On Ad Hoc Hypotheses*

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In this article I review attempts to define the term "ad hoc hypothesis," focusing on the efforts of, among others, Karl Popper, Jarrett Leplin, and Gerald Holton. I conclude that the term is unhelpful; what is "ad hoc" seems to be a judgment made by particular scientists not on the basis of any well-established definition but rather on their individual aesthetic senses. Further, a hypothesis considered ad hoc can apparently be retroactively declared non–ad hoc on the basis of subsequent data, rendering the term meaningless.

1. Introduction. In 1887, A. A. Michelson and E. W. Morley performed an ingenious experiment to detect the motion of the earth relative to the luminiferous ether, then thought to pervade space and provide the medium through which light waves propagated, and which comprised a preferred frame by which one could define absolute space and rest. In the experiment, a beam of light was directed at an angle of 45° at a half-silvered mirror, so that the beam was half reflected and half transmitted through the glass. The pulses then traveled to equidistant mirrors that reflected them back to the half-silvered mirror, where they were again half transmitted and half reflected. A telescope was placed behind the half-silvered mirror to receive the returning pulses. If there were an "ether wind" due to the earth's motion around the sun, the halves of the two half pulses

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would arrive at slightly different times as a result of one pulse's having traveled perpendicular to the direction of the "ether wind" and back and the other's having traveled parallel and back. This would manifest itself in an interference pattern in the light.

However, the velocity Michelson and Morley measured for the "ether wind" was essentially zero (Michelson and Morley 1887). This, the famous "null result" of the Michelson-Morley experiment, was explained acceptably only with the advent of special relativity, introduced by Albert Einstein in 1905.[1] There was no velocity measured relative to the ether because, according to special relativity, there is no ether; with breathtaking audacity, Einstein banished the very concept, writing, "The introduction of a 'light ether' will prove to be superfluous" (1905/1998, 124). However, in the interim, George FitzGerald (in 1889) and Hendrik Lorentz (in 1895) independently suggested another explanation as to why the velocity of the earth relative to the ether was not detected: the length of the arm of the equipment parallel to the ether wind shrank, because of an electrical effect caused by the ether, by exactly the right amount to produce a null result.

The suggestion of the FitzGerald-Lorentz contraction (FLC) has been held up as the very essence of an ad hoc hypothesis in science. For instance, Karl Popper (1959, 83) wrote, "An example of an unsatisfactory auxiliary hypothesis would be the contraction hypothesis of Fitzgerald and Lorentz which had not falsifiable consequences but merely served to restore agreement between theory and experiment." Indeed, Lorentz commented similarly on his own theory, saying that "surely this course of inventing special hypotheses for each new experimental result is somewhat artificial" (quoted in Leplin 1975, 313).

Few things are spoken of more derisively in science or philosophy of science than ad hoc hypotheses. Yet, what is really meant by "ad hoc"? The term means "to this [specific purpose]," and at first blush, this seems to describe very well FLC, something "cooked up" to explain the null result of the Michelson-Morley experiment. But surely all scientific hypotheses are generated for some purpose, to explain something or another that has been observed about nature. Further, restoring agreement between experiment and theory is done all the time in science. Paul Feyerabend (1970, 63–69) argued for the "progressive" role of ad hoc hypotheses. And Hilary Putnam (1977, 432–33) writes, "It is possible to make ad hoc alterations in one's beliefs without being unreasonable." He continues that "the assumption that [some] stars have dark companions is ad hoc . . . the assumption being made for the purpose of accounting

1. Gerald Holton (1988, 279–370) has pointed out, however, that there is no evidence to speak of that Einstein's motivation in formulating relativity was related to the Michelson-Morley experiment.

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for the fact that no companion is visible. The assumption is also highly reasonable."

So exactly what is meant by an ad hoc hypothesis? And do scientists really eschew them, or are they only "supposed" to?

2. What Is Ad Hoc?

Independent Testability. Popper (1965, 241) claimed that in order to avoid ad hocery, a "new theory should be *independently testable*," leading "to the prediction of phenomena which have not so far been observed." Carl Gustav Hempel (1966, 37) added, "Some genuinely testable theories, when found to be false, are still upheld by their admirers—for example by introducing *ad hoc* some auxiliary assumption, or by reinterpreting the theory *ad hoc* in such a way that it escapes refutation. Such a procedure is always possible, but . . . only at the price of destroying, or at least lowering, its scientific status."

Pace Popper, Stephen G. Brush in a series of case studies has found scant evidence that scientists give greater weight to novel predictions than they do to "retrodictions"; this was true even in the case of the quintessential example Popper cited as the way science "should" work: the dramatic, novel prediction of light bending in general relativity theory, observed by A. S. Eddington in 1919 (Popper 1965, 34). In evaluating general relativity, Brush found, scientists typically gave equal or more weight to its success in explaining the advance of perihelion of Mercury—a phenomenon first documented by U. J. J. Leverrier in 1859. As Brush puts it, "Because the Mercury discrepancy had been known for several decades, theorists had already had ample opportunity to explain it from Newtonian celestial mechanics and had failed to do so except by making implausible *ad hoc* assumptions. Einstein's success was therefore immediately impressive" (1995, 138). Those who did look favorably on the success of the light bending prediction did so without reference to the fact that the prediction was novel (see Brush 1989, 1999).

But it turns out that FLC did have independently testable consequences after all. Jarrett Leplin (1975, 315) gives three examples that problematized FLC, different from the Michelson-Morley type: the Rayleigh-Brace, Trouton-Noble, and Kennedy-Thorndike experiments. Thus FLC would not be ad hoc by the standard (Popperian) definition.[2]

2. If an auxiliary hypothesis turns out to make a novel prediction about which the progenitor of the idea was not aware, should it still be considered ad hoc? Adolf Grünbaum (1964, 1409) said no, calling such a thing "*psychologically ad hoc*." Thus Grünbaum proposed the idea that things can be ad hoc in some senses and not in others. However, Leplin and others found the notion of "psychological" ad hocery

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Unobservable in Principle? Mary Hesse (1961, 228) wrote that FLC was ad hoc not only because it was not independently testable but "because it entailed that motion in the aether is in principle unobservable." The reason is that if the arm of the apparatus shrank, any measuring rod used to measure it would ostensibly also shrink by precisely the same amount. This form of unobservability would be in contrast to something's merely being unobservable in practice (perhaps because of limitations in current technology).

The difficulty with this view lies in declaring that something is, in fact, unobservable in principle. Setting aside that scientists may disagree on which of the two kinds of "unobservable" a certain hypothesized entity or situation might be, technology and human inventiveness can increase, and have in the past increased, the range of what is observable to us, and there is no reason to believe that they will not do so even further in the future. It is hard to imagine being able to draw a definitive line between what we believe must (forever) continue to be unobservable and what is unobservable now that eventually may be observed.

For instance, Aristarchus of Samos (ca. third century BCE) and Nicolaus Copernicus (ca. 1512) hypothesized that the stars were at staggering distances away from the earth to explain away the (apparent) absence of stellar parallax predicted by their heliocentric cosmologies; this would make the parallax predicted by the models, while present, unimaginably minuscule and thus invisible to us. This surely must have been considered "unobservable in principle" during their eras. When the telescope was invented around 1608, however, suddenly, the "unobservable in principle" was no longer so, and finally stellar parallax was indeed observed (by Friedrich Bessel in 1838); Aristarchus and Copernicus had been correct. And, as Leplin points out, FLC, *pace* Popper and Hesse, was independently testable in at least three different ways, making predictions that were observable in principle; thus Hesse's definition of ad hoc falters on the very example it was invoked to address.

Given the theoretical difficulties with the concept of "unobservability" and the examples of things that once seemed unobservable "always and forever" that were indeed observed with later technologies and techniques,[3] linking any definition of ad hoc to such conditions seems problematic.[4]

untenable, and Grünbaum apparently recanted following criticism from Hempel (Holton 1988, 329).

3. The declaration of Auguste Comte (1853, 138) that "we can never learn . . . [the] internal constitution" of the distant stars and similar statements by him particularly come to mind.

4. I am grateful to an anonymous referee for encouraging me to explore this line of thought.

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"Interestingly Different" Predictions. Hempel also pointed out a problem with the Popperian definition of ad hoc. Suppose that there exists a theory *T* that is "saved" from an observation *F* by tacking onto it a hypothesis *H* to make a new theory *TH*. If the only difference between *T* and *TH* is *F*, according to Popper, *H* should be considered ad hoc. Thus "all observational consequences of *T other than F* must be *identical* with those of *TH*" (Grünbaum 1964, 1410). However, Hempel suggested imagining minimally different "variants" of the experiment that produced *F*. Then any variant would also give different (albeit minimally so) predictions for *T* and *TH*. Thus *T* and *TH* would not produce exactly the same result in every other possible experiment as the one that produced *F*, and thus *H* would be non–ad hoc under Popper's definition. Hempel then suggested a modification of the definition of ad hoc: "An auxiliary [hypothesis] which enables a theory . . . to explain an [embarrassing] result in conjunction with [the hypothesis] is *ad hoc* if it does not have any observational consequences that are *significantly or interestingly different from the* [*embarrassing*] *result*" (Grünbaum 1964, 1410). Thus Hempel's definition of ad hocery seems to hinge on a subjective judgment on what the definition of "significantly or interestingly different" might be.

Ad Hocery by Comparison. Eventually, Popper retracted his charge of nonfalsifiability against FLC but still maintained that FLC was "less testable" than special relativity, illustrating "*degrees of ad hocness*" (1959, 83). This implies that ad hocery might be not an intrinsic property of a hypothesis but rather something defined by comparison. Hempel also seemed to have a comparative basis for ad hocery in mind when he wrote, "if more and more qualifying hypotheses have to be introduced . . . the resulting total system will eventually become so complex that it has to give way *when a simple alternative conception is proposed*" (1966, 30; italics added).

Imre Lakatos (1970, 175) went further, saying that a theory "may predict novel facts some of which may even be corroborated. . . . Yet one may achieve such 'progress' with a patched up, arbitrary series of disconnected theories. Good scientists will not find such makeshift progress satisfactory"; this situation he called a degenerating research program. Thus good theorizing, as opposed to ad hocery, requires building up a coherent track record; a sequence of theories is required, involving a kind of comparison on the scientist's part. A criticism of Lakatos's idea would seem to be that one cannot tell whether the "track" one is on is a degenerating one until after the fact, but Ian Hacking (1983, 121), among others, does not see this as problematic.

However, making judgments about ad hocery only in retrospect could seem to lend itself to the worst kind of Whiggishness. As Hempel warned,

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"with the benefit of hindsight, it seems easy to dismiss certain scientific suggestions of the past as ad hoc hypotheses, whereas it might be quite difficult to pass judgment on a hypothesis proposed in a contemporary context" (1966, 30). "When the theory is first proposed," Ernan McMullin (1984, 30–31) agrees, "it is often difficult to tell whether or not it is ad hoc on the basis of other criteria of theory appraisal." Lindley Darden (1992, 262) writes, "What counts as a legitimate addition to the theory and what is an illegitimate ad hoc change may be a matter of debate"; she goes on to give examples of this phenomenon in the development of both the gene concept and the provirus hypothesis, the latter of which (according to Robert Gallo) was "met with almost uniform incredulity" and "ridiculed" (Darden 1995, 147).

Consider again the example of Leverrier. It had been noticed by the 1840s that Newtonian gravity theory predicted an orbit for the planet Uranus that disagreed with observation. Leverrier hypothesized that an eighth planet, further out, was gravitationally perturbing Uranus's orbit. His analysis of the orbit and (successful) prediction of Neptune's location were hailed as a triumph. Yet in 1859, Leverrier applied the same stratagem to the planet Mercury, hypothesizing that a planet closer in to the sun ("Vulcan") was responsible for the deviations in Mercury's orbit described earlier. There was and is no such planet; the deviations in Mercury's orbit are due to gravitational effects explained by Einstein's general theory of relativity. Leverrier's Vulcan gambit is now seen as an ad hoc hypothesis (witness the language in the Brush quotation earlier), whereas his prediction of Neptune—using the same methodology in the same time period—is viewed favorably.

With FLC, some scientists had more of a stomach for the hypothesis's ad hoc nature than others. Einstein found the hypothesis "objectionable," saying that "this assumption, introduced *ad hoc*, appeared however to be an artificial means to rescue the theory" (quoted in Holton 1988, 352; Holton's translation). Henri Poincaré and Lorentz, however, while recognizing the somewhat artificial nature of the idea, took the hypothesis more "seriously, considering its 'ad hocness' compensated by other advantages" (Leplin 1975, 332).

A similar situation arose regarding the steady-state cosmology, proposed in 1948 by Hermann Bondi, Thomas Gold, and Fred Hoyle, which suggested that the universe possessed the same large-scale appearance and structure eternally, a condition they called the "perfect cosmological principle" (PCP; Bondi and Gold 1948; Hoyle 1948). This cosmology was in contrast to what eventually became known as the "big bang" model, which suggested that the universe evolved from a hot, dense, early state. The most controversial aspect of the steady-state cosmology was its hypothesis of the "continual creation of matter" ex nihilo, which was required to

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maintain a constant density for the expanding universe, as a varying density for the universe would violate the PCP. As such a process had never been observed (and in fact would occur at a rate—one hydrogen atom in a liter in a trillion years—that would be difficult to observe), many scientists considered the continual creation hypothesis to be unacceptably ad hoc, especially considering that it violated the conservation of matter.

An early adherent and promoter of the steady-state theory was Dennis W. Sciama. Sciama, however, explicitly stated that one of the main reasons he was attracted to the theory was its lack of ad hocness. For Sciama, the continual creation hypothesis "seemed less weird . . . than the big bang, where everything is created in one go. If creation is occurring all the time, it becomes a scientific process you can study because it's repetitive. . . . So it seemed to me attractive and the big bang was an awkward thing" (interview with George Gale, January 7, 1990).[5]

Big bang proponents, Sciama said, could explain any behavior in their model by chalking it up to arbitrary initial conditions or "evolutionary effects" that could be tailored to fit the observations. He criticized theories of galaxy formation in the big bang model because they were predicated on the coalescing of gas in specific ways (1959, 187). Similarly, Sciama criticized the theory of the origin of the heavy elements within the big bang model: "Thus it is possible to find an initial temperature [in the early universe] which is favourable for making heavy elements, and then one simply has to assume that this was the initial temperature" (1955, 42).

However, as the 1950s and 1960s went on, more and more reliable data started coming in contradicting the steady-state model. The three main discoveries in question were Martin Ryle's "3C" survey of celestial radio sources, Maarten Schmidt's discovery of the quasars, and the 1965 discovery of the cosmic microwave background radiation (CMBR) by Arno Penzias and Robert Wilson. The radio sources and quasars were distributed in ways that violated the PCP; namely, they were more abundant at great distances (and therefore more abundant in the distant past). And if there were never a big bang—as posited by the steady-state cosmology—there should be no space-filling blackbody radiation to start with.

For several years, Sciama undertook a program of attempting to "save" the steady-state model from these data. He proposed, for instance,

5. Per the discussion earlier, this indicates that Sciama viewed continual creation as being unobservable "in practice" but that one day it could be studied. Other scientists considered the vanishingly small rate at which this would happen to be so rare as to constitute being unobservable altogether.

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that the reason for the seeming excess of distant radio sources was that we were in a "local hole" of nearby galactic sources, making it only seem that there were more at greater distances, only apparently violating the PCP (Sciama 1963, 1964). Regarding the quasars, while conceding that perhaps many or most quasars were at cosmological distances, Sciama proposed that further data might show that other quasars were indeed local, once again saving the PCP (Sciama 1966a; Sciama and Saslaw 1966). And he also proposed an astrophysical mechanism that could cause an effect that mimicked the blackbody nature of the CMBR (Sciama 1966b).

Each of Sciama's hypotheses made a novel prediction and thus would not qualify as ad hoc under the standard definition. Though Sciama acknowledged that these hypotheses were a stretch, he considered proposing them by no means to be inappropriate. Of his "local hole" hypothesis, he said, "Although slightly artificial, I felt the price wasn't too high for the virtues of the steady state theory."[6] And of his alternative explanation for the CMBR, Sciama recalled, "I made a model . . . of a new kind of radio source. It was a perfectly reasonable object, whose integrated emission would simulate what had then been observed of the microwave background" (interview with Gale).

At the same time, Sciama was critical of Hoyle and others who, in his view, went too far in adopting the same strategy, for instance, proposing a "hole" in the distribution of the radio sources that was exceedingly large or insisting that it was impossible that any quasars could be at cosmological distances: "I could've said, following various people like Hoyle and [Geoffrey] Burbidge . . . that maybe the quasar redshifts are not cosmological. . . . But I felt that was piling things on too much. My instincts were no, that's unreasonable" (interview with Gale). And by the time Sciama reluctantly abandoned the steady-state model in 1966 (largely as a result of increasingly robust data on the distribution of the quasars), he became even more critical of the attempts of Hoyle and others to rescue what he saw as a discredited theory.[7]

For Sciama, like Lorentz and Poincaré before him, it was acceptable to employ "slightly artificial" hypotheses to save his theory—at least for a time. But Sciama considered more radical suggestions to save the steady state too contrived to be taken seriously—increasingly so as the data became more reliable; what was unacceptably ad hoc was a matter of degree. His ultimate abandonment of the steady-state model would seem

6. Tape-recorded interview with Spencer Weart on April 14, 1978. The transcript is available from the Center for the History of Physics, American Institute of Physics, College Park, MD.

7. Helge Kragh (1996) has chronicled the cavalcade of variations on the steady-state cosmology made by Hoyle to accommodate new data.

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to have been based on an evaluation of the theory's increasingly poor "track record," and that in turn was in some sense made in relation to the rival big bang model. Degrees of comparison were going on in his scientific process. Yet Hoyle and others went through a similar process, instead becoming quite recalcitrant; though their processes were similar, Sciama's and Hoyle's scientific judgments about "how much was too much" were quite different.

3. Leplin's Approach. As the FLC case still remains in the minds of many the quintessential case of ad hocery in science, Jarrett Leplin argues for using the data from this historical case to develop criteria for ad hocery rather than defining the term in advance on the basis of more abstract philosophical characteristics.[8]

Leplin concedes that between relativity and the electron/ether theory that underlay FLC there was no *experimentum crucis* to which to appeal and that the two were mathematically equivalent. "Given such experimental parity, we might look to aesthetic and pragmatic considerations to decide between competing theories," he writes. "But the advantages and disadvantages of the electron theory and special relativity in this regard are apparently offsetting" and as a result cannot be invoked as criteria for why relativity emerged victorious (1975, 311–12).

Instead, on the basis of the FLC case, Leplin (1975, 336–37) suggests five criteria that must be met for a hypothesis to be ad hoc in scientists' eyes: "An hypothesis H introduced into a theory T in response to an experimental result E is *ad hoc* if and only if:

(1) E is anomalous for T but not for T as supplemented by H.
(2) E is evidence for H but
  (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,

8. Wryly, Leplin (1975, 314) notes that philosophers' back and forth on the logical status of FLC itself resembles a degenerating research program, "many of whose entries are patently *ad hoc*" themselves.

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(*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."

None of these points include any of the familiar concepts of independent testability or novel predictions that are usually referenced to define ad hocery. The thrust of Leplin's argument (point 5) is that charges of ad hocery are "directed primarily at the theory, and only indirectly at the particular hypotheses proposed as supplementation" (1975, 320). A charge of ad hocery is thus really about fundamentality.

Leplin's fifth criterion also relies then on a kind of track record; auxiliary hypotheses are viewed in the context of "other problems" facing the theory. It also seems to rely on scientists' intuitions regarding what is a "satisfactory" solution to a problem and when there is "good reason" to believe a proposition. Thus "*ad hoc*ness will be a subject of uncertainty and dispute" since there is no universal way to parse Leplin's fifth criterion (1975, 345).

Another interesting aspect of Leplin's reformulation is found in his third criterion; namely, a hypothesis is considered ad hoc if there are no grounds to believe either that it is true or that it is false. Thus if further evidence is uncovered later on that supports the hypothesis or if the qualms about the overall theory's fundamentality are assuaged, the hypothesis may retroactively be declared non–ad hoc.[9]

Leplin then applies his definition to the discovery of the neutrino. In 1911, Lise Meitner and Otto Hahn showed that there were difficulties in physicists' theories of beta decay, seeming to indicate a violation of the conservation of energy. Wolfgang Pauli in 1930 proposed that the missing energy in beta decay might be accounted for if a hitherto-undiscovered particle (ultimately called the "neutrino") was (also) emitted in the reaction.

Leplin points out that at this stage, the only subatomic particles that were agreed certainly to exist were protons and electrons. The neutrino proposal would then have increased the number of particles in the bestiary by 50%, which many viewed as a radical idea. Scientists described this hypothesis as "'unlikely', 'unsettling', 'strange', 'artificial', 'outlandish' . . . [and] 'desperate'" (Leplin 1975, 338).

However, Leplin argues that the reason for such reactions was due not to the neutrino hypothesis itself but rather to larger theoretical issues.

9. Holton (1988, 328) points out that while FLC initially "was clearly and quite blatantly ad hoc," considered so even by its creators, FitzGerald considered FLC to be more and more legitimate as time went on.

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Difficulties facing quantum mechanics had called into question the very concept of conservation of energy, with scientists suggesting that perhaps energy conservation applied only in the statistical aggregate; the Bohr-Kramer-Slaters theory of 1924 is perhaps the best example of this thinking. Many saw the neutrino hypothesis as FLC had been seen: a "band-aid" to cope with an embarrassing datum rather something that cut to the root of the problem. This included P. A. M. Dirac, who saw the beta decay problem as symptomatic of "the problem of constructing a satisfactory relativistic quantum mechanics" (Leplin 1975, 340–41).

Of course, the verdict of the community on neutrino hypothesis was later reversed. Leplin writes that "the discovery of important theoretical uses for the neutrino other than in the problem of beta-decay, and the failure of the view that rigorous conservation laws prevented the relativization of quantum mechanics, were primarily responsible for the neutrino's eventual acceptance." He says that this reconsideration was "a gradual process, and it is difficult to fix a point at which the neutrino's theoretical utility became sufficient in strength and diversity to overcome its initial *ad hoc* character" (1975, 345).

Note the colorful language directed at both the FLC and neutrino hypotheses. Just as aesthetic factors play a significant role in science writ large, surely they play a role in determining whether or not a hypothesis is considered ad hoc. Holton (1988, 321, 326) agrees, writing, "How is one to decide whether an hypothesis is ad hoc or not? And, moreover, whether it is repulsively ad hoc or acceptably so? It is here that we connect with Einstein's criterion of the 'inner perfection' of the theory. The criterion is the feeling for the 'naturalness' of 'logical simplicity' of the premises. . . . What is important [is] the scientist's feeling of ad hocness. . . . This criterion of choice is familiar to every working scientist." His definition, as before, depends on a "feeling." Ad hoc hypotheses "felt" to be acceptable are described as "plausible," "likely," "bold," or even "elegant" whereas unacceptable ones are described as "artificial," "arbitrary," "contrived," "strange," or "ugly" (Leplin 1975, 314; Holton 1988, 327).

The Sciama case illustrates this. Sciama disliked the big bang model for what he considered its ad hoc assumptions, yet he did not see the steady state's hypotheses as being ad hoc; other scientists had precisely the opposite view. Similarly, how far Sciama was willing to go to save his theory—and when others went too far—seems also to have been a matter of personal judgment.[10]

10. In 1960, Sciama wrote that the steady-state picture provided a way in which the "*actual* behaviour of the universe can be accounted for without ad hoc assumptions" (10). But later in his 1978 interview with Weart, he acknowledged that he recognized that the steady-state model was getting perhaps more ad hoc as the hostile data came in and that he was "preparing [his] mind for the trauma that came along."

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The fact that one cannot describe this aesthetic evaluation process of ad hoc hypotheses in strict, logical terms does not bother Holton. He writes, "So-called philosophical mastery must be supplemented by an understanding of matters of scientific taste and feeling. Otherwise . . . it can lead . . . a person to scold an Einstein for not having behaved like an obedient student in the classroom of a logician" (1988, 332).

James McAllister's concept of aesthetic induction suggests that theories are seen as "beautiful" or "ugly" on the basis of an aesthetic canon held at a given time by a community. But the criteria in this canon can change over time; the characteristics that make a theory ugly are soon seen as not so if the theory starts to amass a successful experimental track record (McAllister 1996). Is this not akin to what Leplin suggests? The neutrino hypothesis was cleansed of its original sin of ad hocery as a result of changing standards of what was acceptable in scientific theorizing (an abundance of particle types rather than a few, the preservation of strict energy conservation, etc.), which in turn was based on a track record of empirical success.

4. Summary. Leplin's proposed definition of ad hocery raises significant questions. First, it seems to include an inherent element of aesthetic valuation. As scientists' aesthetic sense or "feeling" governs their judgments in this matter, this will manifest itself in different ways. But this should come as no surprise; aesthetics play a significant role in science. It thus makes sense to see scientists also using aesthetics to adjudicate matters in a specific subset of science—evaluating ad hocery—as well.

Second, Leplin suggests that a community's judgment about what is ad hoc can change retroactively, as apparently happened in the case of the neutrino, and this is based on data later collected that support or refute the basic tenets of the theory in question. If this is the case, then it is not clear what value there is in ever worrying about whether a hypothesis is ad hoc or not. How is this retroactive judgment on ad hocery any different from the distinction between a hypothesis's being validated by the community (i.e., being associated with a theory that is "right" as opposed to one that has been ruled "wrong")? If there is no distinction between right/wrong and ad hoc/non–ad hoc, then is there any use for the term ad hoc at all?

This is not to say that all hypotheses are created equal. A more fruitful concept for evaluating hypotheses might be to use notions of being "warranted" or "unwarranted"; looking at historical case studies might provide a useful way to evaluate what types of hypotheses have been successful in the past and thus might be warranted. Darden (1987, 36), drawing on the work of N. R. Hanson, suggests something akin to this when she writes that scientists "find types of hypotheses proposed in the past, and

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analyze the nature of the puzzling phenomena to which the type applied" and then "use this 'compiled hindsight' in future instances of theory construction."

Considering all this, it seems to me that hand-wringing by historians, philosophers, and scientists over whether or not a hypothesis is or was ad hoc (and therefore "illegitimately proposed") is energy wasted. If what is ad hoc depends on an aesthetic sense (and is thus largely in the eye of the beholder) and, furthermore, can change after the fact, I suggest that the usefulness of the term "ad hoc hypothesis," if it ever had any, has come to an end. At the end of the day there seem to be no ad hoc hypotheses and no non–ad hoc hypotheses, only hypotheses—full stop.

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