably the condition most unique to Leplin’s account. For Leplin, a theory is non-fundamental, “if no satisfactory solution of [phenomenon] P can be achieved without the rejection of propositions in [theory] T and their replacement by propositions inconsistent with T” (325). Yet there is something in this condition which is of more general appeal. What Leplin is suggesting, namely, is that the theory T that is being amended with a hypothesis H in order to accommodate P lacks the resources for unifying the set of interlinked problems that P is a part of. Thus, in Leplin’s view, the ad hoc charge is not so much directed against H but rather against T that H ought to
save. Leplin motivates this view primarily with the criticism of the Lorentz-FitzGerald contraction hypothesis particularly by Einstein as “directed primarily at the theory, and only indirectly at the particular hypotheses proposed as supplementation” (320).

There is an immediate problem with Leplin’s account, relating to condition (5): if there is a phenomenon E that the theory in question accommodates by invoking a hypothesis H—in a seemingly ad hoc fashion—then H cannot be ad hoc when there are no phenomena other than E that indicate the non-fundamentality of T. But that is highly counterintuitive. A plausible account of ad hocness, I take it, must allow for the possibility of a hypothesis or theory to be ad hoc with regard to a single phenomenon. Whether a theory accommodates other phenomena in an ad hoc fashion as well, should not affect the question of whether or not the theory accommodates the phenomenon in question in an ad hoc fashion.

The idea that ad hoc charges have got something to do with lacking unifiedness has currency also among other philosophers. Boudry and Leuridan (2011) in their criticism of (Sober 2008), for example, claim that Paley’s intelligent design hypothesis is objectionable not only because it has no independent support (as Sober claims), but also because it fails to unify the phenomena in a simple way (570). Lipton (1991/2004), too, can be said to defend a view that associates ad hocness with a lack of unification (cf. Harker 2008).

For Lipton, when data are accommodated rather than predicted, “there is a motive to force a theory and auxiliaries to make the accommodation. The scientist knows the answer she must get, and she does whatever it takes to get it. The result may be an unnatural choice or modification of the theory and auxiliaries that results in a relatively poor explanation and so weak support […]” (170). In the case of prediction, in contrast, the scientist will “make her prediction on the basis of the most natural and most explanatory theory and auxiliaries she can produce” (ibid.). Whilst a scientist not necessarily makes an ‘unnatural’ modification of the theory when she accommodates (rather than predicts) data, according to Lipton, there is ‘reason to believe’ that a theory is being ‘fudged’ (as Lipton also likes to refer to ‘unnatural’ modifications) when a theory accommodates data. There is no such reason, according to Lipton, when the theory predicts data. Such reasons have special prominence in Lipton’s
account, because, for Lipton, “inductive support is translucent, not transparent” (178). Even the scientist proposing a theory “should not assume that she is not doing [fudging] just because she is not aware that she is” (179).

It’s not really clear what Lipton means by a theory being ‘natural’, and conversely, what he means by a theory being modified unnaturally or ‘fudged’. He even admits himself that it is a “clear limitation” of his account “that it does not include anything like a precise characterization of the features that make one theoretical system fudgier than another” (180). He does however briefly mention that, for him, a fudged theory “becomes more like an arbitrary conjunction, less like a unified theory” (171).

Also Harker (2008) is sympathetic to the idea of the unification of the phenomena driving the relative importance of predictive over accommodative success. More specifically, Harker claims that predictive success is apt to impress us only by virtue of the theory in question, when successfully predicting a phenomenon E, at the same time unifying E with other phenomena without having to invoke any further assumptions (Harker 2008). A theory’s verified forecasts, as Harker calls successful predictions, are of course also explananda of that theory. And what scientists appreciate about verified forecasts is not so much the fact that the evidence in question was successfully predicted, but rather the fact that the evidence was explained in a simple and unified way with the resources of the theory. For example, in the case of Fresnel’s successful prediction of the famous white spot (Worrall 1989) allegedly “no additional assumptions were required” (Harker 2008, p. 447). Fresnel’s theory, after this successful prediction, “enjoys increased explanatory strength without any loss of theoretical simplicity” (ibid). According to Harker, there is therefore no difference in epistemic import between accommodations and successful predictions, as long as the phenomena are explained in a simple and unified way. Conversely, both accommodations and successful predictions will be of low epistemic quality if numerous assumptions have to be invoked for each phenomenon to be explained. Although Harker does not explicitly link the lack of unification to ad hocness, he does offer his account as an explanation

14 Oddly enough, Lipton mentions the advance in the perihelion of Mercury as a prediction by Einstein’s theory of special relativity (170). Usually that example is regarded as an example for an explanation rather than a prediction (Worrall 1989; Brush 1994).
of the apparent appeal of predictive success (relative to accommodative success) (450).

I doubt that Harker’s insistence on unifying theories not invoking “additional assumptions” when incorporating new phenomena is sustainable. From Duhem we know that theories are never tested in isolation; tests of theories always depend on a host of auxiliary and background assumptions. And particularly in our attempts to confirm newly predicted phenomena we will have to invoke a number of “additional assumptions”, when designing new experiments and when using new instruments. Furthermore, whether or not, for example, Einstein’s theory makes do with fewer assumptions than Lorentz’s in accommodating the Michelson-Morley experiment (as Harker would require), I take it, is not obvious and probably not easily determinable. Yet that does not seem to matter to judgements about the LFC being ad hoc. I am therefore skeptical that operating with the number of assumptions will help us illuminate the notion of ad hocness.

Lastly, Lange (2001) defends a view according to which predictions count more than accommodations because, when evidence is accommodated, “it is possible for the resulting hypothesis to be an arbitrary conjunction”, whereas it is “exceedingly unlikely” that an arbitrary conjunction is proposed before the relevant evidence is in (ibid., in particular pp. 583-4; added emphasis). Predictions are therefore prima facie more trustworthy than accommodations. With arbitrary conjuncts, “apart from the evidence being accommodated, there is no motivation for fastening onto that particular hypothesis rather than onto one with different conjuncts” (584; added emphasis). And because an arbitrary conjunct is “prompted by little in the way of theoretical considerations”, an arbitrary conjunct being true is “likely to be utterly coincidental rather than to possess some physical significance” (ibid.; added emphasis). This is why in Lange’s view, for example, the Lorentz-FitzGerald contraction hypothesis received little support from the evidence it accommodated: “not directly because Lorentz formulated the contraction hypothesis to accommodate the optical evidence […] [but rather because] the contraction hypothesis together with the rest of Lorentz’s electrodynamics forms an arbitrary conjunction” (583). Although Lange thus clearly links arbitrary conjuncts to ad hocness, he is far less explicit on what he considers to be the converse of arbitrary conjuncts. Lange has been read as placing epistemic
weight on unifiedness (Harker 2008), although one may well interpret his writing in terms of coherence (see Section 3).

2.3 Use-novelty and parameter-fixing

In several publications, Worrall has suggested that a hypothesis is *ad hoc* when it was used in the construction of a theory (Worrall 1985, 1989; Scerri and Worrall 2001; Worrall 2002). There are several issues with this account that need not concern us here (cf. Schindler 2013). Worrall’s writings also contain a stronger version of *ad hoc*ness (ibid.), according to which hypothesis H is not *ad hoc* with regard to evidence E, when it was *not needed* in the construction of H. Equivalently, Worrall also speaks of there being *no free parameters* for E in H that could be fixed so that H would (trivially) entail E. Conversely, then, a hypothesis H accommodates E in an *ad hoc* fashion if there is a free parameter in H that needs to be fixed on the basis of E in order for H to (trivially) entail E.¹⁵

A slightly more sophisticated parameter-fixing account than Worrall’s is the one by Hitchcock and Sober (2004). On their view, predictive success can be more valuable than accommodations, when it is indicative of another theoretical property that may not be entirely transparent, but desirable. I have elsewhere referred to this form of predictivism as symptomatic predictivism (Schindler 2013). In justifying this view, Hitchcock and Sober appeal to the model selection literature and “over-fitting”, i.e., the undesirable fitting of a model’s parameters to error and noise in the data at hand. Over-fitting will result in the decrease of a model’s predictive success of future data. When it’s not transparent whether or not the model is over-fitting the data, predictive success can thus be a valuable measure for the model’s degree of over-fitting.

There is a formal measure for optimizing a model’s fit with the data at hand and its predictive success of future data, namely the so-called Akaike Information Criterion (AIC) that Hitchcock and Sober use in their discussion. The details need not concern us here. But the essential idea is that, the fewer parameters a model possesses, the less likely it is to over-fit data that it ought not accommodate, and the more likely is it to accommodate data that differ

¹⁵ The reader is here presented with a streamlined version of Worrall’s account. For several complications consult (cf. Schindler 2013).
from the data at hand. Thus, the lower a model’s number of parameters, the better.16

Although Hitchcock and Sober’s account is plausible as far as it goes, and although parameter-freedom does appear to underlie some ad hoc judgements in science (cf. Section 3.1.5), I don’t think it takes us very far. It is, after all, an account motivated by data models. Only for data models, the above relationship between the number of parameters, empirical fit, and predictive success holds. It is far from clear, how the lesson Hitchcock and Sober draw from their discussion of data models is supposed to generalize to higher-level theories, such as Einstein’s theory of relativity, the standard model in high energy physics, the theory of evolution, etc. (Schindler 2013).

Worrall’s idea of construing ad hocness in terms of free parameters is of course not affected by the problems bugging the more specific parameter-fixing account proposed by Hitchcock and Sober. There is however one general problem for any parameter-fixing account: not all scientific theories are formulated in such a way that they contain parameters in the first place! In particular, non-mathematized theories, for example in biology or psychology, will be of that kind. And if Worrall’s account is supposed to be a general account of the conditions of success of a scientific theory (indeed, much in Worrall’s writing suggests that it is intended as such), then there will always be some reconstruction involved in accommodating those non-mathematized theories. Although that need not be a major obstacle, there might be more straightforward ways of getting to the heart of ad hocness.

2.4 Subjectivist accounts of ad hocness

In a remarkable paper discussing the role of the Michelson-Morley experiment, the historian Holton noted that scientists’ judgments about a hypothesis being ad hoc are often accompanied by the hypothesis in question being characterized in aesthetic terms as ‘artificial’, ‘contrived’, ‘strange’, ‘surprising’, and the like (Holton 1973, 327).17 Holton also claims that ad hoc judgments are highly

---

16 It is easy to see how (Forster and Sober 1994), earlier, argued for the value of simplicity on the basis of the AIC.
17 Holton even claims that an ad hoc judgment need not be pejorative and that there are “acceptable ad hoc hypotheses”. Holton provides no evidence for this claim other than stating that scientists sometimes describe them positively (Holton 1973, 327).
context-dependent, and vary inter- and even intra-subjectively. That is, what might be regarded as *ad hoc* by some may be seen as non-*ad hoc* by others, and what might be at regarded as *ad hoc* one point in time, according to these authors, might be regarded as methodologically sound at a later point in time, even by a single person (Holton 1973, 176-183). In conclusion, Holton urged that philosophical analysis “must be supplemented by an understanding of matters of scientific taste and feeling” (183). Recently Hunt (2012) reaffirms that “scientists’ aesthetic sense or ‘feeling’ governs their judgments in this matter [of *ad hocness*]” and that the answer to the question of whether a hypothesis is *ad hoc* is “largely in the eye of the beholder” (13). In Section 3.1.5 we will discuss some of the historical evidence that has been cited in support of the subjectivist account. Suffice it to say here though that the inter-subjective and intra-subjective variance of *ad hoc* judgements, if it is real, does not entail the conclusion that the subjectivists try to establish. As mentioned above, scientists make mistakes. Thus, when scientists disagree as to whether a hypothesis is *ad hoc* or not, some might be wrong. And scientists might change their minds about the *ad hoc* status of a hypothesis for *all the wrong reasons*. However I do think that at least some of the variance ought to be taken seriously. What I will argue though is that it can be accommodated within an objectivist account. A general objection against the subjectivist account is this: if *ad hoc* judgements are purely subjective aesthetic judgements, then whether or not a hypothesis is deemed *ad hoc* (by someone) ought to have no bearing whatsoever on the confirmation of the hypothesis in question. Indeed, this is the conclusion Hunt draws: “At the end of the day there seem to be no *ad hoc* hypotheses and no non-*ad hoc* hypotheses, only hypotheses—full stop” (Hunt 2012, 13). But this clearly flies in the face of scientific practice. Scientists use aesthetic language not just to express their ‘feelings’ and ‘tastes’. *Ad hoc* judgements are normative judgements that imply that one ought not to construct hypotheses in this way and that hypotheses that are so constructed deserve less confirmation than ones that are not *ad hoc*. But it is not just the scientists’ judgments that speak against a subjectivist understanding of *ad hocness*. If *ad hoc* hypotheses would be just as good as any other non-*ad hoc* hypotheses then there would be very little constraint on theorizing. Any theory facing anomalies could be amended at will without that being objectionable. But how would scientists then deliberate about which theory to adopt in a rational way?
3 A coherentist conception of ad hocness

In this section I will propose a new conception of ad hocness, namely the Coherentist Conception of Ad Hocness (CCA, for short), according to which a hypothesis H is ad hoc, iff theory T and H do not cohere or H does not cohere with the accepted background theories B.\(^\text{18}\) More precisely,

\[\text{Definition of ad hocness: A hypothesis H, when introduced to save theory T, is ad hoc, iff (i) there is evidence E for H and (ii) H coheres neither with theory T nor with background theories B, i.e., iff neither T nor B provide good reason to believe that H (possibly specifying a particular value of a variable), rather than non-H (or some value other than the one specified by H). With regard to T and B, H appears arbitrary.}\]

Condition (i) is trivially true for any ad hoc hypothesis: any ad hoc hypothesis gets introduced in order to accommodate evidence that T cannot. Condition (ii) prominently features coherence relations. Coherence has been described as a measure for “how well things hang together” (BonJour 1985). There is no agreement about what exactly coherence amounts to, but I will assume that if H coheres with T or B, T or B will give one good reasons to believe that H. These reasons, by virtue of being provided by T or B will be theoretical reasons. When T or B are confirmed, those reasons will also be inductive. Presumably, it is only then that those reasons are good reasons. Why background theories are made part of the definition of ad hoc hypotheses will be justified below in the discussion of historical cases (Section 3.1). Before engaging with the historical facts, however, let us add some clarifications regarding the coherence relation.

First, coherence must be distinguished from consistency. A theory may be consistent but still incoherent. That ad hoc hypotheses must be consistent with the theories they are added to should be obvious: if that wasn’t so, ad hoc hypotheses could not save theories from refutation. Second, coherence is normally considered to be a symmetrical relation. I will assume, though, that all that is required for H to cohere with T or B is that T or B provide good reasons for belief that H, and not necessarily vice versa. Of course, when there is evidence E for which H is invoked, and H coheres with T or B, then E will provide reasons for belief not only that H, but also that T or B. In that case the coherence relation between H and T or B would be complete in the traditional,

\(^{18}\)McMullin (1993, 133-4) was perhaps the first to link ad hocness with the lack of coherence. Yet McMullin fails to spell out systematically what that might mean.
symmetrical, sense. But I do not regard this as a necessary condition for H not to be ad hoc. Otherwise, any (temporally novel) prediction would be ad hoc before being confirmed.

My coherentist account of ad hocness is an objectivist account. When a hypothesis does not cohere either with T or B, H will be ad hoc. Contrapositively, when H coheres with T or B, it won’t be ad hoc. Yet, the account does allow for a subjective element in ad hoc judgements: some scientists might judge some connections of H to T or B to be more important than others. I’m therefore proposing an analogue to Kuhn’s model of theory-choice where individuals weigh objective theory properties according to their own (subjective) preferences [Kuhn, 1977 #101]. Scientists can therefore legitimately differ about which coherence relations they consider to be more important. In shall discuss an example for such disagreement in Section 3.1.5. What my account does not license, however, are judgements about a hypothesis being ad hoc when as a matter of actual fact the hypothesis coheres with T or B. My account thus sets clear limits to legitimacy of ad hoc judgements.

Relatedly, proponents of a subjectivist account of ad hocness have cited degrees of ad hocness as evidence for ad hoc judgements being entirely aesthetical (Section 2.4). Yet, once again, one can admit the former without drawing the latter (radical) conclusion. Also CCA allows for degrees of ad hocness: the stronger the coherence between H and T or B, the less ad hoc H. The strength of coherence may perhaps be gauged both in terms of number of relations and quality of relations. It is also worth noticing that degrees of ad hocness allow us to say one theory ought to be preferred over another, regarding ad hocness, even if neither theory manages entirely without ad hoc hypotheses: one theory might just invoke fewer ad hoc hypotheses than the other. Indeed it may well be that there are no theories that do entirely without ad hoc hypotheses and we may still have good reasons for preferring some theories over other on the basis of them making less ad hoc assumptions than others. It is also plausible that in the absence of more coherent theories scientists will stick with not fully coherent theories that contain ad hoc modifications. Arguably, that is what physicists do with the standard model of particle physics, which includes the Higgs mechanism (see Section 2.1).
One may also wonder about how CCA relates to the other accounts of ad hocness that I mentioned above, in particular the parameter-fixing account and the account that construes ad hocness in terms of lacking unification. Let us start with the former. CCA is compatible with the parameter-fixing account of ad hocness (Section 2.3). Free parameters in a theory may be fixed on the basis of some data E without there being a good (theoretical) reason for fixing the parameter at that value. One of the advantages of CCA over the parameter-fixing account is it that it is much broader: one need not embark on any dubious reconstructions in order to make sense of theories that are not formalized. Let’s move on to the unifiedness account. Coherence is indeed closely related to the property of unifiedness. But there is at least one important difference. On the conception of ad hocness which construes ad hocness as the lack of unifiedness (see Section 2.2), one assesses whether a hypothesis H is ad hoc in relation to the theory which H is supposed to save only. Background knowledge, contrary to the CCA, plays no role. We shall see however in a moment (and in particular in Section 3.1.3 and 3.1.5) that background knowledge ought to be part of the equation as a matter of descriptive accuracy. There is another, more important, reason why unificatory accounts of ad hocness are problematic. Often the unifiedness of a theory is said to be measured in terms of how many assumptions it must make when accounting for a set of phenomena (cf. e.g. Harker’s account in Section 2.2 and [Kitcher, 1981 #58]). But suppose we have a theory A which accounts for the phenomena in a unified way and a theory B which needs to invoke further hypotheses accounting for further evidence, which do not fall under the unification of the theory in question. Can we then dismiss B on the number of assumptions it is making? Not necessarily. As we know from Duhem, we must make a host of auxiliary assumptions when testing also A. Whether we end up with more assumptions on B than on A is certainly not determinable a priori. Even a posteriori that might be a difficult task. But a functional account of ad hocness should not be subject to such indeterminacies.

Lastly, this is how CCA relates to novel success: if T predicts E (via H), it will always be the case that T provides (good) reasons for belief that H. Otherwise, there would not be a prediction of E by T (via H) in the first place. So for example, when Einstein was able to derive from theory of general relativity (T) that light would bend around massive objects such as stars (H) in 1915, the
theory provided reasons for belief in the existence of star light bending, which motivated Eddington and others to go out and collect observational evidence (E) for H. In contrast, when some H is invoked to accommodate (rather than predict) some E, then it may or may not be that the relevant T or B provides good reason to believe H. Only in the latter case would H be *ad hoc*, on the coherentist conception. Since on the CCA the question of whether there is good reason for belief in H is entirely transparent, there ought to be no asymmetry in confirmation between prediction and accommodation, so long as H coheres with T or B. Of course, there will be an asymmetry between the entirety of accommodative successes as compared to the entirety of novel success, simply because the former will involve also accommodative successes achieved with hypotheses that do not cohere with T or B. But since, again, coherence relations are transparent relations, this asymmetry is of little interest.\(^\text{19}\)

One might be worried that the CCA could commit one to a coherentist epistemology for science with all its well-known and highly problematic implications. But that is not the case. Coherentism is the view that a belief is justified iff it coheres with other beliefs. Nothing in the CCA would imply that much. On the contrary, I believe that theories and hypotheses in science are justified when they are supported by the relevant evidence, just as foundationalism has it. Nevertheless those support relations, on CCA, will be stronger or weaker depending on the amount of coherence between the hypothesis and the theory in question. In other words, the coherence relations modify the support relations. The epistemology to be associated with the CCA may therefore be referred to as weak foundationalism (Olsson 2012).

### 3.1 Illustrations

Let us now see how the CCA handles some important historical cases and how it does so better than its rivals. Some of the issues we will focus on are *theoretical* reasons for belief in H provided by T or B, subjective weighting of objective

---

\(^{19}\) The notion of *ad hoc*ness defended here is in some ways similar to the one hinted at by Lange (2001), discussed in Section 2.2. Once more (cf. fn. 18), however, I would want to lay claim on having presented a more general and systematic account. And some points made by Lange, I simply don’t agree with. For example, Lange alleges that “[…] a few examined cases do not suffice to lead scientists to formulate [an arbitrary conjunction] … Therefore, a hypothesis judged to form an arbitrary conjunction typically arrives with many accommodations […]” (577; added emphasis). I don’t see at all why this ought to be so.
coherence relations, and the role of background theories in determining the *ad hoc* status of hypotheses.

### 3.1.1 Copernicus vs. Ptolemy: the planets

That coherence considerations might indeed underlie *ad hoc* charges is quite directly suggested by Copernicus’s expressed reasons for dismissing the Ptolemaic system of astronomy in the 16th century. Although the epicycles as used by the Ptolemaists have become the proverbial *ad hoc* hypotheses in the philosophical literature (and beyond), it wasn’t those that Copernicus objected to when proposing the heliocentric system (in fact, he used them himself). In fact, it is not something he could have legitimately objected against: Copernicus used the epicycle device quite extensively himself [Kuhn, 1957 #39]. Instead he, in the preface to *De Revolutionibus*, complained about a lack of coherence in the Ptolemaic system:

> [Ptolemaic astronomers have not] been able [...] to discern or deduce the principal thing - namely the shape of the universe and the unchangeable symmetry of its parts. With them it is as though an artist were to gather the hands, feet, head and other members for his images from diverse models, each part excellently drawn, but not related to a single body, and since they in no way match each other, the result would be a monster rather than a man. (Copernicus cited in Kuhn 1957, 137-8)

At the end of this seminal passage, Copernicus admits that “[t]hough my present assertions are obscure, they will be made clear in due course” (ibid.). What he did make clear (amongst other things), was that there were a number of observations which, on the Ptolemaic system, had to be simply assumed, but for which the Copernican system gave good reasons to believe. Some of these observations were the maximum elongation of the inferior planets, or put more generally, the “striking correlation between one of the two main components of the motion of every planet with the motion of the sun” (Janssen 2002a).

Maximum elongation refers to the fact that the inferior planets never move far away from the apparent trajectory of the sun on the celestial sphere,

---

20 Gingerich [], 1975 #1096] raises doubt that the numbers of epicycles can be determined unequivocally on any of the two systems.

21 Another point that was important to Copernicus was that the Ptolemaists had, with the deployment of eccentrics, departed from the Aristotelian principle of uniform motion. For a highly interesting discussion see (Miller 2014).
i.e., the so-called ecliptic. In any astronomical system this fact had to be represented by the inferior planets not moving beyond a certain distance from the sun. In contrast, the Ptolemaists had to simply decree that the motion of the deferent of the inner planets was for some reason correlated with the motion of the sun. More specifically, the line connecting the earth with the centre of the epicycle of an inner planet had to be parallel to the line connecting the earth and the sun. Similarly, for the outer planets, the Ptolemaists had to assume that for some reason the line connecting an outer planet and the centre of that planet’s epicycle were always parallel to the line connecting the earth and the sun. On the Copernican system, each of those components of motion of a planet / the sun is simply due to the motion of the Earth (for details see e.g. Janssen 2002a).

There are a number of other observations that the Ptolemaic system accommodates by invoking arbitrary assumptions and which the Copernican system provides good reasons for. For example, whereas the Ptolemaic system is ‘indifferent’ towards the order of the planets and inter-disciplinary distances, they can be determined unequivocally on the Copernican system (for details see Kuhn 1957, 173-5; Glymour 1980, 178-203). The Copernican system, but not the Ptolemaic systems, gives good reasons for why the sun and the moon never retrogress and that the frequency of planetary retrogressions decreases from Saturn, Jupiter, and Mars, and increases from Venus to Mercury, and that the superior planets are the brightest in their opposition (Copernicus 1543/1992, 26-7).

Of course, the Copernican system did not do without ad hoc assumptions either. In order to account for the stellar parallax shift, which was entailed by the Copernican system, but which could not be observed up until the early 19th century, Copernicus had to surmise that the stars were much more far away from the solar system than had previously been thought. But that hypothesis was incoherent with the accepted background theories of the time. This was the reason why, for example Tycho Brahe, did not accept the Copernican system. Regardless, is that the Copernican system, as compared to the Ptolemaic system, was much less ad hoc when it came to accounting for the (apparent or real) motion of the planets, the sun, and the moon.

CCA is not only vindicated by Copernicus’s famous monster-analogy when criticizing the Ptolemaic system, but also by the particular failings of the
latter. According to the CCA, a hypothesis is *ad hoc* when it does not cohere with either the theory it saves from refutation or with the background knowledge. The Ptolemaic system was *ad hoc* (amongst other things) because it did not give any (theoretical) reasons for why the inner planets would not move beyond a certain distance from the sun. With regard to this and other astronomical phenomena, the Copernican system was much more coherent in our sense, and therefore less *ad hoc*, than the Ptolemaic system.  

3.1.2 The Lorentz-FitzGerald contraction hypothesis revisited

CCA requires that T or B give good theoretical reasons for H not to be *ad hoc*. The case of the Lorentz-FitzGerald contraction hypothesis is a neat illustration of this. The upshot of the following extended analysis is that Lorentz sought to render the contraction hypothesis non-*ad hoc* by providing good theoretical reasons within his ether theory, but failed.

In the analysis of the LFC, one must distinguish between an early and a later, mature, version. The early version was proposed in cursory form by Lorentz in 1895 (and by FitzGerald around the same time) in a short section in the final chapter of a 139-page strong book, the mature version in 1904. Many philosophers of science, for various reasons, believe that the *ad hoc* charge applies only to the early, but not to the mature version of the LFC (Zahar 1973; Grünbaum 1976; Janssen 2002b; Acuña 2014). And that is consistent with us possessing evidence for scientists such as Einstein deeming the early, but not necessarily the later version of LFC, *ad hoc* (Holton 1969, 169).

Let us briefly recall that, in the early version of LFC, Lorentz simply assumed that (i) “molecular forces are also transmitted through the ether, like the electric and magnetic forces of which we are able at the present time to make this assertion definitely”, that (ii) the ‘attraction and repulsion’ of molecular forces for a body at rest would be in equilibrium, and that (iii) the Lorentz transformations would apply not only to electrostatic forces, but also to the ‘molecular forces’ holding together matter (Lorentz 1875 in Einstein et al. 1952, 6). As Lorentz readily admitted himself, “there is no reason” in his theory

---

22 This case is an interesting example of non-explicit *ad hoc* judgments, the possibility of which I mentioned at the beginning of this paper. Although, as far as I’m concerned, one does not find Copernicus using the word ‘*ad hoc*’, he is adamant in his condemnation of the use of assumptions that do not cohere with T in our sense.
for making the latter assumption in particular. And assumption (ii) is highly implausible (cf. Janssen 2002b, 437).

Some commentators have emphasized that Lorentz’s remarks were specifically catered to the Michelson-Morley experiment and that Lorentz articulated the LFC in more general terms only in the later 1904 publication, where he also managed to derive the second order effects he needed to account for the Michelson-Morley experiment (Schaffner 1974; Janssen 2002b). The mature formulation of the LFC entailed consequences which the early version didn’t (or at least not explicitly), such as the velocity-dependence of mass (Janssen 2002b, 425). That consequence was of course testable. It is also for this reason that some philosophers have claimed that the mature LFC was not ad hoc (e.g. Acuña 2014).

But even with the mature LFC, it has been argued, there is something wrong. According to Janssen (2002b):

In Lorentz’s theory, there is a strict separation of ether and matter […] Lorentz decreed a number of important exceptions to the Galilean-invariant Newtonian laws that are supposed to govern matter, so that the laws effectively governing matter are Lorentz invariant. Why, one can legitimately ask, would the laws governing matter have the property of Lorentz invariance, which so far appeared to be nothing but a peculiar property of Maxwell’s equations? […] In the final analysis, it is thus left an unexplained coincidence in Lorentz’s theory that both matter and fields are governed by laws that are Lorentz invariant [whereas in Einstein’s theory of special relativity, it isn’t]. (p. 423 and 426, added emphasis)

Janssen (2002a) cites for approval Poincaré’s dismissal of the LFC in his introduction of Sur la dynamique de l’électron (1906):

We cannot content ourselves with simply juxtaposing formulas that would agree only by some happy coincidence; the formulas should, so to say, penetrate each other.

And indeed, also in the mature theory Lorentz was merely assuming that the Lorentz transformations would apply to ‘molecular forces’, holding together

---

23 For Janssen (2002b, 425) the “generalised” LFC is the following: “a matter configuration producing a certain field configuration in a frame at rest in the ether will, when the system is set in motion, change into the matter configuration producing the corresponding state of that field configuration in the frame moving with the system.”
matter, in the same way they applied to electrostatic forces (Lorentz 1904 in Einstein et al. 1952, 22).

Janssen does not link this perceived deficit of Lorentz’s theory to judgments about LFC being *ad hoc*. Indeed he concludes that “a solid case can be made for the claim that [Lorentz’s mature] theory is not *ad hoc* by any of the criteria considered here” (ibid., 437). Instead he regards the above shortcoming as a different reason for why Lorentz’s theory was inferior to Einstein’s.24 Yet I don’t see why one ought to keep those reasons separate. After all, already in the publication of the early LFC version, as we mentioned before, Lorentz admitted that “there is no reason” to suppose (as he did) that the Lorentz transformations should apply also to matter. But if that’s so, then it’s not too implausible to suppose that *this* is also what Einstein and others objected to when deeming the (early) LFC *ad hoc*.

It is also interesting to note that Lorentz, in a letter to Einstein in 1915, i.e., 10 years after proposing his mature theory, stated his belief that the LFC was rendered non-*ad hoc* by him offering an explanation for it in terms of molecular forces:

[… ] I had added that one can arrive at this hypothesis [i.e., the LFC], if one extrapolates from what one was able to say about the influence of translation on electrostatic forces to other forces. *Had I stressed it more, the hypothesis would have made less of an impression of having been devised *ad hoc*” (Lorentz 1915 in Schulmann et al. 1998, 71-2).25

What Lorentz had said in 1875, again, was

that [the LFC] is by no means far-fetched, as soon as we assume that *molecular forces are also transmitted through the ether*, like electric and magnetic forces of which we are able at present time to make this assertion definitely […] *From the theoretical side*, therefore, there would be no objection to this hypothesis (Lorentz in Einstein et al. 1952, 6, added emphasis).

So what Lorentz appeared to have thought was that the LFC lost its *ad hoc* character at the moment when he was able to lend to it some theoretical plausibility. And that Lorentz thought, despite the fact that he at the same time

24 See Acuña (2014) for a detailed criticism of Janssen’s account.
25 This is my own translation of the original German text.
admitted that he had devised the molecular forces explanation only *after* he had come up with the LFC (Lorentz 1915 in Schulmann et al. 1998, 74). Although we don’t know what Einstein made of that suggestion, Lorentz doesn’t seem to be alone with this judgement. Leplin (1975, fn. 18 p. 314-5) points out, for example, that two later textbooks (one from 1924 and one from 1969) seem to suggest that “Lorentz’s representation of contraction as a condition of molecular equilibrium mitigated its *ad hoc* character”.  

From the point of view of CCA, what Lorentz appeared to have sought to do in order to diminish the *ad hoc* status of the LFC was to establish a coherence relation between the LFC and the rest of the ether theory. That he achieved only to a limited degree. Although there perhaps was *some* plausibility in assuming that the molecular forces that Lorentz postulated for matter would behave not unlike the electromagnetic forces “since both types of force are states of the same substratum”, as (Zahar 1973, 116) put it, it remained highly curious how this was to be achieved. As mentioned above, the ‘molecular forces’ hypothesis required that there be an electrostatic equilibrium when a body is at rest. But there is no such thing as electrostatic equilibrium (cf. Janssen 2002b). Thus, although Lorentz was able to provide *some* reasons for belief that LFC, he wasn’t able to provide *good* reasons. His attempted explanation of the LFC in terms of molecular forces did not establish coherence with either the ether theory nor with the background theories.

### 3.1.3 The Higgs mechanism revisited

Also the *ad hoc* judgements regarding the Higgs mechanism can be accommodated by the CCA. Recall some of the most frequent charges levelled against the Higgs mechanism by physicists (cf. Section 2.1): (i) the large number of free parameters, (ii) there are no other fundamental scalar particles other

---

26 In his reply to Lorentz, Einstein did not mention the issue (Schulmann et al. 1998).

27 Zahar (1973) suggests it is for this reason that the LFC is not to be regarded *ad hoc*. Zahar claims that Lorentz was able to ‘derive’ the LFC from the molecular force hypothesis. But that’s not the case. Lorentz offered only a ‘plausibility’ argument, no derivation (Janssen 2002b, 436-7). In his mature theory, the length contraction he derived from what Janssen calls the *generalised* LFC (see fn. 23, above).

28 One might be tempted to interpret Lorentz’s molecular forces hypothesis as an attempt to produce an explanation that would engender novel predictions and that it was for the latter reason, not for the former, that Lorentz thought the LFC was rendered non-*ad hoc*. But we already argued in Section 2.1 that independent testability does not render an *ad hoc* hypothesis non-*ad hoc*. This fits with Holton’s conclusion that the independent predictions of the LFC “were not urged as tests that would decide on its acceptability” (Holton 1969, 177). It should also be noted that already Lorentz’s early (non-generalized) LFC was testable with the Kennedy-Thorndyke experiment (Janssen 2002b, 433).
than the Higgs boson, (iii) the symmetry breaking of the Higgs mechanism is
different from all other known cases of symmetry breaking, (iv) the Higgs
particle is ‘fine tuned’ to its interaction mass and unexplained. From the
perspective of CCA it is very clear why the Higgs mechanism is deemed \textit{ad hoc}:
neither the standard model of fundamental particle physics or any background
theory gives us any good (theoretical) reason for why the parameters have the
values that they do and why the bare mass of the Higgs boson is fine tuned to
its interaction mass. Nothing in our background theories gives us any reason
why there ought to be only one fundamental scalar particle and why symmetry
breaking may proceed differently only in the Higgs mechanism.

3.1.4 Neptune revisited

The stipulation of Neptune, I contended in Section 2.1 on the basis of the lack of
any evidence for \textit{ad hoc} judgements in the scientific community, is not to be
considered an \textit{ad hoc} hypothesis. According to the coherentist conception, this is
so because, given the irregularities discovered in the orbit of Uranus,
Newtonian mechanics—by virtue of its laws—\textit{did} provide good reasons for
believing that there was another planet in Uranus’s vicinity. It is also worth
noticing that the case of Neptune provides an example for the introduction of a
hypothesis that saves a theory from refutation but is not \textit{ad hoc}. It thus gives yet
another reason why the intuitive notion of \textit{ad hoc}ness considered in the
beginning of this article is not sufficient for a hypothesis to be \textit{ad hoc}.\footnote{Another example is the modifications of the Bohr model in response to anomalies (see McMullin 1968, 1985). See also \textit{blinded reference}.}

3.1.5 Extended case study: the cosmological controversy

As we’ve seen above (Section 2.3) that some writers have claimed that \textit{ad hoc}
judgements are \textit{nothing but} aesthetic judgements. The main supporting example
of the most recent contribution in this school of thought (Hunt 2012) is a
selection of the cosmological controversy between proponents of the big bang
theory and its main competitor the steady state theory in the 1950s-1960s. This
controversy is rich and telling, I will therefore discuss it in more detail.\footnote{The following significantly expands on Hunt’s (rather cursory) discussion by drawing substantially on (Kragh 1996) and some original sources.} In
particular, it provides a nice illustration of both the idea that \textit{ad hoc} judgements
are the result of subjective weightings of objective coherence relations and the
idea that background knowledge should be part of the equation in the analysis of *ad hoc* judgements.

Both the big bang and the steady state theory presume that the universe is expanding, and in particular (in agreement with Hubble’s law), that the galaxies furthest away from us are moving away from us with the highest velocity.\(^{31}\) In the forward time direction, expansion will result in the density of matter and energy decreasing. In the words of Hoyle (1955) “space is therefore (it seems) getting more and more empty as time goes on” (315). In the backward time direction, density will increase and the universe ‘contract’. It would seem that an *origin of the universe back in time* should have been a natural conclusion, since, going back in time far enough, all matter of the universe should be “squeezed into a uniform mass of very high density” (Gamow 1961, 28). Yet, the idea of an origin of a universe wasn’t so natural to many physicists at the time. In particular, the proponents of the steady state theory sought to evade this consequence. They held high the so-called *perfect* cosmological principle, which had it that the distribution of matter and energy must appear ‘the same’ to an observer anywhere in the universe at any time. As we shall see in a moment, they embraced this principle, because they believed that it was presupposed by the idea that laws of physics applied throughout the universe (cf. e.g. Bondi 1960/1952).

Although the steady state theorists too accepted that the universe is expanding, they had it that the universe, essentially, has always looked the way it does today. Expansion they viewed not as resulting from a big bang, but rather from a dynamic equilibrium of attractive and repulsive gravitational (Newtonian) forces, at short and large distances, respectively. In order to theoretically counter-balance a decrease of the density of matter in space over time as implied by expansion, the steady state theorists postulated the continual creation of matter, evenly distributed throughout the universe. Indeed, in order to maintain the steady state picture of the universe, one had to assume that matter is being continually created in such a way that “the effect of expansion is

\(^{31}\) E. Hubble in 1929 provided the first observational data for an expanding universe, although Hubble himself did not interpret his own results in this way. Before it became widely accepted that the universe is expanding, Einstein had inserted the notorious cosmological constant into his general theory of relativity in order to keep the universe static. After the Hubble discovery, Einstein removed the constant and called his earlier appeal to it his “greatest blunder”. See (Kragh 1996). Without Einstein’s constant, an expanding universe follows naturally from Einstein’s theory.
[precisely] compensated in such a way that the total amount of matter in the observable universe remains constant” (Hoyle 1949, 18). And it was only for this reason that one could deduce the rate of creation from the mean density of matter (which was thought to be constant) and the rate of expansion (as given by Hubble’s law); it was estimated to be three hydrogen atoms per cubic meter per million years (Krugh 1996, 183). However there appeared to be no grounds for supposing that the rate of creation of matter ought to coincide precisely with the expansion rate other than to save the perfect cosmological principle.

From the beginning, the continual creation hypothesis was viewed with great suspicion by the majority of astronomers and physicists. For one thing, the steady state cosmologists were accused of introducing an unnecessary additional assumption in the form of the continual creation hypothesis (McVittie 1949, 49; Milne 1949; cf. Kragh 1996, 190). For another, that selfsame hypothesis was inconsistent with the conservation of energy, as implied by Einstein’s theory of relativity. Furthermore, and perhaps most importantly, whereas the big bang theory could simply employ Einstein’s field equations of general relativity, the proponents of the steady state alternative struggled badly to come up with a workable dynamics. Some provided a merely qualitative theoretical framework (Bondi and Gold 1948). Those who tried to devise a modified field-theoretic treatment (Hoyle 1948) faced the objection of self-defeat. Later formulations faced even more serious obstacles (Kragh 1996, 213). Several contemporary authors stressed this essential shortcoming of the steady state theory when criticizing the theory (cf. Kragh 1996, 213, 222). But even if the steady theorists had come up with a workable mechanics, this would have left at least some of the critics unimpressed, for

If [the steady state theory is] accepted, it is necessary to suppose that the mechanics of the nebular systems are in some way different from the mechanics of all other astronomical systems. If the object of science is to unify phenomena into theoretical systems with as wide an amplitude as possible, the general relativity may be accepted until it leads to some prediction seriously contrary to observation. (McVittie 1951, 75, added emphasis)

---

32 It was criticised that that Hoyle’s treatment failed to deliver on the promise for which the steady state theory had been devised as an alternative to the big bang theory in the first place: its uniqueness (as compared to the flexibility of all the evolutionary relativistic models, such as the big bang theory). See Kragh (1996, 205) and the main text below for more details.
Let us follow the thread of Hunt’s story in when making his case for _ad hoc_ judgements being aesthetic (Hunt 2012). Hunt focuses on the cosmologist D.W. Sciama (1926-1999). He claims that Sciama adopted the steady state hypothesis and not the big bang theory, for he considered the latter, but not the former, _ad hoc_. According to Hunt, Sciama disfavored the big bang theory in particular for its parameter flexibility and the arbitrary setting of an initial temperature for the creation of heavy elements (Hunt 2012, 7). In 1960 Sciama for example wrote that the “actual behavior of the universe can be accounted for [with the steady state hypothesis] without _ad hoc_ assumptions” (Sciama 1960, p. 10; cited in Hunt 2012, fn on p.11), implying that the big bang theory couldn’t afford such a feat. Continual creation, for Sciama, did not have the same aftertaste it had for many other physicists. On the contrary, Sciama thought that “[i]f creation [of matter] is occurring all the time [by virtue of the continual creation hypothesis], it becomes a scientific process you can study because it’s repetitive”. The big bang theory, in contrast, Sciama considered “an awkward thing” in this regard (both quotations from an interview with George Gale on January 27th in 1990, cited in Hunt 2012, 7). Other proponents of the steady state theory, such as Bondi and Gold, also repeatedly emphasized this point (Kragh 1996, 181ff.). For example, Bondi and Gold (1948) could “see no reason why the laws of nature should be invariant while admitting that the one and only application [i.e., the universe itself] is not invariant [but rather unique]”. Hoyle, another major proponent of the steady state hypothesis, wrote that “[a]n explosive creation of the Universe [as envisaged by the big bang theory] is not subject to analysis. _It is something that must be impressed by way of an arbitrary fiat_” (Hoyle 1955, 318 added emphasis). It was also for this reason that Hoyle considered the postulation of an origin of the universe _ad hoc_.33 “In the case of a continuous origin of matter, on the other hand”, Hoyle continued “the creation must obey a definite law, a law that has just the same sort of logical status as the laws of gravitation, nuclear physics, of electricity and magnetism” (Hoyle 1955). Yet, what this law was supposed to look like the proponents of the steady state theory were never able to specify. And as mentioned above, no reason was given why matter creation should proceed at exactly the rate that would counterbalance the

---

33 With regard to the formation of galaxies, Hoyle stated explicitly: “In the explosion theory the formation of clusters of galaxies has to be introduced as an _ad hoc_ process that takes place for no good reason at just the stage where the density of matter falls to a thousand million million millionth part of the density of water (or perhaps somewhat less than this). In contrast, the steady state hypothesis Hoyle thought to offer “a more natural explanation” (Hoyle 1955, 317).
extension of the universe other than that this would make a steady universe possible despite expansion. Indeed, one of the main proponents of the steady state theory, Bondi, admitted that “[t]he theory offers no explanation of the[se] numerical coincidences” (Bondi 1960/1952, 151). So the accusation of arbitrary fiat applied to them as much as it applied to their opponents, if it did at all.  

A related criticism the steady state proponents leveled against the big bang theory had to do with parameter freedom. Bondi and Gold (1948), in their foundational paper, wrote that

[in general relativity a very wide range of [cosmological] models is available and the comparisons [between theory and observation] merely attempt to find which of these models fits the facts best. The number of free parameters is so much larger than the number of observational points that a fit certainly exists and not even all the parameters can be fixed. (262)]

In a similar vein Sciama (1961) wrote that

A theory which, whilst it can be tailored to fit this unique universe, nevertheless has to present certain aspects of it as arbitrary, as though they could have been different, is therefore less satisfactory than a theory in which these aspects are essential. (7; added emphasis)

In the same paper, Sciama presented the steady state theory as a theory “in which the actual behavior of the universe can be accounted for without ad hoc assumptions” (10; original emphasis). In his book, Sciama extended on this thought, writing that

---

34 The continual creation hypothesis spawned the search for stellar (rather than cosmological) creation of elements, which cherished some explanatory success (Kragh 1996, 295). Gamow was not impressed at all. He wrote that the theory effectively required in terms of processes of element creation was similar to “the request of an inexperienced housewife who wanted three electric ovens for cooking a dinner: one for the turkey, one for the potatoes, and one for the pie. Such an assumption of heterogeneous cooking conditions, adjusted to give the correct amounts of light, medium-weight, and heavy elements, would completely destroy the simple picture of atom-making by introducing a complicated array of specially designed ‘cooking facilities’” (Gamow 1961). Furthermore, Hoyle et al.’s theoretical treatment had a decisive defect: it could not account for the creation of the amount of the light element helium, one of the two most abundant elements in the universe (together with hydrogen) (Kragh 1996, 338). It should also be noted that stellar element creation was of course compatible with the big bang theory. Regardless, big bang proponents such as Gamow sought to explain as much element creation as they could in cosmological terms (Kragh 1996, 296).

35 Kragh (1999) has referred to relativistic cosmology as “not a theory in the ordinary sense [but] rather a supermarket of theories which had in common that they were all solutions of the same fundamental [Einstein field] equations” (378).
What the cosmologist requires, therefore, is a theory which is able to account in
detail for the contents of the universe. To do this completely, it should imply
that the universe contains no accidental features whatsoever. This provides us
with a criterion for assessing the validity of rival theories. We believe this
criterion to be so compelling, that the theory of the universe which best
conforms to it is almost certain to be right. (Sciama 1959, 150)

Sciama was confident to have found such a theory in the steady state theory.36
Again, Sciama contrasted the steady state theory with the big bang alternative.
In particular, he criticized that on the big bang theory the size of galaxies was
used to determine the character of the early universe, in particular, temperature
fluctuations. Sciama complained that “no reason is given” why the early
universe should have one particular fluctuation characteristic, rather than any
other, other than such a characteristic providing the initial conditions for
deriving the current form of our galaxies. Such an assumption, Sciama
criticized, was merely “accidental” and “devoid of theoretical significance”
(150).37 Sciama had a similar criticism of the big bang’s explanation of the origin
of heavy elements:

But to my mind there is a more important reason for preferring the steady-state
theory. For in theories which start from an explosion [i.e., big bang-type
theories] the initial properties of the universe are entirely arbitrary. Thus it is
possible to find an initial temperature which is favourable for making heavy
elements, and then one simply has to assume that this was the initial temperature
although the general theory would be equally compatible with any other initial
temperature. This means that in this type of theory the laws of physics do not
specify the contents of the universe, but only show how one state of the universe
follows from another. (Sciama 1955, 42 added emphasis)

The debate that took place in the 1950s between proponents of the big bang and
steady state theory (as far as it occurred at all) could be said to be largely
speculative. The relevant observations that were available, were not apt to
decide between the two theories (Kragh 1996, 269ff.). It was only in the 1960s
that new observations were made that significantly bore on the choice between
them. Those observations, it turned out, worked against the steady state theory:
abundant quasars and radio galaxies were found at large distances, but not in

---

36 Cf. (Sciama 1960, 323).
37 Interestingly, the formation of structure in the history of the universe is an unsolved problem until this
very day.
closer galaxies. On to the steady state theory, such inhomogeneities were puzzling. The proponents of the steady state theory, however, tried to argue for their theory despite these difficulties. Sciama (again, Hunt’s main protagonist in his case study concerning ad hocness) sought to explain the abundance of radio sources at greater distances with our part of the galaxy being in a ‘local hole’ of nearby galactic radio sources (rendering the observed abundance an only apparent one), demanded more data before ruling out local quasars, and also proposed an astronomical mechanism mimicking the blackbody nature of the microwave background. At some point (around 1966), however, Sciama gave up the steady state hypothesis, and in fact criticized those who put even more ‘epicycles’ on it to account for the forthcoming observations. Hunt concludes that “scientific judgments about ‘how much was too much’ were quite different” (ibid., 9), and concludes that “[a]s scientists’ aesthetic sense or ‘feeling’ governs their judgments in this matter [of ad hocness], this will manifest itself in different ways” (ibid., 12).

3.1.5.1 Assessment

Let us now assess this episode from the point of view of CCA. Recall that of the main points of the Big Bang theory (BBT) proponents regarding the steady state theory (SST) appears to have been the fact that it was inconsistent with both the general theory of relativity and conservation principles. Since consistency is a necessary condition for coherence, the continual creation hypothesis (CCH) did not cohere with the relevant background theories. By that count, it was ad hoc according to the coherentist conception of ad hocness. Another criticism—which was perhaps not leveled as explicitly—was that SST gave no reason for belief that CCH, and in particular, for the assumption that the rate of creation of matter ought to coincide precisely with the expansion rate of the universe (as admitted explicitly by Bondi; cf. Section 3.1.5). The only reason that the proponents of SST could provide for CCH was that the perfect cosmological principle was sustainable together with the known expansion rate only with that assumption: CCH had to compensate precisely the expansion rate in order to

38 He for instance refrained from extending the local ‘hole’ in the distribution of radio sources and by “insisting that it was impossible that any quasars could be at cosmological distances” (Hunt 2012, 8).

39 I was not able to find any explicit use of the word ‘ad hoc’ by big bang proponents in the contemporary literature. Yet the remarks by Milne and McVittie, mentioned above in the main text, I take to be (non-explicit) ad hoc judgements. Also Gamow commented on several occasions that the continual creation hypothesis was “unnecessary” (Gamow 1954, 60; 1961, 34) and “artificial” (Gamow 1952, 40), the latter of which in particular is used frequently in conjunction with ad hoc judgements.
save the idea that the universe has always been homogeneous and isotropic. We are thus very much reminded of Lorentz’s attempt to save his ether theory by stipulating length contraction: there was no reason in his theory to believe that ‘molecular forces’ would obey the letter of the Lorentz transformations, just as electrostatic forces did. The only sustainable reason he could provide was that it saved the appearances (the molecular forces hypothesis, as we saw in the last section, was an attempt to give a theoretical reason, but a failed one). Both the contraction hypothesis and the CCH are ad hoc by the lights of CCA because neither T nor B provided good (theoretical) reasons for them: neither T nor B provided good reasons for belief that the Lorentz transformations ought to extend to matter, and that matter ought to be created at a rate that would compensate the expansion of the universe, respectively.

The SST proponents, on the other hand, criticized the BBT on two counts. First, they objected to BBT postulating a beginning of the universe, i.e., a unique, non-repetitive event, which for that very reason would be different from other events subject to the laws of nature. As we saw, Hoyle went as far as saying that the big bang would be awkward because it would be exempt from the laws of nature. It was for that reason that he explicitly considered the BBT ad hoc. Relatedly, the SST theorists were discontent with the fact that there was a multitude of cosmological models (rather than a single determinate BBT) resulting from the fixing of parameters in the relativistic field equations that underlay the BBT. On the coherentist conception of ad hocness, also the BBT can be considered ad hoc (to some limited extent): it gave no reason for belief for fixing the free parameters of the relativistic field equations to the values that they were set to when seeking to account for structure and element formation in the early universe (see Section 3.1.5).

In sum, we can thus say that were objective grounds for both sides to accuse the other camp of making ad hoc assumptions, namely the lack of coherence relations. Those were just weighted differently by different scientists. Again, on CCA we can accommodate a subjective element of ad hoc judgments without rendering ad hoc judgements entirely arbitrary. Likewise, CCA can accommodate the re-evaluations of ad hoc judgements by individual scientists as in the case of Sciama: over time, coherence relations may be established between T or B and H, where there were none before.
What we can also say, however, is that the assumptions the SST had to make were more problematic than the ones the BBT had to make. Again, the SST was inconsistent with two background theories (the conservation principle and the general theory of relativity) and could provide no reason for belief for the creation rate coinciding precisely with the expansion rate. It is thus understandable, from the coherentist conception of \textit{ad hoc}ness, why the broad class of evolutionary relativistic models, which BBT was an instance of, was able to draw much more support from the community of physicists and astronomers than the steady state theory even \textit{before} decisive empirical information could be garnered.

4 Conclusion

The historical cases discussed in this paper support a new conception of \textit{ad hoc}ness, namely the coherentist conception of \textit{ad hoc}ness. Competitor accounts either run into conceptual difficulties or are at odds with the historical cases. While there may be historical cases out there that may undermine the account proposed here, the coherentist conception successfully covers those cases that have been singled out as telling and important by philosophers. Since only little attention has been paid to the meaning of \textit{ad hoc}ness in recent decades, theories of confirmation and other more abstract debates within the philosophy of science, such as the realism debate, may have to be re-assessed.

References


