Invoking the Tooth Fairy Twice, or How to Identify Cases of Ad Hoc Hypothesis Acceptance

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ABSTRACT

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(Under the direction of John T. Roberts)

The light in the refrigerator comes on every time that you open the refrigerator door. Svetlana says that it turns on because of a little man that lives inside. When you ask Svetlana why we never see or otherwise detect the little man, she responds that it must be that the little man is invisible, inaudible and has no mass. A frustrated observer might justly label these posits concerning the little man ‘ad hoc’. Yet, what does this charge of ad hocness amount to? What makes something ad hoc? My dissertation sets out to answer this question.

Previous philosophers of science have unsuccessfully attempted to answer this question: for instance, they have explained ad hocness in terms of unfalsifiability, or in terms of the absence of additional test implications, or in terms of the intentions of scientists modifying theories in the face of recalcitrant evidence. What is different about my approach is that, unlike its predecessors, it successfully avoids the Quine testability problem, while still successfully diagnosing ad hocness in our canonical examples of the phenomenon. In this account, ad hocness is equated with jumping to conclusions: adding hypotheses into a theory in order to deal with disconfirming evidence when such an addition is not otherwise warranted.
To my family
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Chapter One
Introduction: How Do We Determine When Something is Ad Hoc?

I. The Big Bang Theory, Dark Matter Theory and the Problem of Ad Hocness

The Big Bang Theory is now widely accepted by scientists as the explanation of the beginnings of our universe. This theory claims that, at the beginning of our universe, about 12 to 14 billion years ago, all matter was in a very small volume\(^1\). This volume began rapidly expanding. The universe that we see now is a result of this expansion. The theory postulates that the velocity of expansion was greatest at the time of the Big Bang and has been slowing down ever since. Two of the reasons why this theory is so popular are that it predicts correctly (1) that Helium and Hydrogen are the predominant elements in the universe and (2) the specific proportions of these elements in the universe. (Freedman Kaufmann 648-651) It also helps explain why observations that led to Hubble’s law\(^2\) indicate that the universe is expanding, when, previously, many scientists had thought that the universe was static – neither expanding nor contracting.

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\(^2\) Hubble’s Law is a statement of the direct relationship between the distance to a galaxy and the redshift of its spectral lines. The greater the distance of a galaxy, the greater its redshift and the greater the redshift, the greater the galaxy’s velocity will be. The law can be formulated in the following manner: \(v = H_0d\), \(H_0\) being the Hubble constant and \(v\) being the velocity of a given galaxy and \(d\) being the distance of that galaxy from the Earth. (Freedman and Kaufmann 641) (Freedman and Kaufmann 598-599)
However, there are two problems that have arisen with respect to the predictions of this theory: one is that the Big Bang Theory in combination with other theories and presumed knowledge seems to imply that the universe is too young to have formed the galaxies that we see now; the second is that the velocity of individual galaxies as they spin around their centers is very fast – much faster than previously thought. The velocity of a galaxy as it rotates around its center is proportional to the amount of mass and the average distance of this mass from the center. Yet, as in our galaxy, for example, the orbital speeds of objects and gas clouds on the distant edges of a galaxy are about the same as those for objects near to the center of rotation for a galaxy\textsuperscript{3,4}. The amount of observed matter and the amount of visible matter postulated by the Big Bang theory would indicate that galaxies ought not rotate in the manner that they do. So, it would seem that current theories plus current estimates of mass of galaxies leads to false predictions about how galaxies spin.

Maintaining the correctness of the Big Bang theory, this finding implies that there is a lot more matter in these galaxies than can be accounted for with the ‘normal matter’

\textsuperscript{3} So, for example, if the galaxy rotated like a cd does when being played, then the Sun and the stars would always keep the same position relative to each other – something that is not observed to happen. That the entire galaxy moves at about the same orbital speed explains, in part, why the observable heavens do not stay constant for us over time. (573, 575)

\textsuperscript{4} A concise alternative characterization of the problem: “Virtually all observed spiral rotation curves are flat (or rising) to the limits of the observations, implying that the mass density decreases approximately as $1/r^2$. The luminosity however falls off faster, exponentially. Thus, the ratio of mass density to luminosity density increases with radial distance, requiring the presence of a halo of dark matter, which becomes increasingly dominant with increased nuclear distance.” Rubin, Vera C. “Galaxy Dynamics and the Mass Density of the Universe.” \textit{Proceedings of the National Academy of Sciences of the United States of America}, Vol. 90, No. 11 (June 1, 1993), 4814-4821. This quote can be found on p. 4815
that was created in the Big Bang. It is even more matter than can be explained by so-called dim matter: the matter that constitutes dwarf stars and planets that does not emit much radiation and, so, cannot be detected by our current instruments. According to the Big Bang Theory, there cannot be enough of this matter to explain the phenomena that scientists have observed; these types of objects would not have nearly enough mass to explain why the outer edges of a galaxy have the orbital speeds that they do.² (Freedman Kaufmann 574-575) In order to save the Big Bang theory in the face of disconfirming evidence without modifying their other theories, scientists needed to postulate the existence of a large amount of non-visible, exotic matter: dark matter, so named because it does not emit or absorb light. If the matter did either of those things, we would have noticed it before now.

Postulating dark matter, as well, can solve the first problem - that the universe is too young to have formed the galaxies within our universe. If there were large amounts of neutral matter, besides normal matter, found in the universe, this neutral matter would have sped up the formation of galaxies. This is because, unlike normal matter, which had to change from a plasma of particles kept smooth by radiation into a neutral gas which would allow fluctuations in the density of the matter necessary for the formation of galaxies⁶, this dark matter was already neutral and, so, could have started contracting

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² A similar problem is found when looking at our observations of the orbital speed of galaxy clusters.

⁶ The transformation from a smooth plasma to complex structures is concisely stated in “Extracting Primordial Density Fluctuations”:
   “Shortly after the Big Bang, the universe was smooth to a precision of one part in 10⁵. We can measure this smoothness in cosmic microwave background (CMB) radiation –
earlier, speeding up the galaxy forming process. This matter, as noted above, would also
explain why it is that there seems to be a lot more matter in individual galaxies than it
appears: the matter that helped speed up the galaxy-forming process is still found within
these galaxies.\footnote{As stated in Rubin:}

To solve these problems, an enormous amount of dark matter has to be
postulated: according to some views, it makes up as much as 95% of the total mass of our
universe. This is a very surprising claim and it is made in order to get the Big Bang
Theory out of trouble.

Isn’t it rather strange that scientists began to postulate dark matter \textit{because} of the
mass discrepancies that they were observing? Certainly, the Big Bang Theory has been
considered a success but, perhaps, it is ultimately incorrect? Or, notice that the scientists’
calculations of the velocity of galaxies are based on the laws of gravitation – perhaps

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photons that provide us with a record of conditions in the early universe, because they
were last scattered about 300,000 years after the Big Bang. To a remarkably precise
degree, the early universe was characterized by isotropic homogeneous expansion.
However, temperature fluctuations have been measured in the CMB, and complex
structure surrounds us. There is a simple connection: The seeds of large-scale structure
were infinitesimal density perturbations that grew through gravitational instability into
massive structures such as galaxies and galaxy clusters.” \cite{1405}


\footnote{As stated in Rubin:}

“Nearly 60 years ago, Zwicky and Smith discovered that individual galaxies in the
Coma and Virgo clusters of galaxies have velocities so large with respect to the mean
cluster velocity that their mutual gravitational attraction would not be sufficient to bind
the clusters. Hence, either clusters are expanding and galaxies are dispersing (contrary to
observation) or the total mass in the clusters is larger than that implied by the luminous
material. Zwicky called this the “missing mass.” For many years, this conclusion was
relegated to the “things we don’t understand” file and assumed to be a mysterious
property of galaxy clusters. Not until we discovered the need for dark matter in
individual galaxies did these early observations join the mainstream of astronomical
research.” \cite{4819}
these laws are incorrect? Is it possible that scientists postulated the existence of dark matter for no other reason than to save their theories of the creation of the universe and of gravitational laws when these theories were threatened by recalcitrant evidence? Does this make postulating dark matter suspect or, somehow, problematic? Is not the postulation of dark matter an example of what scientists and philosophers call an *ad hoc hypothesis*?

II. Planck and Blackbody Radiation

In 1900, Max Planck published “On the Theory of the Energy Distribution Law of the Normal Spectrum”\(^8\), where he addressed the problems encountered by the Rayleigh-Jeans proposal in explaining black body radiation, as well as those of Wien’s explanation\(^9\). All bodies emit radiation: as the temperature of the body increases, the total energy emitted by the body increases. The Rayleigh-Jeans proposal successfully predicted the energy emitted in radiation of the longer wavelengths but was not able to account for the energy emitted in radiation of shorter wavelengths. Wien’s proposal successfully predicted the shorter wavelengths’ energy but not the longer ones’. Both of these accounts were based upon the then-popular wave theory of light\(^10\). Planck’s account of blackbody radiation, on the other hand, was able successfully to account for all wavelengths of emissions.

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\(^9\) For a further explanation of Wien’s law, see Freedman and Kaufmann 102-103

\(^10\) See, for example, Young’s account of light.
Yet, this success came at a cost. In order to reconcile what people had postulated about electromagnetic energy with the observed data concerning the intensity of the radiation emitted by a black body at certain wavelengths, Planck had to introduce $h$ – now known as Planck’s constant. The introduction of $h$ did not seem to be scientifically justified: instead, Planck seems to have introduced it because its introduction made his mathematical formula get the same results as had been observed. Also, the equation seems to imply that energy itself is quantized, which, at the time, was not considered a viable position and, at the very least, did not have any direct evidence to back it up.

Does this example seem to be an example of an ad hoc hypothesis introduction? Max Planck, himself, seemed to think that it was. And, his motivation for its introduction was to save at least part of a certain theory of electro-magnetic energy against adverse evidence. On the other hand, this hypothesis was demonstrated to be, in fact, correct. Einstein, in his examination of the photoelectric effect, seemed to demonstrate that this energy is quantized. Planck’s hypothesis has also been exceedingly valuable when used as a part of other scientific endeavors.11

How is this example similar to the dark matter example? Well, in both, something seems to be postulated in order to maintain the correctness of a given theory – Planck’s constant was postulated in order to maintain a certain theory of electro-magnetic energy, while dark matter was postulated in order to maintain dynamical laws12 and the

11 I return to this example in chapter 5, section X.

12 That is, Newton’s second law – “A change in motion is proportional to the motive force impressed and takes place along the straight line in which that force is impressed” (Newton 416) – and Newton’s law of gravitation – “planet B will gravitate in turn toward all the parts of planet A, and its gravity toward any one part will be to its gravity toward the whole of the planet as the matter of that part to the matter of the whole[...]The
Big Bang theory. Also, the existence of non-visible matter seems to go against the previously popular belief that all matter must, in principle, emit radiation. Analogously, the introduction of Planck’s constant implied that energy is quantized, against the then-popular wave theory of light.

How are these two examples different? Well, it might be argued that, in the dark matter example, later experiment has not yet validated it in the way that Einstein’s experiments involving the photoelectric effect, which he successfully explained in terms of quantized electromagnetic energy. Also, it may be argued, we know what these quanta are made of, while the composition of dark matter is still quite mysterious. Finally, Planck’s constant was introduced to help a well-respected theory of electromagnetic energy, while dark matter was introduced in order to help the Big Bang theory in conjunction with gravitational theory and acceleration theory. Are these theories on gravitation toward each of the individual equal particles of a body is inversely as the square of the distance of places from those particles.” (Newton 811)


Or, a modern translation: the second law: \( F = ma \), where \( F \) = net outside force, \( m \) = mass of object and \( a \) = acceleration of object. (Freedman and Kaufmann 80) Universal gravitation: \( F = \frac{Gm_1m_2}{r^2} \), where \( F \) is the gravitational force between two objects, \( G \) is the universal constant of gravitation, \( m_1 \) and \( m_2 \) are the masses of the two objects and \( r \) is the distance between the objects.

One place in which Einstein demonstrates the need for quantized electromagnetic energy is in “On a Heuristic Point of View Concerning the Production and Transformation of Light.”

the same footing as electro-magnetic energy theory, or the Big Bang theory? We might argue, “No”.

In fact, the initial modern explanation of gravitation might be considered by some to be ‘ad hoc’. Newton himself did not consider his own law of gravitation entirely satisfactory. He was uneasy about the need to postulate ‘action at a distance’ – the idea that a body exerts force on another body while there is no physical contact between the two bodies. Newton denied the existence of action at a distance in all other physical interactions and, so, the need to postulate it in the case of gravity seemed a bit unsatisfactory. One of the criticisms leveled against Newton was, in fact, the need to postulate action at a distance for universal gravitation. While gravitational theory has been revised since Newton, the problem of action at a distance still remains. And, as has been noted by Mordehai Milgrom14, an opponent of dark matter theories, two major changes have already been made to Newton’s dynamics: “[t]he first upgraded Newtonian dynamics to the theory of relativity – both the special theory (which changed Newton’s second law) and the general theory (which altered the law of gravity)[…] The two proven extensions of Newtonian dynamics come into play under extreme conditions, such as extreme speeds (special relativity) or extremely strong gravity (general relativity).”15 He

14 Milgrom’s MOND theory is the main challenger to dark matter theories. Interestingly, there have been new studies done on the orbits of stars, using observations from the Sagittarius stream (the remnants of the Sagittarius dwarf elliptical galaxy which has been captured by the Milky Way galaxy), that seem to match the predictions of MOND theory.


goes on to note that another extreme condition might well be extreme size, and galactic scales would certainly fall under this heading.

As we’ve noticed in the last two examples, hypotheses that have been labeled ‘ad hoc’ during some scientific period are hypotheses introduced when there has been a problem facing an accepted theory. There are other canonical introductions that have been discussed in the literature. We will turn to one of these next: specifically Ptolemaic astronomy.

Before we get to this example, a cautionary note must appear. With the forthcoming Ptolemaic example, as well as other examples that will arise, we will have to make sure to distinguish between thinking that a hypothesis is ad hoc and thinking that it is just wrong or false. Newton’s second law is a good example to use for this distinction. The second law – that the external force on an object is equal to its mass multiplied by its acceleration – does not hold for objects that are moving at rates near to the speed of light (3 x 10^8 m/sec). Until Einstein demonstrated this failure, however, the second law was thought to be one of the cornerstones of physical theory. The second law was demonstrated to be wrong but it was never really charged with being ‘ad hoc’. One of the concerns that arises when discussing potential examples of ad hoc introductions of hypotheses is how to distinguish between hypotheses that are introduced in an ad hoc manner and those that are simply wrong. I will return to this subject in more detail in section X of this chapter and in chapter 5, section III - especially footnote 13. Right now, while we are trying to compare putative instances of ad hoc introductions, it will be enough to keep that concern in the background of our minds.
I will now turn to another, canonical example of ad hocness: Ptolemaic astronomy. I will use this case to explicate my methodological account of ad hocness and to critique other accounts because the Ptolemaic astronomy case is far less complicated than the dark matter case and is one about which we have definite intuitions. I will leave the dark matter case as an interesting project for the reader.

III. Introduction to the Project and To Ptolemy

My project is to define a methodological test for determining whether a hypothesis acceptance is ad hoc. In order to demonstrate this test’s usefulness and its superiority to other proposed definitions of ad hocness, I will here present a theory that many philosophers have claimed used ad hoc modifications\(^\text{16}\) and that peoples’ intuitions

\(^\text{16}\) Thomas Kuhn, for example, speaks about Ptolemaic astronomy in The Structure of Scientific Revolutions. As he is interested mainly in “paradigm shifts” (his phrase), he cites Ptolemaic astronomy and the emergence of Copernican astronomy as one of the most famous paradigm shifts. That he is dealing with similar issues as this paper is clear from the following quote: “For some time astronomers had every reason to suppose that these attempts [solutions to minor discrepancies] would be as successful as those that had let to Ptolemy’s system. Given a particular discrepancy, astronomers were invariably able to eliminate it by making some particular adjustment in Ptolemy’s system of compounded circles. But as time went on, a man looking at the net result of the normal research effort of many astronomers could observe that astronomy’s complexity was increasing far more rapidly than its accuracy and that a discrepancy corrected in one place was likely to show up in another.”[italics mine] (Kuhn 68)

Kuhn later identifies the nature of these sorts of actions: “By themselves they [epistemological counterinstances] cannot and will not falsify that philosophical theory, for its defenders will do what we have already seen scientists doing when confronted by anomaly. They will devise numerous articulations and ad hoc modifications of their theory in order to eliminate any apparent conflict.” (Kuhn 78)

label as being modified in an ad hoc manner. The theory that I will present is that of Ptolemaic astronomy. I will then characterize why Ptolemy’s account does make ad hoc modifications, and I will also test other theories of ad hocness using this example. I claim that other theories of ad hocness either do not capture the sense in which the modifications Ptolemy makes are rationally impermissible, or do not characterize these modifications as ad hoc at all. The theories that I will be examining are Howson and Urbach’s and Michael Strevens’s versions of Bayesianism\(^\text{17}\), Jarrett Leplin’s account of ad hocness\(^\text{18}\) and Malcolm Forster, Christopher Hitchcock and Elliott Sober’s curve fitting theory\(^\text{19}\). First, however, let’s look at Ptolemy’s account.

IV. Ptolemy and Astronomical Motion


Ptolemy’s account of astronomical motion in *The Almagest* begins with Ptolemy’s eight main theoretical commitments: 1) the movement of the heavens is spherical; 2) the heavens themselves are spherical; 3) heavenly objects are spherical; 4) the Earth is spherical; 5) the Earth is the center of the heavens; 6) the fixed stars are so far away from the Earth that the Earth is like a point to them; 7) the Earth does not, itself, move in any way; 8) there are 2 motions to the heavens. (Ptolemy 7)

The movement of the heavens is spherical because, as Ptolemy says, the stars seem to be moving daily from the same initial starting point in the sky. If the movement were not spherical, we would not observe this phenomenon to occur. The movements of the planets are also, then, circular, as they are a part of the heavens. What Ptolemy means by a circular orbit seems to be an orbit that is circular in shape, where the heavenly body passes through equal arcs of the circle in equal times. (Ptolemy 86-87)

The heavens themselves are spherical because ether, of which the heavens are composed, has “the finest and most homogenous parts” (Ptolemy 8) of all bodies. As its parts are homogenous, its surface must be made of homogenous parts. The only 3-dimensional geometrical figure that fits this description is a sphere. Thus, the heavens must be spherical. (Ptolemy 8) Ptolemy then goes on to discuss why it is that heavenly objects are spherical. According to Ptolemy, heavenly objects are spherical because, if they were another shape, they would not look circular from all angles. That is, if heavenly bodies

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21 This analysis appears also to be supported by Ptolemy’s analysis of the composition of the heavens and the composition of heavenly bodies. “As they are made of homogeneous parts and, as a result, move circularly because things that are homogenous will move in a homogeneous manner, so too does it seem that their speed must also be homogeneous, never speeding up or slowing down.” (Ptolemy 8-9)
were some other shape, I might see them as circular from North Carolina while someone in Taiwan might see them, instead, as a narrow strip. This does not occur and, so, heavenly bodies must be spherical.

Ptolemy also asserts that the Earth is spherical. This is for several reasons. One reason that the Earth must be spherical is because, when we are sailing on a body of water, mountains ‘come up’ from out of the sea as we get nearer to a certain land mass. If the Earth were not circular, then we ought to be able to see the mountains from across the body of water, instead of them being revealed as we come closer to land. (Ptolemy 9)

Additionally, Ptolemy argues that the Earth must be spherical because of the differing times when people, located on different parts of the Earth, see eclipses. People in the East report proportionally later times of observance than do those in the West. These time differences are proportional to the distances between the places where observations were being made. If the Earth were anything other than a sphere, then the time differences of the reports would not be proportional to the distances between the observation locations. (Ptolemy 8-9)

Having discussed his first four commitments, Ptolemy goes on to discuss the reasons for his commitment to the Earth being in the center of the heavens. Where would the Earth be located if it were not located in the center of the heavens? It would either be on the axis of the heavenly sphere but closer to one pole or the other of the heavenly sphere, equidistant from the poles but not on the axis of the heavenly sphere, or neither on the axis nor equidistant from the poles. Ptolemy again uses the appearances of the heavens to motivate his rejection of these three alternative locations for the Earth. (Ptolemy 9-10) If the Earth were located closer to one of the poles, Ptolemy argues, then
the plane of the horizon would cut the heavens into two unequal parts. This cannot be happening, however, because half of the parts of the zodiac are always above the horizon and half are always below, no matter where you are on the Earth. If the Earth were located closer to one of the poles, then we would not expect to encounter such an even split between the parts of the zodiac that we did and didn’t see. (Ptolemy 10) If the Earth was not on the axis of the heavenly sphere, Ptolemy thinks, the equinoxes would not occur when they actually do, halfway between the solstices. Rather, an equinox would occur nearer to one solstice than to another because the Earth would be off the axis of the heavenly sphere. He claims that “[i]t is absolutely agreed by all, however, that these distances are everywhere equal because the increase from the equinox to the longest day at the summer tropic are equal to the decreases to the least days at the winter tropic.” (Ptolemy 9) As the increases are equal, the Earth must be found on the axis of the heavenly sphere. The appearances seem to contradict both the Earth’s being off the axis and the Earth being closer to one of the poles than the other, so the third option is also eliminated, it just being a combination of the first two options. So, Ptolemy thinks he has shown that the Earth must be in the center of the heavens.

Next, Ptolemy explains his commitment to the Earth’s having the ratio of a point to the heavens. He notes two things: firstly, the measurements of angular distances of the stars are the same, no matter where on Earth you might make the measurement; secondly, that, from every observation point on the Earth, the horizon cuts the heavens exactly in half. Ptolemy claims that this would not happen if the Earth were larger than as a point to the heavens. Instead, a point drawn through the center of the Earth would cut the heavens in half but lines drawn from the surface would not. (Ptolemy 10)
Then, Ptolemy asserts his reasoning for why the Earth does not fall in the way that other objects on Earth do. The Earth is a much greater magnitude, Ptolemy claims, than the heavy bodies that are found on it. The Earth’s greater magnitude absorbs the fall of these heavy bodies that are attracted towards its center. If the Earth were to fall, in proportion to its size as those of the heavy bodies, Ptolemy says, then the Earth’s movement would leave the other bodies behind, its movement being so much the greater. Thus, all the animals and objects on the Earth would be left in the air, with no Earth beneath them. (Ptolemy 11)

Ptolemy’s final assumption is that there are two movements in the heavens. The first movement is “that by which everything moves from east to west, always in the same way and at the same speed with revolutions in circles parallel to each other and clearly described about the poles of the regularly revolving sphere.” (Ptolemy 12) What sort of movement is Ptolemy thinking about with this description? It looks like he is talking about the way in which the heavenly bodies seem to all rise and set in the same direction, keeping their angular distances with respect to each other the same. (Ptolemy 13) However, there is another, additional movement. This movement is “that according to which the spheres of the stars make certain local motions in the direction opposite to that of the movement just described [the east to west movement] and around other poles than those of that first revolution.” (Ptolemy 12-13) This is a movement Ptolemy ascribes to the sun and the planets, in contrast to the fixed stars, which move only according to the first movement. Ptolemy has already asserted that planetary movement is regular, so he cannot ascribe non-uniform movement to explain this motion. Instead, he claims this
movement to be a regular movement, where this movement is on a “circle oblique to the equator.”

It is unclear whether Ptolemy created his theory of planetary motion with all of these assumptions in place, for it could be claimed that Ptolemy wrote the introduction of the Almagest after completely hammering down all the details and modifications of his theory. I do not wish to engage in historical debate over whether this is the case or not. Instead, suppose he did have all of these theoretical commitments. Suppose that he arrived at this theory on the basis of the evidence described and then some new evidence came to light. This new evidence seems to contraindicate the theory, forcing Ptolemy to then modify some part of his theory/auxiliary hypotheses. Are all of the modifications that Ptolemy makes non-ad hoc?

The case for which I will consider this question is Ptolemy’s treatment of the motion of the sun, as the sun is the first heavenly body whose apparent movement Ptolemy considers. The rest of the heavenly bodies whose orbits Ptolemy attempts to describe all build on the geometry he uses in describing the sun’s orbit.

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22 Imagine the sun, for example, getting carried about with the fixed stars in the heavenly sphere. While the sun is getting carried about in this manner, it is also traveling around the ecliptic (concentric to the location of the Earth, albeit at an angle to the equator) in the direction opposite of its 1st movement. If we were to hold the heavenly sphere fixed, the sun would be moving from west to east at an oblique angle to the equator.

23 In addition, the other heavenly bodies’ orbits admit of 2 anomalies instead of just one, while the sun’s admits of just the first. The first anomaly is described as the heavenly body appearing to describe unequal arcs in equal time around the ecliptic. The second is the zodiacal anomaly, (Ptolemy 272 fn.1) which explains why certain heavenly bodies appear to have retrograde motion. The first anomaly can be explained by either of Ptolemy’s 2 hypotheses that will be outlined, below. The second, he claims, can only be explained by the epicycle hypothesis. (Ptolemy 291) It has been Ptolemy’s exposition of the orbit of the moon and of the planets that has been especially criticized, previously: Copernicus attacks modifications made for the moon’s orbit - that the epicycle need not
The sun, as well as the other heavenly bodies, is thought to move at a regular pace, never speeding up nor slowing down. This is because the sun is posited to move in a perfectly circular orbit. As Ptolemy states this: “the straight lines, conceived as revolving the stars or their circles, cut off in equal times on absolutely all circumferences equal angles at the centres of each” (Ptolemy 86) As a result, the sun’s movement ought to be observed as completely regular. Ptolemy notes, however, that in the data collected both by Hipparchus and by himself, the sun does not appear to move completely regularly. Instead, it seems to have an inconstant speed – that is, it does not seem to describe equal arcs in equal time. Its seasonal movement back and forth speeds up and slows down.24

However, Ptolemy still maintains that all movements of the heavens are circular. Thus, this motion of the sun, the one causing its apparent anomaly, must also be perfectly circular, though it makes the sun’s movement seem imperfectly circular25. The way that he explains this apparent anomaly is through the use of either epicycles or eccentrics. As Ptolemy states:

move around the center of the circle it moves on as long as it moves regularly around a point near the Earth [the equant] (Copernicus 677) - and others have attacked the amount of epicycles and eccentrics needed for some of the planets. However, I wish to concentrate on Ptolemy’s first steps, to see if his initial commitments were warranted. I discuss Ptolemaic theory as a series of added epicycles in chapter 2, section VI, as well as in chapter 3, section VI, and in chapter 5, section XI. I also allude to it in chapter 4, section II.


24 From now on, I will abbreviate this phenomenon to ‘the sun’s anomaly’.

25 By the sun’s movements being imperfectly circular, I mean that the sun appears to pass through unequal arcs in equal times around a circle.
But the cause of this irregular appearance can be accounted for by as many as two primary simple hypotheses. For if their movement is considered with respect to a circle in the plane of the ecliptic concentric with the cosmos so that our eye is the centre, then it is necessary to suppose that they make their regular movements either along circles not concentric with the cosmos, or along concentric circles; not with these simply, but with other circles borne upon them called epicycles. For according to either hypothesis it will appear possible for the planets seemingly to pass, in equal periods of time, through unequal arcs of the ecliptic circle which is concentric with the cosmos. (Ptolemy 87)

So what, exactly, do the two potential modifications entail? The first proposes that the sun revolves in a perfect circular orbit, describing equal arcs in equal times, where the orbit’s center does not coincide with the location of the Earth. Thus, when our eyes view the sun’s orbit, it appears that the sun describes unequal arcs in equal time because the sun will be farther away from us at its apogee and closer at its perigee and, therefore, its apparent speed will seem different in these two locations – appearing to move more quickly at the perigee and more slowly at the apogee. This option is what Ptolemy calls the hypothesis of eccentricity: the sun is posited to move around a circular orbit eccentric to the ecliptic circle that is concentric to the heavenly sphere. (Ptolemy 87)

The second also proposes that the sun revolves in a perfect circular orbit, describing equal arcs in equal times. However, this circular orbit itself orbits on a circle whose center is coincident with the location of the Earth (a deferent). (Ptolemy 87) Thus, it will appear to the observer located in the center of the ecliptic that the sun is not moving in a perfect circular orbit because the rotation of the epicyclical circle around the ecliptic will make the sun appear to move more quickly through certain arcs of the ecliptic and more slowly through others. (Ptolemy 87) With the hypothesis of the epicycle, it does not need to be the case that the heavenly body revolving on the epicycle
will appear to move more quickly at the perigee and more slowly at the apogee, as with the eccentric hypothesis. Rather, the heavenly body’s apparent speed at these two locations will depend on the direction in which the epicycle is rotating. (Ptolemy 87)

So, Ptolemy realizes that he cannot coherently maintain all aspects of his original hypotheses. Instead, he will have to either reject his astronomical theory altogether (which he does not do) or he will have to modify some part of this theory so that the theory will again accord with all the available evidence. In order to account for the 1st anomaly that the sun’s movement exhibits, and still maintain his theory, Ptolemy must modify the idea that all heavenly bodies move in a perfect circle whose center is coincident with the Earth’s location. Does he have a way to choose between the 2 hypotheses? He cannot determine the superiority of, say, the eccentric hypothesis over the epicycle hypothesis by examining the empirical data. As he states:

And it must be understood that all the appearances can be cared for interchangeably according to either hypothesis, when the same ratios are involved in each. In other words, the hypotheses are interchangeable when, in the case of the hypothesis of the epicycle, the ratio of the epicycle’s radius to the radius of the circle carrying it [the deferent] is the same as, in the case of the hypothesis of eccentricity, the ratio of the line between the centres (that is, between the eye and the centre of the eccentric circle), to the eccentric circle’s radius (Ptolemy 88)

This statement makes clear that Ptolemy thinks that the two hypotheses are empirically adequate to the same degree for use as explanation of the 1st anomalies. In fact, he demonstrates through the use of geometric proofs that both will explain the sun’s apparent orbital anomaly. (Ptolemy 88-93) Given that both’s results are empirically identical, it’s not immediately clear that Ptolemy has a basis for choosing between the two. Yet, choose he does. He chooses the eccentric hypothesis to explain the sun’s
anomaly and proceeds to use it for all of the other heavenly bodies whose orbits he
describes in order to explain this 1st anomaly. And Ptolemy chooses to use the eccentric
hypothesis as the most reasonable explanation of this anomaly, even though he continues
to insist that the two hypotheses are interchangeable. (Ptolemy 291, e.g.) Ptolemy claims
that “it would be more reasonable to stick to the hypothesis of eccentricity which is
simpler and completely effected by one and not two movements.” (Ptolemy 93) So,
because it is simpler, Ptolemy will use the hypothesis of eccentricity rather than that of
the epicycle to explain the 1st anomaly.

Here, two questions arise. Is Ptolemy accepting the hypothesis of eccentricity, or
is he just using it as a matter of convenience? If he is accepting this hypothesis over the
other, is he making an ad hoc move?

Again, I do not wish to engage in a historical debate. So, I will sidestep the first
question. Suppose that Ptolemy is accepting the hypothesis of eccentricity. Let us focus
on the second question. Intuitively, I claim, this is an ad hoc – and, therefore, rationally
impermissible – move. The eccentricity hypothesis and the epicycle hypothesis are
empirically equivalent. Ptolemy claims that the eccentricity hypothesis is simpler26, yet
one might argue that it requires more of a deviation from the first principles set out by
Ptolemy; after all, the eccentric hypothesis does require the ecliptic to no longer be
concentric with the heavenly sphere. And, as was discussed earlier, Copernicus was
quite suspicious of the equant, which seems akin to the eccentric in that the circle in
question no longer has the Earth as its center and is no longer concentric with the
heavenly sphere.

26 I discuss his claim of simplicity in more detail in chapter 4, section V.
I do not believe that this is my intuition alone, either. Certainly, philosophers such as Kuhn (cited earlier) have upheld Ptolemaic theory modification as emblematic of an ad hoc move, although they do not necessarily agree on why this is so. Additionally, most who have discussed the ad hoc nature of Ptolemy’s theory modification see it as a bad thing: that is, something like a vice, or rationally impermissible, or illicit in some way.

So, I argue, other theories of what makes a hypothesis introduction or acceptance ad hoc will need to illustrate why Ptolemy’s theory modification is, in fact, ad hoc, as well as illustrating why this modification is a vice in some way. If they do not, then they will have to satisfactorily explain why their conclusions go against our common intuitions concerning the Ptolemy case. And, I claim, none of the theories being considered will fulfill these obligations.

Before I get to the more contemporary views, however, I would like to examine the traditional view of ad hocness. The traditional view originated with Karl Popper and a version of his account is clearly outlined by Carl Hempel in Philosophy of Natural Science. These two views, although not identical, are similar in spirit. I will refer almost exclusively to Hempel’s account, although I will return briefly to Popper’s account of ad hocness where it differs from that of Hempel’s.

V. Hempel and Popper on Ad Hocness

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Hempel asserts that hypotheses introduced ad hoc are objectionable in science. He claims that they are objectionable because a hypothesis introduced ad hoc is introduced “for the sole purpose of saving a hypothesis seriously threatened by adverse evidence; it would not be called for by other findings and, roughly, speaking, it leads to no additional test implications.” (Hempel 29) He later admits that there is no precise criterion for deciding whether or not a hypothesis has been introduced ad hoc: there are only defeasible prima facie indicators, such as whether the hypothesis leads to additional predictions, and whether there is other evidence that corroborates the hypothesis. As he says: “is the hypothesis proposed just for the purpose of saving some current conception against adverse evidence, or does it also account for other phenomena, does it yield further significant test implications?” (Hempel 30)

So, Hempel thinks that there are two different indicators of ad hocness that tell us when a hypothesis ought to be considered ad hoc. The first indicator is that an ad hoc hypothesis is one that is introduced solely to save a theory from adverse evidence. The second, related, indicator is that an ad hoc hypothesis does not have any additional test predictions.

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28 I should note that there are some very appealing intuitions that seem to underly the traditional account. One appealing intuition is the idea that ad hocness is always objectionable in science and ad hoc hypotheses ought not to be permitted or, at least, censured as a result of their being ad hoc. Scientists themselves certainly use the term ad hoc in a pejorative sense and condemn hypotheses with this appellation. Another appealing intuition motivating the traditional account is the idea that the hypothesis is introduced to solve only one problem in the face of recalcitrant evidence – the particular problem that arose due to this evidence. This, intuitively, seems to be a problem because the solution seems to be a band-aid applied to a theory in order to save it. (Part of Leplin’s criteria seems motivated by this intuition. See chapter 3, section II.) Shortly, I will discuss major problems with the way that the traditional account of ad hocness is formulated from these intuitions, and at least one other intuition motivating the traditional account that motivates other, more modern accounts of ad hocness that I think is misguided. However, I will motivate my positive account in part by these appealing intuitions that seem to motivate the traditional account.
implications. Before looking at several criticisms of this view, it is important to examine further what Hempel’s account signifies.

Hempel first characterizes an ad hoc hypothesis as one that is introduced solely to save a theory from adverse evidence. The intuition behind this indicator for ad hocness seems to be that a hypothesis ought not be introduced just because it saves a theory. Thus, the first indicator seems related to the second because it condemns hypotheses that are just introduced to fix one problem.29 Suppose I were a proponent of a certain theory. I introduce a hypothesis into this theory, where my sole justification is to save this theory. This sort of hypothesis introduction might then be looked upon as quite suspect: I do not have any other reasons to introduce the hypothesis other than the recalcitrant evidence that made an adjustment of this theory necessary. Since my motives to introduce this hypothesis are suspect, won’t it be more likely that this hypothesis is suspect and, thus, that it will be less likely that my now modified theory is true or empirically adequate?

If a hypothesis is ad hoc, on Hempel’s view, it cannot have any additional test implications besides the problem that it is supposed to help the theory to which it is being added solve. There are two ways in which this indicator can be understood: either there will be no other test implications in that historical context at that time, given what scientists know or think possible at the time, or there will be no other test implications in principle for the hypothesis. I will examine these two options when I come to my criticism of Hempel’s view.

29 These are indicators and not criteria because it is not clear whether Hempel is claiming that each is sufficient on its own for determining whether or not a hypothesis is ad hoc, or whether both conditions need to be fulfilled in order for a hypothesis to be ad hoc. At the end of the section on the first indicator of ad hocness, I will consider what might happen if both are necessary conditions of ad hocness.
Karl Popper’s related view\textsuperscript{30} is that ad hoc hypotheses are unfalsifiable: that is, that there are no experiments different than the one that caused the hypothesis to be introduced into a given theory that can be performed where there is an opportunity for the hypothesis to be demonstrated false. These two criteria are related because both trade on the idea that an ad hoc hypothesis is one whose only implication is the accommodation of the recalcitrant evidence so that scientists may still uphold their theory. An example that Hempel uses (Hempel 28-29) is the \textit{horror vacui} theorists. As late as the 17\textsuperscript{th} century, people believed that nature abhors a vacuum and, so, something like a suction pump works because “water rushes up the pump barrel to fill the vacuum created by the rising piston.” (Hempel 28-29) Yet, Torricelli had already found evidence that air is pressurized and Pascal had already performed the Puy-de-Dome experiment, which demonstrated that a suction pump raised water to a lesser height when on top of the Puy de Dome in Auvergne than did it when of top of Diepe, which is at a much lower altitude than the Puy de Dome. These findings seem to disconfirm that nature abhors a vacuum, as it would seem strange that nature would abhor a vacuum in one place more than another. As Pascal said, “Does nature abhor a vacuum more on mountains than in valleys, more in wet weather than in clear weather? Does she not hate it equally on a belfry, in an attic, and in the yard?”\textsuperscript{31} Instead, Torrecelli and Pascal’s findings seem to

\textsuperscript{30}See, for example:


\textsuperscript{31}Pascal, Blaise. \textit{The Physical Treatises of Pascal: the equilibrium of liquids and the weight of the mass of the air}. New York: Columbia University Press, 1937. 429
maintain Pascal’s thesis, that there are vacuums and that the weight of the mass of the air is the cause of the height of water in a suction pump.

In the face of this evidence, the *horror vacui* theorists might have proposed the following hypothesis: nature does abhor a vacuum more in certain places and at certain times than others. And experiments done at different times and places will help us determine where it is that nature abhors vacuums more. Hempel would label this sort of hypothesis ‘ad hoc’ because it does not look like this hypothesis will give us any additional test implications other than those of Pascal, whose evidence the hypothesis was supposed to mitigate.\(^{32}\) And Popper would agree that this hypothesis was unfalsifiable because the amount that nature abhors a vacuum will just be whatever readings are obtained at certain places and at certain times: there is no way to create an experiment where there is a possibility of disconfirmation for this hypothesis.\(^{33}\)

### VI. Problems for Hempel’s First Indicator

Hempel’s two indicators, however, are not adequate for determining ad hocness. Hempel’s first indicator – that an ad hoc hypothesis is one introduced solely to save a

\(^{32}\) For an additional example of the sort of hypothesis that Hempel considers ad hoc, see Hempel’s discussion of the funiculus hypothesis. (Hempel 29) I’ll discuss the funiculus hypothesis in chapter 4, section VI.

\(^{33}\) I will maintain that there will be (insurmountable) problems faced when claiming that a hypothesis has no additional test implications, or that a hypothesis is unfalsifiable, in section VII. There is a related discussion for Hempel’s 2\(^{nd}\) criterion in section VI. Right now, I am just trying to describe the traditional view in more detail. This includes giving examples characterizing how the proponents of the traditional view thought that they could show how a certain hypothesis is ad hoc and motivating the intuitions behind the view.
theory from adverse evidence – runs into trouble. It seems to commit us to claiming that
ad hocness depends on the psychology of the scientists introducing the hypothesis.
Here’s an example to illustrate this point: Scientists Kevin and Brian work together to
create a new hypothesis. They create and then introduce this hypothesis because there
has arisen new evidence that is inconsistent with the current theory T. They are
committed to the same scientific background, have read the same articles, and are
committed to the same theories. Everything about their respective situations is exactly
the same with one exception: Brian is motivated to introduce this hypothesis because he
knows that the President really likes theory T and will generously fund any project that
‘saves’ theory T. Kevin, on the other hand, realizes that this new hypothesis will also
predict other phenomena that will likely occur, based on the new evidence that has been
found.

If we are to side with Hempel and claim that an ad hoc hypothesis is one where
there are no other motivations (NOM), then it looks as if we are committed to one of the
following claims: (a) the hypothesis is not ad hoc if we look at the motivations of Kevin
but it is if we look at Brian’s motivations; (b) the hypothesis is sort of ad hoc – because
the two scientists’ motivations were different; (c) the hypothesis is ad hoc – at least, until
Brian realizes that it does predict other phenomena over and beyond theory T and, thus, T
+ hypothesis is a better predictor than T alone. (c) does not seem to be a very satisfactory
option because it more heavily weighs Brian’s motivations without giving justification
for this. Yet, options (a) and (b) both force us to give up the idea that something is either
ad hoc or not.
There is, here, a dubious connection between the reasonableness of a scientist\textsuperscript{34} adopting a given hypothesis (barring other major problems) and what the scientist was actually thinking at the time. Appropriate theory modification is a matter of following the correct procedures. It is in following these procedures that the scientist will be likely to make the appropriate modifications. It does not matter why it is that the scientist is following these procedures, nor does it matter if the scientist understands how the procedure works.

Additionally, it might well be the case that a certain hypothesis introduction ends up being a benefit for a given theory, even though the scientists introduced it NOM-ly. An example would be Planck’s introduction of $h$ in order to solve the problems that arose for the then-current theories of black body radiation. As stated previously, Planck himself thought that this hypothesis introduction was ad hoc. Yet, Einstein later validated this introduction. Would, then, we want to say that this hypothesis introduction was ad hoc but that it was a good introduction, nonetheless?\textsuperscript{35}

NOM does not construe ad hocness as always a vice, thus going against scientific and philosophical tradition concerning the usage of this term. Scientific tradition is such

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\textsuperscript{34} Or, perhaps, it being justified to adopt.

\textsuperscript{35} I am using this example merely in order to motivate our intuitions concerning the nature of ad hocness. Using this example is a bit misleading, as I claim (in chapter 5, section X) that, in introducing $h$, Planck created a new blackbody radiation theory instead of modifying one of the existing theories. Thus, his action is not one that can be evaluated for ad hocness. However, in the context in which the example is currently being discussed, the important element is that Planck introduced it solely to reconcile the existing blackbody radiation theories with the evidence. The example could be changed so that, in fact, Planck was modifying an existing theory instead of changing over to a new theory. Thus, I think that my use of this example, albeit not truly an example of an ad hoc act, is harmless.
that a charge of ad hocness always gives negative connotations. Yet, on views that rely on NOM, ad hoc hypotheses need to be divided into a ‘good ad hoc’ category and a ‘bad ad hoc’ category. This is because the motives of scientists cannot alone cause a hypothesis adoption itself to be either good or bad.36

A skeptical reader might view the Kevin and Brian example with suspicion. He might think that this story is being set up as if Kevin and Brian have identical situations except for one insignificant difference in their motivations. Yet, the objection goes, this difference ought not be considered insignificant. The only way that this difference would be insignificant, the skeptic claims, is if the example presupposes a consequentialist methodology. In ethics, consequentialism is the view that what counts, morally, are the outcomes of actions. Whatever action will cause the greatest good or the least bad will be the morally right action. According to ethical consequentialists, there is no difference between a boy scout helping an old lady across the street because he wants to show his respect for the elderly and the boy scout doing so because he wants to impress his girlfriend, as long as the same consequences occur. Like consequentialists in ethics, the skeptic claims, there can be consequentialists in scientific methodology. And, as in

36 It is true that, on my view, it can become a good thing that some hypothesis is a part of a given theory, when it wasn’t previously. However, seeing the eventual benefit of having the hypothesis as part of a theory does not erase the fact that the acceptance of this hypothesis into the theory was ad hoc and that the scientist committed a methodological error in accepting it at the time that she did. I have a detailed discussion of the combination of ad hocness with other virtues and vices in chapter 5, section XIII. A hypothesis acceptance is either ad hoc, or it is not. A scientist commits a methodological error in accepting an ad hoc hypothesis. There might be epistemological reasons for accepting such a hypothesis, despite its being ad hoc.

As I was just discussing elements of my own view, I was being much more careful in stating what can be labeled ‘ad hoc’. On my account, only certain types of hypothesis acceptances can be labeled ‘ad hoc’. I discuss my reasons for this distinction in section XI of this chapter, as well as in chapter 5, section I.
ethics, there is also another way to view scientific methodology that is not recognized in the Kevin and Brian example. This would be a deontological view of scientific methodology, where the motivations of the scientists are the most important criterion for the acceptability of their actions.

The Kevin and Brian example, by claiming this difference in motivations is insignificant, is presupposing a consequentialist conception of the situation: that is, Kevin and Brian were committed to the same theory, were aware of the same recalcitrant evidence and came up with the same hypothesis to save the theory in the face of this evidence. Therefore, supposing consequentialism, Kevin and Brian’s hypotheses ought to be evaluated the same. However, the skeptic continues, this conclusion only follows given the presupposed consequentialism. Yet, there is a very important difference between Kevin’s situation and Brian’s situation: they have different motivations to introduce this hypothesis. Having read all of the same literature and being committed to the same background, etc., gives both of them access to the same sorts of motivations. Kevin acted according to the correct motivation and, so, is justified to believe that the new hypothesis is true\(^\text{37}\). Brian, on the other hand, did not have as his motivation the correct motivation to introduce the hypothesis. Yet, he, too, had the correct motivation at his disposal. Therefore, Brian is not justified in believing that the new hypothesis is true. Therefore, in Brian’s case, the hypothesis is not on the same footing as in Kevin’s case. So, in fact, scientist’s motivations are very important in deciding whether a certain hypothesis is ad hoc or not.

\(^{37}\)I am not using ‘true’ in any strong sense here. You can insert your favorite term, such as ‘empirically adequate’. The critique and my response will not depend on the term.
This reply to my objection rests on a misunderstanding of what is going on when a hypothesis is labeled as ‘ad hoc’. This is because, in discussing which one of these scientists is justified in believing that the hypothesis is true, the reply is making ad hocness an epistemological matter. However, ad hocness is a methodological concern. Labeling a hypothesis ‘ad hoc’ is to claim that the scientific endeavor went wrong in its procedures. An account of ad hocness will give scientists a rule for determining whether or not the acceptance of a hypothesis is a permissible act. It is a far different issue as to whether or not a scientist is justified in believing that a hypothesis is true. It will be up to the proponent of the methodological account to demonstrate that it is a good account that will cause scientists that follow it to get the right results. However, the scientists themselves don’t need to be justified in their beliefs in order to use the methodology to prevent ad hoc acts.

In Experience and Prediction, Hans Reichenbach touches on a similar point when he speaks about the distinction between context of discovery and context of justification. This distinction arises in his discussion of epistemology and what scientific epistemology entails. (Reichenbach 6-7, 382) Reichenbach claims that epistemology concerns itself with the way that scientific information is disseminated to others, and not the way the scientist himself comes to accept this scientific information. (Reichenbach 6-7) As he says, when discussing the theory of induction:

What we wish to point out with our theory of induction is the logical relation of the new theory to the known facts. We do not insist that the discovery of the new theory is performed by a reflection of a kind similar to our exposition; we do not maintain anything about the question of how it is

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performed – what we maintain is nothing but a relation of a theory to facts, independent of the man who found the theory. (Reichenbach 382)

In this quote, Reichenbach separates the way in which a theory is discovered from the way in which the theory is justified to others. While he is not discussing ad hocness and his emphasis in this book is the context of justification, the separation of justification and discovery helps to strengthen my claim that whether a person is justified in believing a hypothesis true is a different question than whether he used the right methodology to arrive at this hypothesis.

Here is an example in order to clarify the way in which justification separates from methodology. There are several mechanics that work at a garage that is noted for its excellence in auto repair. There are several ways that the different mechanics in this shop determine what procedures they need to undertake in order to fix the cars that come into their shop. Some of the mechanics hook up the cars to a computer that has been pre-programmed to interface with the computer chips in the cars in order to determine what repairs need to be made. Some of the mechanics use the algorithms that the computer uses in order to determine the needed repairs. The rest have a detailed understanding of the components of the cars, how the computer chips work to make the car operate and how certain types of problems cause certain issues. The diagnostic computer doesn’t understand what it’s doing: it has been programmed to produce certain results given certain inputs. Those mechanics that are using the computers don’t need to understand how the diagnostic computers work, either. And they could just be using the diagnostic computers because they know that the boss really wants them to do so and the mechanics that use the computers get more money from the boss. The story might be similar for those that use the algorithms. Those mechanics also don’t need to have good reasons to
use the algorithms and also don’t need to understand how these algorithms work. In this
garage, all the mechanics get the same results, even though they have different reasons
for arriving at these results. And, therefore, the system by which they fix the cars is still
good, even though not all of the mechanics understand how the system works and not all
of them are using the system because of a motivation to fix the cars properly – some are
using the system just because the boss will pay them more if they do.

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

This rulebook-model of methodology covers all of scientific practice. I will not
attempt to defend all of the different chapters of this rulebook, as this is outside of the
scope of my discussion. There is, however, a page of this rulebook that concerns ad
hocness. This page is the one that I am interested in examining. And it is this page that
lets scientists know whether a given hypothesis ought to be accepted into a theory, given
that theory, its background, and the recalcitrant evidence facing that theory. Based on a
specific input, the scientists will be able to use this page of the rulebook in order to create
an appropriate output.
So, in the case of Kevin and Brian, it would be inappropriate for the rulebook—the system for determining whether or not a certain act is ad hoc—to give them two different rules to follow. This would be inappropriate because they have identical inputs. These inputs are identical because the only difference between Kevin’s situation and Brian’s situation is each person’s motivation, which does not factor in to what is relevant according to the rulebook. Kevin might still be justified to believe that his hypothesis is true, and Brian might not be. However, the rulebook or the system, given the same inputs, must give them the same output at the same time.

VII. Treating Hempel’s Indicators as Criteria to be Jointly Fulfilled and the Problem of Additional Test Implications

I have been treating the Kevin and Brian example in terms of Hempel’s first characterization of ad hocness alone. As I stated above, Hempel was not clear whether the two characterizations were just two good ways to tell if a given hypothesis is ad hoc, or if they were two criteria, both of which need to be fulfilled, in order for a hypothesis to be ad hoc. In case the latter is true, let us re-examine the Kevin and Brian situation, adding the condition that hypotheses lead to no additional test implications. Kevin is still motivated by the realization this new hypothesis might also predict other phenomena that will likely occur, based on the new evidence that has been found. Brian is still motivated by the realization that the President really likes theory T and will fund whoever tries to save it in the face of recalcitrant evidence. Does the addition of the second formulation, seen as an additional criterion for ad hocness, cause a difference in the outcome of this example? At first glance, it might seem that it does. After all, Kevin thinks that this
hypothesis might lead to additional test implications. Perhaps it actually will lead to additional implications. Since both of them are introducing the same hypothesis to the same theory in the face of the same recalcitrant evidence, it might seem that neither of them did something ad hoc in introducing the identical hypotheses.

However, a problem arises. How can it be determined whether or not a hypothesis leads to additional test implications besides that of the (formerly) recalcitrant evidence? After all, most, if not all, of the time, to claim that a hypothesis leads to additional test implications seems to presuppose a lot about background conditions and about other hypotheses. For example, suppose we have a hypothesis that is introduced to save a certain theory. It was introduced because part of the theory claimed that the sky was blue and the sky was observed to be red on a certain occasion. The hypothesis that is introduced replaces the previous part of the theory that claimed the sky was blue. It says that the sky will be red at certain times and blue at certain times, and that barometric pressure will rise and fall, according to what color the sky is. On the face of it, it looks like this hypothesis will lead to additional test implications: the amount of barometric pressure that will exist given a certain sky color. Suppose, then, that we observe that the barometric pressure does seem to rise and fall based on the color of the sky. Did this hypothesis predict these phenomena?

All by itself, the barometer-sky hypothesis doesn’t lead to predictions. Auxiliary hypotheses need to be combined with the hypothesis to lead to any predictions whatsoever. The auxiliaries that need to be supposed in conjunction with the hypothesis are hypotheses such as the barometer is well-constructed, or that the scientists observing the barometer and the sky color are competent observers of such phenomena. It doesn’t
look like we can claim that a single hypothesis, by itself, will lead to additional test implications.

This problem is very similar to what A.J. Ayer encountered in *Language, Truth and Logic*. While he is not concerned with ad hocness, the problems that he encounters will cause even greater problems for accounts of ad hocness that require there to be additional test implications.

These problems arise while Ayer is trying to explain verificationism. To Ayer, the only statements that are meaningful are either analytic – such as statements of logic – or empirical hypotheses that admit of truth or falsity. All other statements are "metaphysical" and meaningless. As Ayer states:

> [W]e shall maintain that no statement which refers to a “reality” transcending the limits of all possible sense-experience can possibly have any literal significance: from which it must follow that the labours of those who have striven to describe such a reality have all been devoted to the production of nonsense. (Ayer 34)

Thus, the statements that metaphysicians make are meaningless because they fail to adhere to the guidelines that determine that which makes a statement significant. (Ayer 35)

The question then becomes, what makes a statement significant? What makes it express a genuine empirical hypothesis, capable of being either true or false? Ayer uses the criterion of verifiability in order to determine the significance of statements. This

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40 I will describe Ayer’s formulation of this criterion found in the introduction to the second edition of *Language, Truth and Logic*, as this formulation has been crafted in response to objections to the manner in which Ayer first formulated it. Throughout the previous and following discussion, I will also use the term ‘statement’ in place of
criterion consists in the following: a statement is directly verifiable if it is an observation statement, or if it plus one or more observation statements entails an observation statement. A statement is indirectly verifiable if: “in conjunction with […] other premises it entails one or more directly verifiable statements which are not deducible from these other premises alone; and secondly, that these other premises do not include any statement that is not either analytic, or directly verifiable, or capable of being independently established as indirectly verifiable.” (Ayer 13) If a statement is neither directly nor indirectly verifiable, it is not a genuine statement, as it is not expressing a genuine empirical hypothesis.

Ayer reformulated the verification principle to its above shape as a result of a criticism brought against his original formulation: that, if properly formulated, any statement whatsoever can be considered directly verifiable. Unfortunately, Ayer’s reformulation still allows any statement whatsoever to be considered directly or indirectly verifiable, and, thus, capable of being either true or false. All that needs to be done is to create a disjunction between the statement in question and some statement that is verifiable. Therefore, every statement whatsoever can be considered literally meaningful. So, Ayer’s delineation between meaningful and non-meaningful statements in philosophy is an empty one. Crispin Wright, in Realism, Meaning, and Truth⁴¹, illustrates this devastating problem quite clearly:

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Let N be any ‘nonsensical’ statement, and O₁ and O₂ any pair of observation statements which are logically independent of each other. Then consider the statement

\[ A: \text{Either } O₁ \text{ or (not-} N \text{ and not-} O₂ \text{)} \]

Conjoined with O₂, A entails O₁. But O₂ alone does not entail O₁ (by hypothesis). So A is directly verifiable. Therefore, since N, conjoined with A, entails O₁, which is not entailed by A alone (assuming that not-N and not-O₂ do not collectively entail O₁), N also passes as indirectly verifiable. (Wright 281)

So, it looks like any statement whatsoever can be either directly or indirectly verifiable. All that is needed is the proper formulation of a statement that is directly verifiable, which includes some ‘nonsensical’ statement in the right relationship to the rest of the statement. This ‘nonsensical’ statement will end up, as a consequence, as indirectly verifiable and, so, as a genuine statement. Yet this ‘nonsensical’ statement is ‘nonsensical’ because it is going to be exactly the sort of statement that Ayer wished to exclude from the set of statements that are meaningful, as it is going to be the sort of statement that Ayer wished to claim was unverifiable. Therefore, Ayer cannot demonstrate that certain statements, and not others, lead to test implications.

If Ayer can’t demonstrate that certain statements can and certain statements cannot lead to test implications, how can it be demonstrated that something might or might not lead to additional test implications? This seems to be the problem facing Hempel’s second characterization of ad hocness.
VIII. More on the Ayer Testability Problem

This discussion of Ayer’s problem and how it relates to Hempel's second indicator of ad hocness leads us into a discussion of the difficulty this second indicator faces, either in conjunction with the first indicator or by itself. Typically, in mature sciences, we cannot test a single hypothesis in isolation. So, if the requirement for non-ad hocness is that the hypothesis by itself leads to additional test implications, all hypotheses will be ad hoc. However, we can always tinker with the auxiliary hypotheses. Because we can do so, and if we allow a given hypothesis to be tested in conjunction with auxiliaries, we can engineer the auxiliaries in such a way that no hypothesis will be ad hoc because all hypotheses will lead to additional implications.

Ayer formulates the problem a bit differently when he says: “it is never just a single hypothesis which an observation confirms or discredits, but always a system of hypotheses.” (Ayer 94) There might well be other hypotheses or some fact about the universe that we are missing when we claim that something is not testable. Or, we might illicitly combine certain hypotheses with another in order to claim that the hypothesis is, in fact, testable. This is the problem of independent testability. It appears that a

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42 Duhem speaks of this problem, as well as Quine. Later, I will refer to this problem of independent testability as the Quine-Duhem-Ayer problem.


hypothesis is (almost) never independently testable and, so, it is unclear whether a hypothesis might seem untestable because of a problematic auxiliary hypothesis, or whether the hypothesis really doesn’t lead to any test implications at all.

IX. A Critique of Popper’s Conception of Ad Hocness

In a related manner, suppose we were to say, as Popper did, that ad hoc hypotheses are bad because they let you protect your theories from falsification without being independently testable. And suppose we were to determine whether or not a hypothesis alone is independently testable. Again, we run into this problem: what does it mean to be ‘independently testable’? As Duhem notes in The Aim and Structure of Physical Theory, an experiment can never condemn an isolated hypothesis but only a whole theoretical group. In an experiment that is supposed to test a hypothesis, the scientists derive a prediction of a fact from the hypothesis in conjunction with a set of auxiliary theories and hypotheses that the scientists already accept. So, the experiment cannot show, e.g., that a specific hypothesis is false. Therefore, it does not seem that any hypothesis is independently testable. If no hypothesis is independently testable, as was noted previously, then certainly no hypothesis can lead to additional test implications by itself. Therefore, this distinction does not seem to be an adequate one, as it would make all hypotheses ad hoc\textsuperscript{43}.

\textsuperscript{43} Also, see Bamford for a detailed discussion of Popper’s distinction.

On the other hand, if we were to claim that testability of a hypothesis means that a hypothesis, in conjunction with certain auxiliary hypotheses, is testable, then all hypotheses would seem to be testable. This problem is the exact problem Wright mentions for Ayer’s view in the quotation above.

For example, let us take up the hypothesis that God exists. This hypothesis might not seem, on the face of it, testable. Yet, we might add a few auxiliary hypotheses: whatever God wants, happens; God wants the sun to set; God wants the wind to blow. Does the sun set? Yes. Does the wind blow? Yes. Therefore, we can test whether or not God exists. And, according to this test, we now have evidence that God exists.

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

If we were to limit the auxiliaries to ones that we think are true, we might be able distinguish legitimately testable hypotheses from those such as the ‘God exists’ hypothesis. That will depend, of course, on whether or not we think that the auxiliaries added to ‘God exists’ are true. However, there is the possibility, at least, that we might be able to go further in making a distinction between testable hypotheses and untestable ones, using the ‘believe to be true’ criterion.

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

Instead, we might claim that a hypothesis is testable only if, when combined with one or more auxiliary hypotheses that we already believe to be true, we can infer some observation statement not previously inferable from the auxiliary hypotheses alone. This formulation would get around the previous objection because the additional testability in the previous objection will arise from the auxiliary hypotheses alone – in that case, ‘the world is round’ hypothesis. However, this does not get rid of all objections to this formulation of testability. Suppose you have a hypothesis H, and suppose A is ‘either ~H or else R’, where R is some testable hypothesis that you already believe. A will be true, according to you, and A & H entail observation statements that are not entailed by H alone. And this sort of situation can be constructed for any such hypothesis. So, even limiting the auxiliaries in this way will not be enough to fully overcome the testability problem.

Instead, the criterion of testability ought to be eliminated from any appropriate characterization of ad hocness, or so I claim.

I am not yet done critiquing traditional conceptions of ad hocness. The most compelling, but still misguided, view I will discuss in a bit more detail. This is the
characterization of ad hocness as a state of incessant modifications, made famous, in part, by Thomas Kuhn.44

X: Ad Hocness as the State of Incessant Modifications

Consider the following case:

Ptolemy case45: Ptolemaic astronomy is a geocentric theory that posits circular orbits for the heavenly bodies. In order to explain why the heavenly bodies appear not to travel in circular orbits, Ptolemaic astronomy posits for each of the heavenly bodies some combination of eccentrics (circular orbits whose centers are not concentric with the Earth’s) and epicycles (circular orbits whose centers are on circular orbits known as deferents). A proponent of Ptolemaic astronomy makes planetary observations that cannot be explained by the posited epicycles and eccentrics. Therefore, she adds some epicycles to the model.

On the repeated modifications view, it is the historical background that causes a hypothesis adoption to be ad hoc. In this history, a theory has encountered a series of empirical problems, each of which is fixed with a similar (minor) patch. The philosopher’s Ptolemy case just described is a canonical example for this view. I’ll agree that there do seem to be too many (similar) hypotheses adopted in order to save Ptolemaic astronomy. So, why doesn’t this view get to the heart of what it is to be ad hoc?

The impropriety cannot rest solely on the idea that we are accepting these hypotheses into our theory just because there is evidence that seems to disconfirm the

44 See footnote 16 of this chapter for Kuhn’s characterization of Ptolemaic astronomy in this manner.

45 This example is not, in fact, historically accurate. However, I am going to use this as an example because it has become a canonical example of ad hocness.
theory. If the distinction just were this, then there would be no real way to differentiate between the Ptolemaic sort of case and a non-ad hoc case where a theory has been repeatedly (and similarly) modified, as in the case of Newtonian mechanics before the advent of relativity. The emphasis, on this view, is placed on the historical background. So, it is not just that the auxiliaries have been tinkered with to fit the facts but especially that this sort of tinkering has happened before. Yet, theories are continually being modified in some way or another in order to fit the facts, and there is no clear way to determine which of these modifications qualify as ‘tinkering’ and which are not. As there is no clear way to make this distinction, there is no real way to determine whether or not the background is the appropriate sort of background in order for a specific hypothesis adoption to be considered ad hoc.

46 I discuss the postulation of Neptune and of Vulcan as two such examples of non-ad hoc acts. These examples are found in chapter 5, section V.

47 In an article by George Musser in Scientific American, he quotes Robert Scherrer of Vanderbilt University as saying the following:

“As my senior colleagues used to say, “you only get to invoke the tooth fairy once.” Right now we have to invoke the tooth fairy twice: we need to postulate a yet to be discovered particle as dark matter and an unknown source for dark energy. My model manages to explain both with a single field.”

Scherrer is speaking in favor of his theory of scalar dark matter and against traditional theories. He seems to be subscribing to some version of the repeated modifications account of ad hocness. In the same article, Sean Carroll criticizes Scherrer’s view, claiming that there is a nagging problem in that Scherrer’s theory requires laws of physics to possess a unsuspected symmetry. As Carroll says, “such symmetries are possible, although they appear somewhat contrived.” [italics mine]


48 When I am stating that there is no clear way to make this distinction, I am gesturing towards a problem that is much more pressing than merely an issue of vagueness as to where, exactly, one category begins and the other ends. Lots of notions have vague boundaries, after all.
We might have a good reason to think that a theory that has been faced with an enormous amount of instances of recalcitrant evidence in a (shortish) period of time ought to be discarded. However, thinking one ought to discard one’s theory is something different from claiming that an ad hoc move has been made.

Or, to put the point slightly differently: suppose a scientist holds a theory for which recalcitrant evidence comes to light. In this type of situation, there are always three choices for the scientist. Option A: The scientist can discard her theory. Option B: The scientist can accept a hypothesis that makes the evidence no longer disconfirming. Option C: The scientist can do nothing, recognizing that there is a tension, or even an inconsistency, between the evidence and her theory plus the background but deciding to hold off doing anything about it for the time being. To discard one’s theory is to take option A. To commit a potentially ad hoc act, on the other hand, is to take option B.49

Of course, Kuhn’s version of this view also included the notion that complexity was increasing faster than accuracy, which is a more nuanced view than the one critiqued above. However, Kuhn’s version is not satisfactory because of serious issues on how to determine simplicity and the appropriate relationship between complexity and accuracy.50

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49 I discuss these options in more detail in chapter 5, section I. I also use them when critiquing Sober and Forster’s account of ad hocness in chapter 4, section VI.

50 Graham Priest, for example, discusses the problem of determining when a curve is simpler than another in “Gruesome Simplicity”. Elliot Sober and Malcolm Forster refer to this problem in their paper, “How to Tell When Simpler, More Unified, or Less Ad Hoc Theories Will Provide More Accurate Predictions.” They claim to avoid the problem by discussing sets of curves. I critique their view in chapter 3. There is a further, although related, problem for Kuhn in that the complexity warranted for a solution to a particular problem would seem to depend radically on the specific theory and the significance of the problem.
There is another version of this view worth mentioning. It states that ad hoc hypotheses are those that lead to no new test implications other than the piece of evidence they were introduced in order to avoid, or one very similar in content to previous adoptions. Thus, we have an ‘automatic recipe’ in the cases of ad hoc modifications and we don’t in other cases of non-ad hoc (repeated) modifications.

This version will not work, either, for two reasons. Firstly, there is the test implications problem associated with Hempel’s 2nd guideline. The second is that this version will categorize some hypothesis additions as ad hoc when they seem, intuitively, not to be. In Newtonian mechanics, neither the positing of an extra-Uranian planet, as in the example above, nor the positing of an intra-Mercurial planet seems intuitively ad hoc. Yet, in this example, there are repeated modifications of the same kind. The Ptolemy case also adds similar hypotheses, yet is an intuitive example of ad hocness. The problem arises in how to distinguish these two examples, according to the repeated modifications view.

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51 I owe this formulation of the repeated modifications account to Marc Lange.

52 And philosophers such as Jarrett Leplin argue that these examples are not ad hoc.

53 A problem with the orbit of a certain planet is found and, in order to reconcile Newtonian astronomy with the evidence, the existence of a massive body in a certain region of space is postulated.

54 In chapter 5, section V, I discuss this objection in more detail. There, I acknowledge that it is not a knock-down argument against this version of the repeated modifications account: maybe there needs to be a certain number of similar modifications in order for the modifications to be considered ad hoc. However, this objection ought to, at least, be a concern for the ‘automatic recipe’ advocates. Also, the first objection is, in my opinion, a
I have, until now, been applying the term ‘ad hoc’ either to hypotheses or to their introduction, as this is how the more traditional accounts have applied the term. However, I think that the term ought to be applied to other types of acts. I will now turn to what types of acts can be ad hoc.

XI. What can be Ad Hoc?

Notice, so far, I have been quite vague as to what sort of thing can be considered ‘ad hoc’ mainly because the traditional view chiefly refers to hypotheses when discussing what can be ‘ad hoc’. However, it is an open question as to what can qualify as being ad hoc.

Among the candidates for ad hocness, there is the possibility raised in Hempel that it is only appropriate to label hypotheses as ad hoc. Although hypotheses are often what are labeled ad hoc in the literature, I do not wish to claim this. The same hypothesis might be introduced in two difference circumstances but might differ with respect to ad hocness in these circumstances. As the circumstances into which the hypothesis is introduced seem to make a difference as to whether or not the hypothesis is labeled ‘ad hoc’, it seems strange to label hypotheses themselves as ad hoc. The knock-down argument against the ‘automatic recipe’ version of the repeated modifications account.

55 Many people do seem to talk about, e.g., hypotheses as being ad hoc. However, I wish to claim that it is not, in fact, the abstract object ‘hypothesis’ that is actually being labeled ad hoc. The statement, “this is an ad hoc hypothesis” seems, instead, to be shorthand for something along these lines: “The introduction or acceptance of this hypothesis was an ad hoc move” – where the application of ‘ad hoc’ may or may not be context-dependent.
difference in ad hocness for the same hypothesis in two different circumstances occurs because a hypothesis does not, by itself, have the sort of qualities that we might wish to attribute to something that is ad hoc. There does not seem to be anything inherently problematic with the hypothesis that the moon is made of green cheese, for example: there may well be certain instances in which this hypothesis would be perfectly acceptable. Instead, it seems that the act either of introducing or accepting a hypothesis is the sort of thing that is qualified to be ad hoc.56

Instead of hypotheses themselves, then, it looks like what can be labeled ad hoc are acts. The obvious candidates for these acts are the introduction of a hypothesis and the acceptance57 of a hypothesis. The introduction of a hypothesis is distinguished from the acceptance of a hypothesis in that the introduction of a hypothesis is how that particular proposition came to be entertained as a hypothesis at all, while the acceptance of a hypothesis is an additional step. Once a hypothesis has been introduced, scientists will have to think that the hypothesis, given the theory to which it is added, is true or

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56 This distinction is an important one to make, in part because of the issues raised when considering testability. There will likely be other issues raised in considering something like an act ad hoc, different than in considering a hypothesis ad hoc. Greg Bamford, in “Popper’s Explications of Ad Hocness: Circularity, Empirical Content and Scientific Practice“, highlights this point. He notes that people often equivocate between talking about a hypothesis being ad hoc and the way that the hypothesis is introduced as being ad hoc. Yet, theories and hypotheses are propositions and moves are not. Only a proposition can be tested: a move cannot. (Bamford 336 -340) So, there is another way in which theories of ad hocness that have testability as a criterion go astray, if these theories really are using ‘ad hoc hypothesis’ as a placeholder for ‘the introduction or acceptance of this hypothesis is an ad hoc move’, as I suggest in the previous footnote.

57 I wish to sidestep, here, the issue of whether something that is accepted is thereby believed to be true or empirically adequate, in a van Fraassen sense. Insert your preferred position; it will not affect this discussion. I return to this point in chapter 5, section XVI.

empirically adequate or well supported before they accept that hypothesis. Merely introducing a hypothesis does not require this additional step.

The *introduction* of a hypothesis – even one for which we do not seem to have any additional predictions or additional corroborating evidence – is perfectly o.k. Different standards govern the introduction of a hypothesis than govern the acceptance of a hypothesis. It might well be rationally permissible to introduce a hypothesis that you do not think is a very good claim, for example, if you have no other candidates to solve some problem. Or, suppose you are sitting around discussing disconfirming evidence for your theory with some other scientists and you say, “What if hypothesis Y were the case?” Even if this hypothesis, were it to have been *accepted*, would have fulfilled your criteria for ad hocness, it does not seem right to claim that the *introduction* of this hypothesis is ad hoc. All that you are doing is throwing the idea around to see where it might take you. There is not enough commitment to hypothesis Y to think that any methodological error has occurred.

Suppose, instead, that you *accepted* hypothesis Y into your theory. By accepting it, you are taking an additional step. Instead of, say, merely entertaining the possibility of Y, you are committing to the claim that Y is likely to be empirically adequate, or true, given the theory to which it is added. In this case, there is enough of a commitment that there will be a methodological error being made if it turns out that accepting this hypothesis would be ad hoc, according to your criteria for ad hocness.58

In other words, accepting a hypothesis into a theory is a much more serious action than merely introducing a hypothesis. The appropriate action that can qualify as being ad

58 I discuss the differences between introducing and accepting a hypothesis again in chapter 5, section I and in chapter 5, footnote 13.
hoc, then, must be the act of accepting a hypothesis, as ad hocness is a serious methodological error and needs to be paired with a more serious action.

XII. Conclusion

We have seen that Hempel’s two guidelines are neither individually necessary nor jointly sufficient for determining ad hocness. And, we have seen that there are problems with any of the repeated modifications accounts of ad hocness. So, there must be something else wrong with the objectionable hypothesis acceptances that we have described, above, than what those accounts of ad hocness would tell us. It will now be helpful to examine some other views of ad hocness, in order to see what they got right and what they got wrong. The examination and critique of these other views of ad hocness will lead the way to my positive view of ad hocness, which I claim includes the good elements and intuitions both of the traditional view of ad hocness and these other, more recent views, while excluding the bad elements of the same.
Chapter Two
Why a Bayesian Account of Ad Hocness Is Not Satisfactory

I. Introduction

I will argue that Bayesian accounts of ad hocness are, ultimately, unsatisfactory. Several of my objections will be criticisms of Bayesianism in general, while others will be criticisms of a specific version of Bayesianism as introduced by Michael Strevens in “The Bayesian Treatment of Auxiliary Hypotheses”. Firstly, then, I will outline the basic Bayesian approach and how it characterizes ad hocness, using Colin Howson and Peter Urbach’s discussion in Scientific Reasoning: the Bayesian Approach.¹ I will then outline the differences between their characterization of ad hocness and that of Strevens’s Bayesianism and will apply both to several examples. Finally, I will show why either Bayesian-type approach is unsatisfactory in characterizing ad hocness, using, in part, Branden Fitelson and Andrew Waterman’s paper, “Bayesian Confirmation and Auxiliary Hypotheses Revisited: A Reply to Strevens.”²


II. Introduction to the Bayesian Approach

The Bayesian approach to scientific reasoning has become a popular one, due to its response to the problem of induction: that is, how to determine whether or not a particular theory is the right one (or the likely one), given that all evidence for such theories is empirical evidence. It proposes a probabilistic induction, where a given theory or hypothesis is deemed more or less probable, based on the scientists’ degrees of belief about them and how these degrees of belief ought to change given some particular evidence that bears on that theory or hypothesis. (Howson Urbach 9) I will need to go into more detail for all of these elements. What I will turn to first, however, is the theorem that all Bayesians utilize in order to determine the probability of some hypothesis or theory given the empirical data: Bayes’s Theorem.

Bayes’s Theorem can be stated in the following form:

\[
P(a | b) = \frac{(P(b | a) P(a))}{P(b)}, \text{ where } P(a), P(b) > 0 \text{ (Howson Urbach 28)}
\]

\(P(a | b)\) represents the probability of the hypothesis \(a\), given the evidence \(b\), which is equal to the likelihood of the evidence given the hypothesis multiplied by the initial probability of the hypothesis, all divided by the probability of the evidence. (Howson Urbach 28)

So, in order to determine how much some piece of evidence affects (negatively or positively) the probability of a certain hypothesis or theory (being true), the scientist just needs to determine what the relevant prior probabilities were – those before the evidence came to light – and plug them into the theorem.
Of course, this depends on there being some principled way to determine the prior probabilities needed to complete this equation: clearly, the probabilities cannot just be determined by a roll of the dice, or by picking numbers out of the air. If they were so determined, there would be no reason to believe the Bayesian when she said that her theory was a good theory of confirmation.

The standard method of describing the prior probabilities is to speak in terms of fair betting odds. (Howson Urbach 75-76) Given a certain hypothesis, the fair betting odds of that hypothesis would be ones where, if someone were to take those odds and bet for the hypothesis, there would be no expectation of an advantage or disadvantage as opposed to betting against the hypothesis. (Howson Urbach 75) These odds, then, are the prior probability of the hypothesis, or the degree of belief in that hypothesis. P(a) is the bet that the hypothesis is true. P(b | a), then, will be the odds of the evidence, given the truth of the hypothesis. This would be a conditional bet on b, given a. (Howson Urbach 81-82) And these bets, as they do not predict a net advantage for either betting for or against, obey the probability calculus.3 This is important because, as Howson and Urbach claim:

[I]f a set of betting quotients fails to satisfy the probability calculus, then were anybody to bet indifferently on or against the associated hypotheses, at the odds determined by those quotients, he or she could be made to suffer a net loss (or gain) independently of the truth or falsity of those hypotheses. The importance of this

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3 Howson and Urbach present a series of arguments for this claim (Howson Urbach 78-95), which I will defer to, here. The thrust of these sections seems to be that there are so many different arguments, starting in different places, that lead to the same conclusion – the probability calculus. As they say: “The latter [probability calculus] seems, in other words, to be a sort of invariant of different ways of defining uncertainty, or as Lindley puts it, “inevitable”, meaning that the choice of any plausible way of mathematically measuring uncertainty will lead to it. This convergence of arguments has a powerful cumulative effect and increases our conviction that the probability calculus corresponds to some quite objective feature of subjective uncertainty.” (Howson Urbach 95)
result lies in the corollary, that betting quotients that do not satisfy the probability axioms cannot consistently be regarded as determining fair odds. (Howson Urbach 79)

In other words, if these ‘fair odds’ do not satisfy the probability calculus, the probabilities associated with these odds will not represent justified prior probabilities.

There are two elements of this description that are stressed, rightly, by Howson and Urbach. The first is that there need not be a propensity to bet in favor of the hypothesis if the odds are fair, much less does there need to be an acceptance of a bet on those, or greater, odds. (Howson Urbach 77) There may be many reasons as to why a person would not actually entertain, or make, a bet even if the odds are considered fair. It is enough that there is no perceived advantage or disadvantage to someone that would bet for the hypothesis rather than against it. Secondly, Howson and Urbach stress that this discussion of subjectively fair odds needs only that people sometimes perceive odds as being fair. There do not, in fact, have to be fair odds at all in order for there to be subjectively fair odds and, therefore, justified prior probabilities. (Howson Urbach 77)

III. Confirmation and Ad Hoc Hypotheses

Confirmation or disconfirmation of a theory or hypothesis comes as a result of comparing the prior probability of that theory or hypothesis to the posterior probability of that theory or hypothesis, given the evidence that has come to light. If the prior probability of the hypothesis is greater than the posterior probability, then that evidence disconfirms the hypothesis. If the prior probability is lower than the posterior probability, then the evidence confirms the hypothesis. (Howson Urbach 117-118)
In order to determine the posterior probability of a hypothesis, then, it is necessary to know the prior probability of the hypothesis, the prior probability of the evidence and the prior probability of the evidence given the hypothesis.

Consider the following case. There exists some evidence $e'$ that disconfirms $h_a$. Due to the prior probabilities, it is determined that $a$ is to blame. Hypothesis $a'$ is to replace the auxiliary or set of auxiliaries, $a$, refuted by the evidence. According to Howson and Urbach, the posterior probability of $a'$ given $e'$ and given any other relevant information, needs to be greater than .5 in order for that hypothesis to be deemed ‘acceptable’, which means that the hypothesis is more likely to be true than false. The acceptability of a hypothesis given $e'$ and other relevant information will be impacted by the prior probability of this hypothesis.

The theory $t$ to which $a'$ is being added will also be examined to see if it credibly accounts for $e'$. As Howson and Urbach state, “[i]t would do so only if $t \& a'$ was sufficiently credible; since $P(t \& a'|e')$ [is less than or equal to] $P(a'|e')$\(^4\), this would be the case only if $a'$ was itself acceptable, in the sense indicated.” (Howson Urbach 158). So, a theory with an unacceptable modification would be treated with suspicion and considered improbable. The probability of that theory plus the modification given the evidence and other relevant information will be less than the probability of the modifier hypothesis given the evidence and other relevant information, and the probability of an unacceptable hypothesis given the evidence and other relevant information will be less than .5. This will render the probability of the theory plus this hypothesis less than .5,

\(^4\) Strictly speaking, both of these probabilities ought to be conditional on the evidence and the background. So, for example, $P(t \& a'|e \& b)$ instead of just $P(t \& a'|e')$. However, I am taking the background out as it will not affect the outcomes of the probabilities I am discussing.
which would cause the theory plus this hypothesis not to be acceptable: that is, less likely
to be true than false. (Howson Urbach 158)

The discussion of the acceptability and unacceptability of hypotheses and the
acceptability and unacceptability of theories plus modifying hypotheses is important to
Howson and Urbach’s discussion of ad hoc hypotheses. This is because Howson and
Urbach disagree with the traditional account of ad hocness in thinking that the term ‘ad
hoc’ is not always a pejorative, although they do not seem to take issue with Hempel’s
criteria for ad hocness. Howson and Urbach present several examples of ad hoc
hypotheses/theories (Howson Urbach 147-149) in order to support this position. Several
examples are ones where the additional modification is, intuitively, problematic but the
postulation of Neptune to account for the perturbations of Uranus is one where the
modification is, intuitively, unproblematic. Yet, all the examples they label ‘ad hoc’, in
that “[i]t is not likely that they [the new theories] would have been put forward except in
response to particular empirical anomalies, hence the label “ad hoc”, which suggests that

5 According to Hempel, a hypothesis is ad hoc if it is introduced “for the sole purpose of
saving a hypothesis seriously threatened by adverse evidence; it would not be called for
by other findings and, roughly, speaking, it leads to no additional test implications.”
(Hempel 29) Howson and Urbach seem to embrace the idea that ad hocness just is the
introduction for the sole purpose of saving a hypothesis or theory being so threatened.
They do take issue with how the traditional view determines both the no additional test
implications requirement and a related requirement of no independent evidence, claiming
that these requirements are untenable as formulated by the traditional view. (Howson
Urbach 154-157, 158-161) I am sympathetic to at least some of their claims and have
criticized the traditional account in a similar spirit in chapter 1, sections VIII and IX.

6 Howson and Urbach appear to be using the terms ‘theory’ and ‘hypothesis’
interchangeably in this discussion. I have already presented an argument for why it is
only the acceptances of hypotheses that can be properly labeled ‘ad hoc’, in chapter 1,
section XI. However, their argument does not seem to rest heavily on some other notion
of what can be properly labeled ‘ad hoc’. So, if my argument has been convincing, you
may read ‘acceptances of hypotheses’ when Howson and Urbach are speaking of ad
hocness of theories or hypotheses.
the theory was advanced for the specific purpose of evading a difficulty.” (Howson Urbach 149) So, in both the cases where the modification due to disconfirming evidence seems problematic and in the case where it does not, these modifications are considered ad hoc because they were modifications made in response to this disconfirming evidence.

So, Howson and Urbach claim, there seem to be cases where some theory is plausible, such as the theory of the existence of Neptune, even though it is ad hoc. Therefore, ad hocness must sometimes be acceptable, against the traditional view. And, according to them, an acceptable ad hoc hypothesis or theory is one where the probability of that hypothesis or theory, given the evidence and relevant background, is greater than .5: where it is more likely true than false. Considering a theory plus some ad hoc hypothesis, the acceptability of that theory will be determined, in part, by the acceptability of the ad hoc hypothesis because the probability of the theory plus the hypothesis, given the evidence and relevant background, will be at most equal to the probability of the hypothesis, given the evidence and relevant background.7 (Howson Urbach 157-158)

7 Howson and Urbach state explicitly that ad hoc hypotheses or theories can be either acceptable or unacceptable. However, they do seem to imply that it is more likely that ad hoc hypotheses or theories will be unacceptable, in the conclusion of their discussion of the Bayesian treatment of ad hoc hypotheses:

“The Bayesian approach, incidentally, explains why people often respond immediately with incredulity, even derision, on first hearing certain ad hoc hypotheses. It is hardly likely that their amusement stems from perceiving, or even thinking that they perceive, that the hypothesis leads to no new predictions. Surely it is more likely that they are reacting to what they see as the utter implausibility of the hypothesis.” (Howson Urbach 158)
I will not assess the adequacy of Howson and Urbach’s Bayesian account of ad hocness yet. First, I will turn to Strevens’s Bayesian account of ad hocness, highlighting the differences between his and Howson and Urbach’s.

IV. Michael Strevens’s Bayesian Account of Ad Hocness

In “the Bayesian Treatment of Auxiliary Hypotheses”, Michael Strevens main focus is on modifying the Bayesian approach in order to address the Quine-Duhem-Ayer problem. After outlining this solution, he applies his version of Bayesianism to the identification of ad hoc hypotheses. While his solution of the Quine-Duhem-Ayer problem allows for additional information given by the evidence to affect the posterior probabilities, the (interesting) majority of cases are ones where Strevens claims that he can equate the partial posterior probability (the prior probability of, say, \( h \) plus the change in probability of \( h \) due to the falsification of \( ha \)) with the actual posterior probability of, e.g., \( h \) (the prior probability of \( h \) plus the change in probability of \( h \) due to the falsification of \( ha \) plus the change in probability of \( h \) because of information that is irrelevant to the falsification of \( ha \)). These cases occur when the probability of \( e \) given \( h \) and \( \sim a \) is equal to the probability of \( e \) given \( \sim (ha) \). (Strevens 531) I will not get into the details of which sorts of cases will be ones where these two probabilities are not equal: I

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8 See Chapter 1, section VII for a discussion of this problem.

9 Strevens, like Howson and Urbach, seems to use ‘theory’ and ‘hypothesis’ fairly interchangeably throughout his discussion of ad hocness. Sometimes, he talks about ad hoc theories and sometimes about ad hoc hypotheses. I will use his language, for the most part, with the caveat that I will later speak of ‘theory’ and ‘hypothesis’ non-interchangeably in Chapter 5.

10 Strevens thinks that the standard Bayesian approach is not enough to solve the Quine-Duhem-Ayer problem – how to distribute blame among the main hypothesis and its auxiliaries when disconfirming evidence arises – because it does not take into account situations where the disconfirming evidence, in addition to falsifying \( ha \), adds some additional information. This additional information does not indicate anything about the falsity of \( ha \) but might still impact the confirmation of either \( h \) or \( a \). He modifies the Bayesian probability notation to include this additional information when calculating the posterior probabilities. (Strevens 520)
problem diverges from Howson and Urbach, Strevens follows Howson and Urbach in subscribing to a standard definition of ad hocness: the differences between Strevens and Howson and Urbach arise in their interpretation of Bayes’s theorem and how these interpretations make distinctions between types of ad hoc hypotheses.

Suppose there is a hypothesis and its auxiliaries, $h_a$, and disconfirming evidence $e$. Suppose also that $h$ is rescued from falsification by $a'$, where $h a'$ is not disconfirmed by $e$. Then, $a'$ would be ad hoc if it were introduced solely to save $h$ from the disconfirmation brought about by $e$. (Strevens 533) Strevens, in the spirit of Howson and Urbach, then divides the cases of ad hocness into two: desperate rescues and glorious rescues. A desperate rescue, according to Howson and Urbach, is one where the rescue by an ad hoc hypothesis causes $h$ to be much less credible. A glorious rescue, on the other hand, is one where the rescue by an ad hoc hypothesis causes the probability of the ad hoc hypothesis to be greatly increased. (Strevens 534)

There are two ways in which Howson and Urbach’s account differs from Strevens’s. The first is in what sorts of probabilities are to determine the gloriousness or desperateness (or, the acceptability or unacceptability) of a rescue, once an ad hoc

will grant Strevens that most cases are going to be of the type where the two probabilities are, in fact, pretty much equal. What I am interested in outlining, however, is how Strevens thinks that his partial posterior probability theory will deal with cases of seemingly ad hoc hypotheses. And, so, I will concentrate on the differences between Strevens’s account of ad hoc hypotheses and that of Howson and Urbach.

See, for example, Hempel’s account of ad hocness and my discussion of it in sections V-VII of chapter 1.

He is identifying these cases in the spirit of the 2nd edition of Howson and Urbach. The first edition’s description of ad hocness makes it a purely pejorative term.
hypothesis has been introduced\(^\text{13}\) to save the theory or hypothesis from disconfirming evidence. For Howson and Urbach, it is the posterior probability of the new hypothesis given the evidence and any other relevant information, that will determine whether the ad hoc hypothesis introduction is a ‘good’ introduction (one where the scientist follows proper Bayesian reasoning) or a ‘bad’ introduction (one where the scientist does not follow proper Bayesian reasoning). Strevens does not think that this way of determining good and bad introductions explains enough. Specifically, he thinks that it does not explain why the probability of the new hypothesis increases in a glorious rescue, nor does it explain why the probability of the main hypothesis or theory decreases in a desperate rescue. (Strevens 534) Strevens believes that he can answer these questions by focusing on their prior probabilities. I will use the example that Strevens does – the discovery of Neptune – in order to illustrate the distinction that Strevens makes.

According to Strevens, posterior probabilities\(^\text{14}\) have the following features that apply to glorious and desperate rescues:

Recall that, when \(h_a\) is falsified, \(h\) suffers more when \(P(h)\) is smaller and \(P(a)\) is larger\[…\] If \(P(h)\) is very high\[…\] it will decrease very little upon the falsification of \(h_a\). Central hypotheses with lower probabilities will suffer

\(^{13}\) I am using ‘introduce’ because that is the verb that both Howson and Urbach and Strevens use when discussing ad hoc acts. In my own account, the difference between introducing a hypothesis and accepting a hypothesis is important. However, I do not think that either Howson and Urbach’s or Strevens’s accounts are sensitive to this distinction. If preferred, read ‘accept’ where I state ‘introduce’ in discussion of these two accounts.

\(^{14}\) Strevens himself is referring to partial posterior probabilities – the probabilities that can be seen as equivalent to the posterior probabilities when the evidence impacts the probability of, say, \(h\), only insofar as it falsifies \(h_a\). As he thinks that cases where glorious or desperate rescues can occur are cases where these two are equivalent (see Strevens 533), I will use the more familiar ‘posterior probability’ as opposed to Strevens’s phrase ‘partial posterior probability’.
more from the refutation of \( ha \); their rescue will be accompanied by a more embarrassing probability loss. (Strevens 535)

Here, \( P(h) \) is short for the prior probability of the main hypothesis or theory and \( P(a) \) is short for the prior probability of a group of auxiliary hypotheses. The posterior probabilities of the main hypothesis or theory given the evidence that falsifies \( ha \) will be dependent on the prior probabilities of the main hypothesis and the group of auxiliaries. Suppose \( h \) has a high prior probability and \( a \) has a low one. Then, an ad hoc hypothesis introduction that, when combined with \( h \), accommodates \( e \), will get an increase in probability. Suppose, on the other hand, that \( h \) has a low prior probability and \( a \) has a high one. Then, an ad hoc hypothesis introduction will not get as high an increase in probability – if an increase at all. (Strevens 535) And, since the posterior probabilities dictate what counts as a glorious or desperate rescue, and since these are determined largely from the relevant prior probabilities, it is these prior probabilities that will ultimately determine whether or not an ad hoc hypothesis introduction is a glorious or a desperate rescue.

In the case of the discovery of Neptune, Strevens claims, the addition of the auxiliary hypothesis that postulated Neptune’s existence was an example of a glorious rescue. The prior probability of \( h \) (Newtonian theory) was very high at the time and, so, the falsification of \( ha \) (where \( a = \) no other planets) would not greatly depress the posterior probability of \( h \). Additionally, Strevens claims, the ad hoc hypothesis that was added is the most plausible of the alternatives and, so, its posterior probability will also increase when combined with \( h \). As Strevens states it:

In summary, a glorious rescue occurs roughly when \( P(h) \) is considerably higher than \( P(a) \), while a desperate rescue occurs roughly when \( P(h) \) is considerably lower than \( P(a) \). In words, a glorious rescue occurs when the
auxiliary hypothesis receives most of the blame for a false prediction, and is rightly discarded by researchers in favor of some other auxiliary hypothesis that makes the correct prediction. (The degree of glory, I remark in passing, is perhaps inversely proportional to the prior probability of the ad hoc hypothesis.) A desperate rescue occurs when the central hypothesis receives most of the blame for a false prediction, but where researchers cling to the central hypothesis and discard the evidently superior auxiliary. (Strevens 535-536)

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

There is a second way in which Howson and Urbach’s and Strevens’s accounts differ. Howson and Urbach distinguish between ‘good’ ad hoc introductions and ‘bad’ ad hoc introductions by determining whether or not the posterior probability of the hypothesis, given the evidence and the relevant background, is greater than .5 – where the hypothesis is more likely to be true than false. If this probability is greater than .5, then it was a ‘good’, or acceptable, ad hoc introduction. If it is less than .5, then it was a ‘bad’, or unacceptable, ad hoc introduction. Strevens does not make this distinction. Instead, whether a ‘good’ or ‘bad’ ad hoc introduction occurs – whether there is a glorious or a desperate rescue – will be determined by the damage done to the probability of the main hypothesis as a result of the disconfirming evidence. If the main hypothesis’s probability was not damaged, then it is a glorious rescue. If it was badly damaged, then it is a desperate one.
Before critiquing both Bayesian accounts, I will apply both accounts of ad hocness to several examples, beginning with the Ptolemy example.

V. The Bayesians versus Ptolemy

I am going to apply both Bayesian accounts of ad hocness to this and the other examples, due to the two differences in determining good and bad ad hoc introductions that they have. I will start with Strevens’s account.

For Strevens’s account to apply to the Ptolemy example, it has to be a case such that the partial probability will equal the posterior probability. I will suppose this to be true. After ceding this point to Strevens, I’ll examine the Ptolemy case. $h$ will consist

$15$ Strevens presents 3 criteria to be fulfilled in order for this to occur.

1. $P(e|h) = P(e|\neg(h))$

2. There is no real change in the central hypothesis’s probability due to factors other than the falsification of $ha$: that is, that $\delta_c$ – the change in probability due to information contained in the evidence that is irrelevant to the falsification of $ha$ – is close to zero. $\delta_c$ is from $P^+(h) = P(h) + \delta_{qd} + \delta_c$ (Strevens 520, 521, 531), where $P^+(h)$ is the posterior probability of $h$, $P(h)$ is the prior probability of $h$, $\delta_{qd}$ is the change in probability of $h$ due to the falsification of $ha$, and $\delta_c$ is the change in probability of $h$ because of information that is irrelevant to the falsification of $ha$ – that is, the additional information carried by $e$. This last part is Strevens’s major modification, which he believes allows him to properly assess the posterior probabilities in situations where information irrelevant to the falsification of $ha$ will still impact (potentially greatly) the posterior probability of a given $h$. (Strevens 521)

See, e.g., Strevens’ Newstein example, where you are very confident of a certain scientist’s abilities. This scientist claims that $h$ is true and, additionally, that $e$ will be observed. (Strevens 521) Then, if either one of these statements is found to be true, your expectation that the other will be true will rise dramatically. (Strevens 521) In this way, information irrelevant to the falsification of some $ha$ will impact the posterior probability of that $h$.

3. $P(e|h_b) = P(e|\neg(ha))$
in Ptolemy’s eight historical commitments: 1) the movement of the heavens is spherical; 2) the heavens themselves are spherical; 3) heavenly objects are spherical; 4) the Earth is spherical; 5) the Earth is the center of the heavens; 6) the fixed stars are so far away from the Earth that the Earth is like a point to them; 7) the Earth does not, itself, move in any way; 8) there are 2 motions to the heavens. (Ptolemy 7) 


a will consist in the other auxiliaries to which Ptolemy is committed. These will include hypotheses needed to test Ptolemy’s theory and will include the claim that the heavenly bodies move in perfect circles *concentric* to the Earth. The disconfirming evidence, *e*, will be the observations of the sun that seem to show the sun moving in unequal distances in equal times as observed from the Earth.

*ha* does seem to be falsified by *e*. It does not, in fact, appear that heavenly bodies move in perfectly circular orbits centered at the Earth. (Strevens 533) However, there is

where *b* represents other possible alternative auxiliaries.

I need to grant these conditions because, otherwise, \( P(e|h-a) \) will not equal \( P(e|\neg(ha)) \) and Strevens’s account would not apply. These conditions relate back to the discussion in footnote 10. As I stated, Strevens thinks that situations where a hypothesis introduction can be ad hoc and either glorious or desperate are situations where the partial posterior probability is (approximately) equal to the posterior probability. These are situations where information from the evidence that does not falsify *ha* also does not cause a change in probability. These conditions are the ones that Strevens claims a hypothesis introduction must pass in order for the partial to be (approximately) equal to the actual posterior probability. Otherwise, we would be dealing with situations such as the Newstein example mentioned earlier in this footnote.

16 A critic might argue that the Ptolemy case does not, in fact, fulfill these three conditions. As these conditions need to be fulfilled in order for a charge of ad hocness to be able to be appropriate, so much the worse for Strevens’s account. If this case fails to meet the criteria, then the cases I discuss in section VI will also fail, as their structures are identical to the structure of the Ptolemy case. The critic might argue that there is no strong intuition concerning the ad hocness of the Ptolemy case and, therefore, that it is failing to meet the criteria is not a problem for Strevens. The other two cases failing to meet the criteria would definitely be a problem for the critic’s argument, however, as these two are very clear intuitive examples of ad hocness.
an alternative $a'$ to $a$ such that $ha'$ is consistent with $e$. The alternative $a'$ will include, perhaps, the hypothesis that our eyes see apparently uncircular orbits because the heavenly bodies travel on perfectly circular orbits whose centers are not located at the center of the Earth. Then, by adding some additional specifications to $a'$—how these orbits are constructed or where their centers are located, Ptolemy’s theory can be “rescued from falsification”. (Strevens 533) The additional information might come from either the eccentric hypothesis or the epicyclic hypothesis.

As Strevens appeals to a traditional definition of ad hocness, the alternative $a'$ will be considered ad hoc if it were added solely to rescue Ptolemy’s theory from the disconfirming evidence. (Strevens 533) How should we determine whether Ptolemy decided to modify his theory to include eccentrics in order to rescue his theory from $e$? Not being able to ask, we must look at what he wrote concerning the eccentric hypothesis and the epicyclic hypothesis. As quoted previously, Ptolemy states: “the cause of this irregular appearance can be accounted for by as many as two primary simple hypotheses [the eccentric and the epicyclic].” (Ptolemy 87) That he presents the eccentric and the epicyclic hypotheses as ways to account for the disconfirming evidence suggests that Ptolemy might have added either of these hypotheses to his theory in order to save his theory from the disconfirming evidence of the sun’s path in the heavens. This, of course, is not proof that Ptolemy was thinking in this way but, at least, these words make it seem plausible that Ptolemy was picking out a hypothesis in order to rescue his theory from the evidence. So, on Strevens’s account, it would appear that the eccentric hypothesis brought in by Ptolemy was ad hoc.
Note the differences between Strevens’s account’s treatment of the eccentric hypothesis’s addition and my discussion of the Ptolemy case in chapter 1, section IV. On Strevens’s treatment, the fact that there were two different, interchangeable hypotheses, either of which could have been used to modify Ptolemy’s theory, does not impinge on whether or not the chosen hypothesis was ad hoc. This is because the ad hocness of a hypothesis arises in the motivations for moving to $a'$ from $a$. Additionally, like Howson and Urbach, Strevens does not claim that being ad hoc ought always to be a source of criticism.

I have not yet completed Strevens’s account’s treatment of the Ptolemy case. It has been determined, fairly easily, that Ptolemy’s $a'$, which included the eccentric hypothesis, was ad hoc. What still must be determined is whether the event of changing to $ha'$ from $ha$ was a glorious or desperate rescue.

In a glorious rescue, the probability of the modified set of auxiliary hypotheses, including the new hypothesis, is increased. Thus, glorious rescues seem to be justified events, according to Strevens. Desperate rescues, on the other hand, seem to detract from the legitimacy of the central theory, as the probability of the central hypothesis goes down because of the conditionalizing on new evidence, yet the scientist does not respond appropriately. (Strevens 534) Instead of questioning the central hypothesis, the scientist holds on to it, replacing the auxiliary instead. It seems, according to Strevens, that the prior probability of the central theory will greatly influence whether a certain case is a case of a desperate or glorious rescue, as evidenced by the quotations and discussion in the previous section. It now needs to be determined whether or not Ptolemy’s theory was well regarded and, so, whether its prior probability was high.
Examining Strevens’s treatment of Newtonian theory in the face of the perturbations of Uranus will shed light on this issue. The probability of $h$ – Newton’s theory of gravitation\(^\text{17}\) - was very high at the time that the perturbations of Uranus caused Adams and Leverrier to independently postulate the existence of an additional planet, Neptune. One of the reasons that the probability of Newtonian gravitation was so high, it seems, is because there weren’t any other plausible rivals to Newtonian gravitation theory that explained the evidence. (Strevens 535) So, the high prior probability of Newtonian gravitation, compounded by the lack of plausible rivals, makes the modification of Newtonian theory to include the existence of another planet a glorious rescue.

And similarly, so, with the Ptolemy case. Ptolemy’s theory was, arguably, without any real rivals. And, in the Newtonian case, it was important that there were no plausible rivals: this helped elevate the prior probability of Newtonian gravitation. The prior probability of Ptolemaic astronomy would also likely be very high, as it had no real rivals and had been much more successful than its predecessors such as Aristotelian astronomy. As Ptolemaic astronomy’s prior probability may be considered quite high, the probability of \(a\) would have suffered greatly. Its alternatives that made \(e\), in conjunction with \(h\), more probable would have then seen an increase in probability. The eccentric hypothesis was created to solve the problem of the sun’s anomaly, so its probability will rise. The combined \(ha’\) will then have a high posterior probability. Thus, I would argue, Ptolemy’s modification could be seen as a glorious rescue. Yet, our intuitions seem to tell us that the Ptolemy case is a clear case of a bad act: a hypothesis

\(^{17}\) Strevens is not entirely clear as to whether he means Newtonian theory in general or Newtonian theory of gravitation. However, I do not think that the choice matters much for the discussion at hand.
acceptance that shouldn’t have occurred at the time that it did. This result, while not damning for Strevens, is still one that ought to cause some concern for Strevens. A good account of ad hocness should either account for our intuitions in the clearer cases of ad hocness or should give us a compelling reason as to why the counter-intuitive result should obtain. Strevens’s account does not do the first and, I will argue, does not do the second. This is because the elements of his theory that Strevens touts as beneficial – the Bayesianism and the glorious and desperate rescues – are not as felicitous as he thinks they are. This should become clear in what follows.

Next, I will apply Howson and Urbach’s account to the Ptolemy case, with the same variables consisting in the same propositions. So, \( a' \) will represent the eccentric hypothesis, \( e' \) will represent the anomalous orbit of the sun. \( b \) represents any relevant background information. In my actual calculations, I will be dropping this variable, as it does not affect them.

That the introduction of the eccentric hypothesis is considered ad hoc should come as no surprise, as both Howson and Urbach and Strevens use the same criterion for determining ad hocness. The second step is to determine whether this ad hoc introduction is acceptable or not, and then whether the theory, in combination with this hypothesis, credibly accounts for the disconfirming evidence. (Howson Urbach 158) The eccentric hypothesis is acceptable if the probability of the eccentric hypothesis given the evidence and other relevant information is greater than .5. This probability is equal to the likelihood of the hypothesis multiplied by the prior probability of the hypothesis, all divided by the probability of the data. (Howson Urbach 26, 110) Or, in equation form:

\[
P(a'|e') = \frac{(P(e'|a') P(a'))}{P(e')}
\]
As previous astronomical theories, such as Aristotle’s, held that all heavenly bodies had perfectly circular orbits centered at Earth and Ptolemaic theory, prior to modification, held the same, $P(a')$ would have been quite small and $P(e')$ even smaller. Suppose $P(a') = .1$; $P(e') = .09$; $P(e' | a') = .6$, where $P(e' | a') = (P(e' & a')) / P(a')$. Then, $P(a' | e') = .667$ and the introduction of the eccentric hypothesis will be acceptable, on Howson and Urbach’s account. These numbers are not implausible, although a change in the prior probabilities on the right hand side of the equation will lead us to different posterior probabilities of the eccentric and, given enough number manipulation, might cause the introduction of the eccentric hypothesis to be rendered unacceptable.

As with Strevens’s account, labeling the eccentric hypothesis an acceptable ad hoc hypothesis introduction is something that should cause concern for Howson and Urbach, for similar reasons as I gave for Strevens. Notice, too, that Howson and Urbach’s verdict on the epicyclic hypothesis will likely be very similar to the verdict for the eccentric hypothesis, and the same for Strevens. Also, given that a hypothesis is ad hoc, whether this hypothesis introduction is good or bad is independent of there being two hypotheses, either of which could have been used to modify Ptolemy’s theory. This is because the ad hocness of a hypothesis arises in the motivations for moving to $a'$ from $a$.

VI. The Bayesians versus the Lab-Break In Case and the Philosopher’s Ptolemy Case

For both Strevens’s and Howson and Urbach’s accounts, introducing the eccentric hypothesis turned out to be ‘good’ ad hoc – that is, for Strevens, it is a glorious rescue
and for Howson and Urbach, it is an acceptable introduction. I noted that these results, while not damning for either account, are still results that ought to cause some concern for both Strevens and Howson and Urbach. A good account of ad hocness should either account for our intuitions in the clearer cases of ad hocness or should give us a compelling reason as to why the counter-intuitive result should obtain. I will run both accounts through two examples, both with similar structures to that of the Ptolemy case, in order to make these results look even worse for both of these accounts. Here’s the first:

**Break-in case:** Brady accepts the theory that mercury, under standard atmospheric pressure, boils at 357°C. Brady tests this theory (in conjunction with certain auxiliary hypotheses) by heating mercury and having his lab assistant record the temperature at which it boils in a notebook. The next day, Brady checks the book and finds that the temperature written down is 359°C. In the face of this disconfirming evidence, Brady rejects the auxiliary hypothesis that the temperature written in the book is the one that the assistant wrote down the previous day and accepts the hypothesis that someone broke into his lab and changed the temperature in the notebook.

Notice the similarity in structure to the Ptolemy case. Both Strevens and Howson and Urbach will count the introduction of the temperature-changing hypothesis as ad hoc, as it is introduced solely to save the theory that mercury boils at a certain temperature from the disconfirming evidence of the temperature written down by his lab assistant in the notebook. Both Strevens and Howson and Urbach counted the introduction of the eccentric hypothesis as ad hoc for a similar reason. Additionally, the fact that there were two different, interchangeable hypotheses, either of which could have been used to modify Ptolemy’s theory, makes no difference to Strevens and Howson and Urbach’s treatment of it; similarly in the lab break-in case, although in its case there are more than two different, interchangeable hypotheses. His assistant could have accidentally written
the temperature wrong, for example. Or, the room might not have been at standard atmospheric pressure. Or, the substance being tested might not have really been mercury.

To determine whether the temperature-changing hypothesis is a glorious or a desperate rescue is to determine whether the prior probability of the mercury-temperature theory was high. One of the reasons for it to be high would be the lack of other plausible rivals to it that explained the (previous) evidence. Suppose Brady has run this experiment many times and has previously obtained the results that mercury, under standard atmospheric conditions, boiled at 357°C. This information would cause the prior probability of the mercury-boiling theory to be high. It will also be quite high because there don’t seem to be any other plausible rivals to this theory that explain the evidence. So, the high prior probability of the mercury-boiling theory, compounded by the lack of plausible rivals, makes the modification of the mercury-boiling theory to include the temperature-changing hypothesis a glorious rescue. Again, this is a problematic result for Strevens’s account.

Next is to determine the outcome for Howson and Urbach’s account. To determine whether a hypothesis introduction is acceptable or not is to determining whether the probability of the new hypothesis, given the evidence and relevant background information, is greater than .5. Again, using

\[ P(a'|e') = \frac{P(e'|a') P(a')} {P(e')} \]

where \( P(a') = .05 \); \( P(e') = .04 \); \( P(e'|a') = .75 \) (both the evidence and the temperature-changing hypothesis being initially very improbable), the posterior probability may be calculated

\[ (0.05 \times 0.75) / 0.04 = 0.9375 \]
The posterior probability of the temperature-changing hypothesis comes out to be about 0.94 and, so, will be an example of an acceptable hypothesis introduction.\textsuperscript{18} In a case with an analogous structure to the Ptolemy case, and where our intuitions even more strongly indicate that this hypothesis introduction is suspect, Howson and Urbach’s account deems this introduction acceptable. This seems problematic for their account.

The second example is the philosopher’s Ptolemy case, which I introduced in chapter 1, section X. This example is very similar to the original Ptolemy case, with the exception that the structure is then repeated several times.

The initial calculations for both Strevens and Howson and Urbach in the philosopher’s Ptolemy case will be the same as in the original Ptolemy case, as again either an eccentric or an epicyclic hypothesis will be introduced in order to save the theory from disconfirming evidence. So, the first modification will be a glorious rescue or an acceptable introduction. And, both theories ignore the fact that there are other, interchangeable hypotheses that might have been introduced. What I will need to determine is whether the additional modifications come out differently than the first.

In a glorious rescue, the prior probability of $h$ was very high and, so, the probability of $h$ will decrease very little when $ha$ is falsified. Discarding $a$ and adding $a'$, whose probability will greatly increase as a result of $e$, will cause the scientist to have

\textsuperscript{18} Again, as in the Ptolemy case, one might argue with my initial probabilities. However, it does seem likely that the prior probabilities of both the temperature-changing hypothesis and the evidence in the notebook would be very small. I will discuss a related problem in section X of this chapter, when I discuss the differences that prior probabilities make. It seems like the priors make too much of a difference in determining the acceptability or unacceptability of a certain hypothesis introduction, given that there seems to be a plausible variation in these values, when assigned, depending on what is perceived as the ‘fair betting odds’ on them.
accepted a hypothesis where the probability of $h'a'$ is equal, or almost equal, to the probability of $ha$ before $e$ came to light. After the first modification, the probability of Ptolemaic astronomy will still be very high (for the above reason and also because there are no plausible alternatives to it). So, it seems, the next modification will also be a glorious rescue, and the next. As the probability of $h$ may decrease a bit after each piece of new evidence arises that disconfirms $ha$ (where $a$ is whatever set of auxiliaries or auxiliary that ends up being replaced with $a'$ when the rescue occurs), it is certainly possible that, after some number of iterations, the rescue will no longer be considered glorious. How much iteration this would need is not clear. Why would it be, though, that one particular modification would be a desperate move when other, identical modifications were not? And, just as with the first modification, the desperateness or gloriousness of each successive modification will be invariant on whether there are other, interchangeable hypotheses that might be used to modify the theory.

An analogous assessment will occur on Howson and Urbach’s account. Where the difference might lie would be on the number of iterations that it would take for the given hypothesis introduction to be unacceptable: it would seem that this point would come more quickly for Howson and Urbach than for Strevens. However, Howson and Urbach’s account will be equally invariant on whether there are other, interchangeable hypotheses that one might introduce.

The lab break-in case and the philosopher’s Ptolemy case, as they are structurally similar to the original Ptolemy case, serve to underscore the fact that both Strevens and Howson and Urbach’s accounts evaluate these types of cases as positive ad hoc introductions, which seems to get these cases wrong. Even more importantly, why
Strevens and Howson and Urbach’s accounts get the cases wrong is because of these accounts’ insensitivity to cases where there is only one possible hypothesis that will reconcile the theory with the disconfirming evidence, as opposed to cases where there are multiple alternatives. I will proceed by highlighting other problems for both Howson and Urbach and Strevens’s accounts of ad hocness.

VII. A Divergence in Terminology from Both Howson and Urbach and Strevens and a Gesture to a Later Objection

The Bayesian accounts I have discussed all share a version of the standard definition of ad hocness. They claim that a hypothesis is ad hoc when the hypothesis is introduced for the reason that this hypothesis can rescue the central thesis from disconfirming evidence. As a result of their reliance on this definition of ad hocness, two points arise for these Bayesians. Firstly, these Bayesians do not believe that to be ad hoc is either acceptable or unacceptable. Ad hocness is merely a psychological characteristic on the Bayesians’ accounts. This is in contrast to Hempel (as well as Leplin, Sober and myself), who believes that ad hocness has a normative significance. On Strevens’s account, normativity does not enter the picture until he further separates the types of ad hoc hypotheses into glorious rescues and desperate rescues. Similarly on Howson and Urbach’s account, when separating acceptable and unacceptable hypotheses.

Which brings me to the second point that this definition of ad hocness raises. Ad hocness is a psychological characteristic on the Bayesians’ accounts because this definition commits us to claiming that the ad hocness of a hypothesis depends on the
psychology of the scientists introducing the hypothesis. It is ad hoc only if the scientists had certain motivations for introducing it.

In fact, this might be a slightly misleading way to put this point, as the psychological characterization of ad hocness might seem to rest solely on the fact that Howson and Urbach, and Strevens, use this particular definition of ad hocness. It is true that the particular way in which this characterization of ad hocness is psychological in nature is a result of this definition. However, the Bayesian accounts I’ve considered, no matter their definition of ad hocness, will make ad hocness be a psychological, and not a normative, characteristic. This is because the possibility for normative claims arises in these accounts only at certain points when updating one’s beliefs using Bayesian conditionalization. The first point normative claims can be made is when the prior probabilities are determined. These priors will either constitute ‘fair odds’, or not. Then, normative claims can be made as to whether you update your beliefs according to Bayes’s theorem. Suppose you update your beliefs due to some piece of evidence and there are two possible hypotheses you can accept. One has a much higher posterior probability, given the evidence, than the other. Then, a Bayesian might say that you were unjustified in choosing the one with the lower posterior probability. There are no other points at which normative claims can be made. Determinations of ad hocness are not made at either of the normative points, so they cannot be normative claims. A similar point may be made for any Bayesian treatment of ad hocness that has a similar structure to the accounts that I’ve considered.

One might think that, in fact, the psychology of the scientists is very important to whether or not a hypothesis is ad hoc. After all, if the scientist does not think that the
hypothesis will add anything else to the theory – additional predictions, e.g. – then it
seems less likely that the hypothesis is a good one, or that the hypothesis is less likely to
be true – or so the thinking goes. I address this in chapter 1, section VIII. It might well
be that a scientist is more justified in believing in a hypothesis if his motivations are
exemplary in introducing a certain hypothesis to a given theory in the face of recalcitrant
evidence and, so, a scientist might be less justified or not justified in believing in a
hypothesis introduced solely to solve a problem. However, this is an epistemological
point, whereas determining whether something is ad hoc is a methodological issue. In
issues of methodology, the rule or guidelines created to deal with these issues do not tell
the scientists what motivations to have. Therefore, the motivations of the scientists ought
to be irrelevant in determining ad hocness.

I will return to the Bayesian normative account of ad hocness in the next section.

VIII. Ad Hocness Ought Not Be the Umbrella Term for Two Distinct Phenomena

Strevens’s account of ad hocness includes two different types of ad hoc
hypotheses – the ones that are a part of a glorious rescue and those that are a part of a
desperate rescue. Howson and Urbach’s account of ad hocness also includes two
different types of ad hoc hypotheses – the acceptable and the unacceptable. And it looks
like any similar Bayesian account of ad hocness will also include two different types of
ad hoc hypotheses, whatever their labels. I will use Strevens’s account as the illustration
for the problem found in so doing, but the critique will apply to all of these Bayesian
accounts of ad hocness.
The problem in so doing is twofold. The first problem is that ad hocness is used as a pejorative in science and in (most of) philosophy. The Bayesians will have to give a good reason as to why the glorious rescues are actually ad hoc moves at all. Strevens, for example, differentiates between the introduction of good ad hoc hypotheses and bad ad hoc hypotheses – that is, the types of ad hoc hypotheses that are justified and those that are not. This is the distinction identified in the distinction between glorious and desperate rescues. However, this sort of heterogeneity of ad hocness runs strongly counter to how scientists use the term ‘ad hoc’. Having part of one’s theory labeled ‘ad hoc’ is considered a detriment to the theory, not a form of approbation. Something that philosophers of science must be sensitive to is to how terms are actually used in science. If we are to differ markedly in our use of terminology, we must give a good reason for so doing. I do not think that Strevens has done this. I will argue this point more both in the second, related problem in this section and also in the criticism found in the next section.

The second, related, problem is that, on occasions where certain types of acts can be clearly distinguished, and there is a reason to so distinguish them, it is a mistake to place such acts under the same umbrella term. For example, Strevens’s label ‘glorious rescue’ in the Uranus perturbation case seem to be another way of labeling an instance where it was rationally permissible (maybe even encouraged) for scientists to postulate the existence of another planet to account for these perturbations. To label some act a desperate rescue seems to be another way of labeling an instance where it was not rationally permissible for scientists to postulate such a hypothesis. To label the former as a good ad hoc move and the latter as a bad one seems to unnecessarily complicate the issue.
In sections VII-X of Chapter 1, I discuss instances where there does not seem to be any principled way to make a distinction between two types of hypotheses or two types of acts. In contrast, there seems, here, to be a major and clear distinction between a move that is rationally permissible to make and one that is not rationally permissible to make. Why not respect this distinction, and the way in which scientists use the term ‘ad hoc’, and label the rationally impermissible moves ‘ad hoc’ and the rationally permissible moves as good scientific methodology?

After all, an important part of scientific methodology is to modify one’s theories in the light of disconfirming evidence. To do otherwise would be either to throw over a theory the instant disconfirming evidence arose, no matter the theory’s virtues, or to maintain theories that will be empirically inadequate. These modifications are being made because of disconfirming evidence, which seems to point to the possibility of their being ad hoc. To label indiscriminately all (or most) of these types of theory modifications as ad hoc is either to misrepresent what is going on or to render the term ‘ad hoc’ rather meaningless. It will become meaningless if it embodies too many actions, especially if the actions that it embodies are very heterogeneous, or if the term

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19 In “The Assessment of Auxiliary Hypotheses”, Jarrett Leplin puts this point quite well: “If in fact ‘ad hoc’ is used in science to mark a particular methodological liability, if it has the univocity I have claimed in making this liability a necessary condition of its application, then a neutral epistemic analysis, which must distinguish as many senses of ‘ad hoc’ as there are forms of empirical deficiency, radically underestimates the scientific importance of the concept. […] In so far as ‘ad hoc’ is subjected to distinctions of sense, there is a natural inclination, exhibited by many along the slippery path to Holton, to attribute such differences to differences in usage whose only legitimate significance is biographical. In this situation, if it can be shown that a common judgment was made in even a small number of importantly different cases by use of the concept, then univocity should be presumed pending evidence to the contrary. The distinguishing of sense so popular from ordinary language philosophy is a valuable tool only if not exploited ad hoc to convert philosophical problems into historical ones.” (Leplin 1982 240-241)
goes too far afield from scientific practice. If the term is meaningless, why use it at all? I don’t find any of these consequences acceptable.

**IX. Too Much Depends on Prior Probabilities**

With Fitelson and Waterman, I claim that Strevens’s conception of desperate and glorious rescues ends up hinging almost exclusively on the prior probabilities of \( h \), which does not seem right, for several reasons. Fitelson and Waterman, in “Bayesian Confirmation and Auxiliary Hypotheses Revisited: A Reply to Strevens”, criticize Strevens’s solution to the Quine-Duhem-Ayer problem: a solution that I do not discuss in the body of this chapter\(^{20}\), as the distinction that Strevens makes does not affect the nature of ad hoc hypotheses, according to him. However, a brief digression into Strevens’s solution and Fitelson and Waterman’s critique of it will show how an assumption that Strevens makes in the solution to the Quine-Duhem-Ayer problem will bleed into his criteria for desperate and glorious rescues.

Fitelson and Waterman claim that Strevens makes a critical oversimplification when characterizing the problem, which causes other problems for Strevens’s theory. According to them, Strevens claims that:

\[ e \text{ is equivalent to } \neg(h \land a) \]  

(Fitelson and Waterman 294)

where \( e \) is the disconfirming evidence, \( h \) is the central hypothesis and \( a \) is the set of auxiliary hypotheses. Fitelson and Waterman’s issue with this simplification is that the simplification says that \( e \) consists of no more than the fact that \( h \) and \( a \) both aren’t true.

\(^{20}\) Although I do discuss the essentials of it in footnotes 10 and 15 of this chapter.
As a result of this, they argue, Strevens’s later claim that the relationship between the confirmation of \( h \) given \( e \) and the confirmation of \( a \) given \( e \) will depend on both the prior probabilities of \( h \) and \( a \), as well as conditional probabilities such as the probability of \( a \) given \( h \) and \( h \) given \( a \), is a false one. (Fitelson and Waterman 295) In fact, they claim, the simplification of the Quine-Duhem-Ayer problem makes the comparison of the confirmation of \( h \) given \( e \) and the confirmation of \( a \) given \( e \) just a comparison between the prior probabilities of \( h \) and \( a \). This makes the disconfirming evidence irrelevant to the relationship between these two confirmations. (Fitelson and Waterman 295) And, as Fitelson and Waterman state: “[i]t seems clear to us that, in the original Q-D problem, the mere relationship between the priors of \( H \) and \( A \) should not by itself determine the relative support that \( E \) provides for \( H \) vs. \( A \).” (Fitelson and Waterman 295) They continue the critique by saying:

We take Quine and Duhem to be asking the following question: In cases where \( H \land A \) entails \( \neg E \), can the evidence \( E \) differentially confirm \( H \) vs. \( A \) – *a posteriori* – and, if so, *how*? This is *not* a question about the relative *a priori* plausibilities of \( H \) vs. \( A \) but rather a question about the *a posteriori* confirmational power of \( E \) to discriminate between \( H \) and \( A \) when \( H \land A \) entails \( \neg E \). (Fitelson and Waterman 296)

In other words, Strevens’s solution of the Quine-Duhem-Ayer problem misses its mark. It misses its mark because it relies solely on the prior probabilities of \( H \) and \( A \) to determine which ought to be thrown aside or modified given a certain piece of disconfirming evidence.

That Strevens’s account doesn’t adequately solve the Quine-Duhem-Ayer problem is an issue for Strevens in a couple of ways. The first issue is that other attempts

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\(^{21}\) Strevens himself seems to be hinting at something like this in the two excerpts quoted in section IV of this chapter. (Strevens 535, 535-536)
to define ad hocness – such as Hempel’s – fall into the Quine-Duhem-Ayer problem and, thus, are not good definitions of ad hocness for the reasons stated in chapter 1, section IX. Therefore, it does not appear that Strevens can try to get out of the problem via any of the pathways that the other, failed, attempts take.\footnote{Fitelson and Waterman claim that the likelihood-measure \( l \) is a better Bayesian measure of confirmation and Bayesian accounts using \( l \) can better deal with the Quine-Duhem-Ayer problem. I will not evaluate these claims here.}

The other way that Fitelson and Waterman’s critique is problematic for Strevens occurs as a result of Strevens’s distinction between glorious and desperate rescues. If the confirmation of \( h \) given \( e \) and the confirmation of \( a \) given \( e \) are based merely on the prior probabilities of both \( h \) and \( a \), then whether a rescue is glorious or desperate will also be based on these prior probabilities. This is a problem because it would seem that, given a relatively well-established theory without serious rivals, any rescue of it would be a glorious – and, therefore, justified or, at least, permissible – one. This is not a happy outcome, for it seems perfectly plausible to think that there could be cases (such as the Ptolemy case) where, because of disconfirming evidence, the introduction of a hypothesis into a well-established theory without any serious rivals ought to be desperate, unjustified. However, this sort of outcome looks very improbable, given the dependence of Strevens’s account on the prior probabilities of \( h \) and \( a \). Alternatively, it would seem that a newly presented theory, introducing a hypothesis because of disconfirming evidence...
evidence, would almost always be committing a desperate rescue. This, too, seems an unfortunate consequence.

To have ad hocness reduce to whether or not a hypothesis is being introduced into a well-established theory or a non-well-established theory because of disconfirming evidence creates two negative consequences. Firstly, that there are good and bad ad hoc acts does not map on to scientists’ use of ad hoc. Secondly, as ad hocness is equivalent to the prior probabilities of theories, the vice of ad hocness just refers to the modifications, due to disconfirming evidence, to a theory with a low prior probability and the virtue of ad hocness refers to the relevant modification to a theory with a high prior probability. This is to place the emphasis, and to move the label ‘ad hoc’, on the theories themselves, without taking into account the particular hypothesis being accepted. Yet, this is precisely where the emphasis, and the label ‘ad hoc’, should be placed. And, Fitelson and Waterman have pointed to the same problem of emphasis in Strevens’s attempt at a solution to the Quine-Duhem-Ayer problem.

**IX. The Problem of Prior Probabilities for the Possible a’s**

As evidenced by the previous section, the designation of a glorious, versus desperate, rescue is determined almost exclusively by the prior probability of the theory being modified due to disconfirming evidence. However, there is another determiner for that designation. In a glorious rescue, the probability of the modified set of auxiliary hypothesis, including the new hypothesis, is increased (usually dramatically). In order for such an increase to occur, the prior probability of $a$ (before the advent of the
evidence) must be rather low. This is because there will not be an appreciable increase in
the probability of the modified hypothesis if its prior probability were already high.\textsuperscript{24} In
looking at the original Ptolemy case, an interesting problem will arise concerning the
prior probability of $a'$. In the original Ptolemy case, there were two different hypotheses from which to
modify the auxiliaries: the eccentric and the epicyclic. Whichever one will get the
biggest boost in probability seems to be the hypothesis that ought to be adopted as $a'$. The Bayesian will claim that the $a'$ chosen should have the highest boost in probability so
that $h + a$, given the evidence and relevant information, will have the highest posterior
probability. So, it looks like whichever of the eccentric and the epicyclic gets chosen
ought to be on the basis of its prior probability. This way of deciding which hypothesis
to use seems to miss the point again. It is not going to be a good thing to choose a
hypothesis on the basis of it having a low prior probability before the advent of $e$. Not
only does this seem wrong-headed, it also doesn’t get to the point of the problem. There
is a problem here because Ptolemy has two, empirically equivalent hypotheses from
which to choose and no criteria upon which to make the decision. Yet, he makes the
decision anyway. That one has a lower prior probability than the other is not giving him
sufficient criteria to decide.\textsuperscript{25}

\textsuperscript{24} A similar situation is found in Howson and Urbach’s criteria for acceptable and
unacceptable hypotheses and acceptable and unacceptable modified theories.

\textsuperscript{25} Strevens might claim, here, that this case cannot be decided using his version of
Bayesianism because he relies on the partial posterior being equal to the true posterior
probability in order to get around the Quine-Duhem-Ayer problem. His claim is that, in
most (interesting) cases, his three criteria will be fulfilled and the partial posterior will be
equal to the true posterior probability, which information, he claims, can be used to
determine ad hocness and glorious versus desperate rescues. (Strevens 529-531, 533)
In order to highlight the importance of this problem, I will redescribe the original Ptolemy case. Suppose the three following scenarios: (1) the eccentric hypothesis is introduced and it is the only alternative to \(a\); (2) the epicyclic hypothesis is introduced and it is the only alternative to \(a\); (3) either the epicyclic or the eccentric hypothesis is introduced and they are both alternatives to \(a\). If the prior probability of Ptolemy’s eight commitments is (much) greater than the prior probability of the \(a\) that is discarded, all three of these scenarios will be examples of glorious rescues. So, whichever rescue will be glorious in these conditions, despite the fact that two scenarios provide only one alternative to \(a\), while the third provides more than one. \(^{26}\)

\[X. \text{ Conclusion}\]

Due to their requirements that ad hocness be merely psychological in character, and due to the problems faced by these theories as a result of their insensitivity to the available number of possible modifications, both Strevens’s and Howson and Urbach’s Bayesianism fail to give an adequate account of ad hocness. Strevens’s account, in particular, fails to give an account of ad hocness where a well-established theory could be

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\(^{26}\) In fact, I could run this same sort of argument for the other two examples discussed in section VI. That I can do this strengthens my claim. I have left out the treatment of the lab break-in case and the philosopher’s Ptolemy case because of length considerations. However, their outcomes are the same as the original Ptolemy case when run through this argument.
modified in such a way that it was a desperate rescue and vice versa for not-well-established theories. So, we will have to turn elsewhere to find an acceptable account of ad hocness.
Chapter Three
Jarrett Leplin’s Account of Ad Hocness: The Closest to Being Successful

I. Jarrett Leplin’s Criteria of Ad Hocness

Jarrett Leplin’s view, as formulated in “The Concept of an Ad Hoc Hypothesis” and revised in “The Assessment of Auxiliary Hypotheses”, is closer to my own view in certain ways than is Strevens’s. Leplin’s definition of ad hocness, for instance, is a methodological rather than an epistemic one.¹ (Leplin 1982 236) Leplin and I both think that the appeal to the psychology or biography of scientists is not the appropriate way to understand what makes something ad hoc. Leplin also considers ad hocness always to be a vice, as I do. As well, he and I both think that there is and ought to be a distinction between something being false or improbable and it being ad hoc. (Leplin 1982 236) However, I think that his criteria fail, ultimately, to properly characterize the nature of ad hocness. After discussing Leplin’s view, I will demonstrate how his criteria do fail.

In “The Concept of an Ad Hoc Hypothesis”, Leplin states his account of ad hocness. He thinks, in this paper, that he has found individually necessary and jointly sufficient conditions for ad hocness. (Leplin 1975 333) As Leplin states it:

¹ Our conceptions of what is methodologically suspect in ad hoc acts are quite different, however.
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,
   (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 1975 336-337)

Leplin seems to emphasize different elements of his theory of ad hocness in the 1982 paper than he does in the 1975 paper. After detailing the criteria, I will discuss both characterizations of Leplin’s view, in order that I present the most charitable reading.

I will concentrate my discussion on the more contested conditions: conditions such as (4), although not completely uncontroversial, are the sorts of conditions that

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2 I suppose there might be a case where H is consistent with much of the propositions of T but not consistent with other parts of accepted theory. Or, perhaps H is not consistent with an essential proposition of T. In the second case, it would seem as if a scientist that introduced that sort of hypothesis is either mistakenly doing so or is radically changing the theory itself – moving to a new theory. Thus, the second case would not be problematic because the move would not be ad hoc but, rather, either wrong or a radical departure. In the first case, as long as the hypothesis were consistent with the essential propositions of T, it seems plausible that its introduction could be a candidate for ad hocness.
pretty much all theories of ad hocness share. If the hypothesis weren’t at all consistent with the essential propositions of T, it would seem silly or false to introduce this hypothesis into a theory that you wish to maintain: it would not, however, be the sort of act that could be labeled ‘ad hoc’. (1) is another non-contested condition. If there were no disconfirming evidence, there would be no need for a hypothesis introduction.\(^3\) If the hypothesis didn’t then fix the problem, the hypothesis would have even more serious issues than being ad hoc. The other criteria, however, are more controversial.\(^4\)

II. The 3\(^{rd}\) and 5\(^{th}\) Criteria

In the 3\(^{rd}\) criterion, Leplin claims that a hypothesis is ad hoc only if there is not enough evidence either way to hold that the hypothesis is true or false. (Leplin 1975 321) Once the hypothesis is introduced, it is possible that more evidence will come to light that will either give the scientists reason to believe that H is true, or evidence that will give the scientists reason to believe that H is false. Leplin thinks that this is an important criterion for a couple of reasons. Firstly, it looks like he wishes to distinguish ad hocness from, say, a clearly false hypothesis or a clearly true hypothesis. Leplin thinks that there

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\(^3\) I will use the term ‘introduction’ throughout my discussion of Leplin’s view, as this is the term that he uses. I have stated, in Chapter 1, section XI, why I think that it is the act of accepting a hypothesis, rather than introducing it, that can qualify as ad hoc.

\(^4\) I will not discuss the 2\(^{nd}\) criterion, although it is not as clearly uncontroversial as, say, the 1\(^{st}\). A dogged critic of the criterion of additional test implications would attack 2 (b) for seeming to imply that a requirement for ad hocness is that the hypothesis reconciles E with the theory and that’s it. I discuss possible interpretations of this part of the criterion in section IV, when discussing the Ptolemy case. The criticisms of the interpretations just mentioned are found in Chapter 1, sections VII and VIII.
are ways that the introduction of a hypothesis can be bad besides it being ad hoc: introducing a false hypothesis, for example, would be bad but not ad hoc.\footnote{So, on Leplin’s account, introducing a patently false hypothesis due to disconfirming evidence will not constitute an ad hoc act, as it will not fulfill the 3\textsuperscript{rd} criterion. Introducing a clearly true hypothesis due to disconfirming evidence will also not constitute an ad hoc act for the same reason. The charge of ad hocness can be appropriate only where the scientists do not know whether the hypothesis is true or false. This is an interesting consequence of his account. There might be times when a hypothesis that is known to be false is introduced into a theory as a result of disconfirming evidence where its introduction seems clearly ad hoc or clearly not ad hoc, and similarly with a hypothesis that is known to be true. It is not entirely clear to me why a hypothesis, known to be true, can’t be introduced ad hocly and similarly for a hypothesis known to be false, although I don’t have any strong convictions concerning this point. I discuss the separation between the different virtues and vices (between ad hocness and truth or falsity, e.g.) in Chapter 5, section XIII. I will reserve judgment, here, on this part of criterion 3.} Secondly, Leplin thinks that it is a positive aspect of his view that he can distinguish two types of ad hocness: the ad hocness of a hypothesis and the ad hocness of the introduction of the hypothesis. A hypothesis, on his view, can be ad hoc initially, and then not ad hoc after more evidence has been brought to light that dictates that the hypothesis ought to be held either true or false. (Leplin 1975 321) In other words, the hypothesis will be initially ad hoc because its introduction was ad hoc but shed that label after more confirmation or disconfirmation has arisen for it. The introduction of this hypothesis, however, will always be labeled as ad hoc, even if/when the hypothesis itself is no longer considered to be ad hoc. I will take issues with this criterion when I critique Leplin’s view.

Skipping Leplin’s 4\textsuperscript{th} criterion, I will discuss his 5\textsuperscript{th} criterion in more detail. This criterion deals with the concepts of completeness and fundamentality, which Leplin wishes to differentiate. He thinks that a theory could be both incomplete and non-fundamental: however, he wants to claim that these two concepts do not always go hand in hand. A theory may manifest both types of problems. However, any given problem
will be a problem either of completeness *or* of fundamentality. So, what does Leplin mean by these terms?

Firstly, I will discuss Leplin’s concept of incompleteness. He claims that a problem P is a “‘problem of completeness’ for [theory] $T$ if it shows that $T$ is incomplete.” (Leplin 1975 325) A theory that is complete is one that adequately addresses the phenomena in a given domain: $T$ will be incomplete if there is a problem that arises for $T$, which will be some phenomenon in the relevant domain either not addressed by $T$ or seemingly in conflict with some part of $T$. This problem will be one that can be resolved by adding some other hypothesis to $T$, whereby the hypothesis addresses the additional or previously discrepant phenomenon. The addition of this hypothesis then causes the theory to be complete. An important characteristic of a problem of completeness is that such a problem will not compromise the integrity of $T$: its solution will be consistent with, at least, the ‘essential propositions’ of $T$, where the essential propositions are those that, if modified or discarded, would cause the scientific community to consider the thus-modified $T$ a new, different theory. (Leplin 1975 325)

A problem of non-fundamentality, on the other hand, is one where a problem arises, the solution of which is inconsistent with either all of or parts of a given theory. (Leplin 1975 325) Unlike a problem of completeness, the solution to a problem of fundamentality requires abandoning either all or part of the essential propositions of the

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6 See, e.g., Leplin’s discussion of the Lorentz theory. Leplin 1975 321-323.

7 Leplin thinks that completeness is domain-sensitive: a theory about something in geology, for example, ought not be considered somehow deficient for failing to address something that occurs in, for example, animal husbandry. (Leplin 1975 325) This seems to be a reasonable stipulation. Otherwise, very few – if any – theories would be complete.
theory faced with the problem. This is in contrast to a problem of completeness, whose solution will not require discarding any of the essential propositions.

Although Leplin wishes to distinguish these two types of problems, it is not the case that all problems are definitively considered problems of completeness or problems of fundamentality. The division of problems into one category or the other will be subjective: whether some (if not all) problems are those of completeness or of fundamentality will depend on how the given scientist views the problem. Leplin does not explicitly state the reasons that a problem ends up getting categorized one way rather than another: however, it seems that whether the given scientist will see the problem as that of completeness or of fundamentality will depend on the scientist’s degree of belief in the theory under attack. If the scientist’s degree of belief is fairly high, it is likely that the problem will be characterized as one of incompleteness. If it is lower, it is likely that the problem will be characterized as one of fundamentality. These correlations come about because to treat a problem as one of completeness, the scientist will look for a solution to the inconsistency that is consistent with the (essential propositions of the) theory under attack – not something that a scientist skeptical of a theory would be likely to do – while to treat a problem as one of fundamentality is to come up with a solution for the inconsistency that requires the rejection of all or parts of the theory under attack – not something that a scientist with a high degree of belief in a theory would be likely to do. In other words, a scientist with a high degree of belief in his theory would be much more likely to search for a solution to the problem that allows him to maintain the essential propositions of his theory. A scientist with a low degree of belief in his theory, on the
other hand, will be much more willing to entertain solutions that necessitate discarding some or all of these essential propositions.\(^8\)

It might seem that, nevertheless, there will be definite cases of completeness problems and definite cases of fundamentality problems, where the degree of belief of the scientists involved would not dictate what sort of problems they are. After all, one might argue, there are certainly going to be cases where the solutions clearly will be logically inconsistent with the theory under attack, while other cases’ solutions will necessitate only a minor addition. However, I do not think that all cases will be as clear-cut as those and Leplin seems to agree with me. He notes that “the judgment that a theory is logically inconsistent is generally problematic.” (Leplin 1975 326) What he seems to be intending, here, is that, faced with a charge of logical inconsistency in a given theory, a supporter of this theory can always claim that the theory merely looks inconsistent because the theory is not yet complete. Once the theory gets expanded more, the claim goes, it will be demonstrated that what looked like a logical inconsistency is, in fact, not. One of the examples given by Leplin is that of Stokes’s theory of aberration, which evaded charges of logical inconsistency by having additional hypotheses added to it and thereby making its account of ether coherent. (Leplin 1975 326) Other examples of this sort of phenomena are early 20\(^{th}\) century theories of light, which seemed to claim that light was

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\(^8\) As a result, it seems likely that problems for less-well-established theories will be much more likely to be treated as problems of non-fundamentality, while problems for well-established theories will be much more likely to be treated as problems of completeness. This is not altogether felicitous, as I will discuss in my last objection in this chapter. In the previous chapter, I criticized Strevens because creating a ‘glorious rescue’ or a ‘desperate rescue’ depended almost exclusively on the high or low prior probability of the theory facing the problem. A problem being one of completeness or of non-fundamentality would seem to depend on something similar.
at once composed of particles and of waves: these two claims seem logically inconsistent but the addition of certain hypotheses reconcile them.

Similarly, there do not seem to be many definitive cases of problems of completeness. (This point is one upon which Leplin cannot agree with me, as will be indicated in the next paragraph.) It might well end up that a seeming problem of completeness cannot actually be solved without casting aside some essential proposition of the original theory – or, at least, so it seems at that stage of scientific inquiry. And the difference between problems of completeness and problems of fundamentality does not seem to be how large the problem is: there could well be large problems that arise for a particular theory – for example, quantum mechanics’s seeming incompatibility with relativity – that are treated (at least initially) as problems of completeness and vice versa for problems of fundamentality. The fact that labeling a problem one of completeness or of fundamentality seems to rest on scientists’ degrees of belief in a particular theory under attack is a problem for Leplin, which I will discuss in greater detail in section XI.

Leplin wants to distinguish problems of completeness from problems of fundamentality because he thinks that a criterion for being an ad hoc hypothesis is that it is introduced because the author of the hypothesis believes she is trying to solve a problem of completeness, while the problem is really one of fundamentality. Thus, the introduced hypothesis is inadequate as a solution to the problem. (Leplin 1975 326, 329-332) So, it is important to Leplin that we can distinguish between problems of completeness and problems of fundamentality: otherwise, he could not make this distinction a part of the criteria for being an ad hoc hypothesis. That it is part of the criteria for being ad hoc is explicit. 5(a) states that the fact that there is some problem for
which scientists are trying to find a solution, together with other problems faced by the
theory, “indicate[s] that \( T \) is non-fundamental”. (Leplin 1975 337) Leplin thinks that the
existence of other, related, problems seems to indicate that the theory is non-fundamental
because these problems are related to each other and with the specific problem that the
scientist is addressing at the moment. It might seem, in this sort of case, that a solution
for the problem currently being addressed will be inadequate if it does not also address
the related problems.

This idea, I take it, is what motivates 5(c): that the solution of the problem the
scientists are trying to resolve, if adequate, will have to also help solve the other (related)
problems that the theory is facing. It is not immediately clear what Leplin means by
‘helps to solve’: it could mean either that the specific solution for the problem that the
scientist is currently working on will be used as an actual part of the solution to the other
problems, or it could mean that the solution of the current problem will help inspire
solutions for the other problems with a similar solution structure. From the example that
Leplin uses to illustrate 5(c), it looks like he means the latter. In this example, Leplin
describes how Einstein used the analogy of thermodynamics and its use of statistical
averages when dealing with gases in order to solve a similar problem in radiation. (Leplin
1975 332) Clearly, the problem in radiation could not be solved by the exact same
solution that solved the problem for gases. However, the solution for gases in
thermodynamics inspired the solution for the problem in radiation. The solutions are
similarly structured and, thus, analogous in a certain sense. I will discuss a problem for
this part of the criteria later, using the Ptolemy example. In order to be as charitable as
possible to Leplin, I will outline the consequences for both interpretations of 5(c) in that
discussion.

5(b), too, supposes that the problem facing the scientists and some other set of
problems also facing the theory must be related, since it claims that part of being an ad
hoc hypothesis is an attempt to solve a problem that, along with other (related) problems
the theory is facing, cannot be solved without changing the theory somehow. An
adequate solution for the problem - one that solved this problem and helped solve the
related problems - would be inconsistent with some or all of the essential propositions of
the theory. (Leplin 1975 337)

In his 1975 paper, Leplin claims that the five conditions discussed above are
independently necessary and jointly sufficient for labeling a hypothesis (or a hypothesis
introduction) ‘ad hoc’. (Leplin 1975 332-333) Scientists that introduce ad hoc hypotheses
are being too restrained in their treatment of the problem with the given theory: they
think it is merely a problem of completeness when it is really a more fundamental issue.
In so doing, they are placing a band-aid on a gut shot. Leplin says just this, albeit with a
different emphasis, in “The Assessment of Auxiliary Hypotheses”:

The key claim is then that \( H \) [a given hypothesis] is \textit{ad hoc} relative
to \( T \) and \( e \) [a given theory and a given piece of contraindicating evidence]
only if there are problems for \( T \) other than \( e \) which there is reason to
require that a solution to \( e \) solve or help to solve as well. The judgment
that \( H \) is \textit{ad hoc} is the judgment that these further problems require major
modification of \( T \), whereas the point of the modification effected by \( H \) is
to preserve \( T \) essentially intact. An \textit{ad hoc} \( H \) is simply too conservative a
response to a problem that ought to occasion far reaching reevaluation of
\( T \)’s principal assumptions. Rather than solve the problem posed by \( e \), \( H \)
evades the real issue which \( e \)’s connection with other outstanding
problems raises. (Leplin 1982 237)
So, when scientists don’t go far enough in solving certain problems, the hypotheses introduced to solve the problems are ad hoc.

**III. The Puzzle of What ‘Satisfactory Solution’ Means in Leplin’s 5th Criterion**

There is a question as to how to read Leplin’s 5th criterion. This question arises in the interpretation of the phrase, ‘satisfactory solution’. According to Leplin, one of the criteria that a hypothesis must fulfill in order for it to be considered ad hoc is that it must address only one problem in a set of problems that the scientist has ‘good reason to believe’ are connected, while a *satisfactory* hypothesis would introduce a solution to one of these connected problems which would help to solve the other connected problems.

Consider three different readings of this phrase, ‘satisfactory solution’. One way to read it would be ‘true solution’. What would this reading signify? It looks like, if we read the phrase to mean ‘true solution’, then a charge of ad hocness will never be appropriate. This is because the hypothesis being tested for ad hocness, which must solve only one problem in order to fulfill the first part of the 5th criterion, is being contrasted with the satisfactory solution that contributes to more than one solution. If ‘satisfactory solution’ signifies ‘true solution’, any true solution will contribute to more than one solution. The hypothesis being tested for ad hocness will not do so, as per the first part of the fifth criterion. So, (we are justified in believing) the hypothesis being tested for ad hocness is false. In fact, reading ‘satisfactory solution’ in this manner implies that all ad hoc hypotheses will be false. And Leplin very much wishes to distinguish false hypotheses from ad hoc ones: he considers these two different vices.
This reading seems even worse upon returning to the 3rd criterion. The 3rd criterion states that we cannot have sufficient grounds for holding the hypothesis being tested as either true or false. And, given that the hypothesis being tested fulfills the 5th criterion, as per our discussion, the hypothesis being tested will be false if we read ‘satisfactory solution’ as ‘true solution’. Of course, that the hypothesis fills the 5th criterion and, therefore, is false, does not mean that we know that the 5th criterion has been satisfied. However, in cases where we have sufficient grounds to know that the 5th criterion is satisfied, we will have sufficient grounds for holding the hypothesis as false. Therefore, on this reading of ‘satisfactory solution’, no hypothesis that we have sufficient grounds to know that it fulfills the 5th criterion will fulfill the 3rd criterion. So, we won’t have sufficient grounds for holding any hypothesis to be ad hoc. Additionally, Leplin does not use the phrase in the context of truth or justification.

The second way that we could read ‘satisfactory solution’ is to read it as meaning ‘methodologically satisfactory’. Then, any methodologically satisfactory solution would contribute to the solutions of the other, related, problems. For a solution to be methodologically adequate, it must be constructed appropriately. So, for example, a methodologically adequate solution to the trajectory of the orbit of a given planet might be one where the correct mathematical computations were applied to the data compiled by multiple observations of the location of this planet in the sky. A solution being methodologically adequate is, of course, a separate issue from a solution being epistemologically adequate. Being adequate according to the former reading of the phrase ‘satisfactory solution’ does not necessitate a solution’s being adequate according to the latter reading.
Leplin’s 5th criterion, using the methodologically satisfactory solution reading, in section IX.

The third reading of ‘satisfactory solution’ is to read it as meaning ‘epistemologically satisfactory’. Thus, any epistemologically satisfactory solution would contribute to the solutions of the other, related, problems. For a solution to be epistemically satisfactory, the scientist must be justified in believing the solution. When he is justified in believing the solution, he is justified in believing it is true. At first glance, it might seem as if this reading of ‘satisfactory solution’ will have the same problem as the first reading. After all, both of these readings involve some discussion of what is true. However, at second glance, this reading does not have the same problem as the first reading. This is because the first reading is equating ‘satisfactory solution’ with ‘true solution’, while this reading is equating ‘satisfactory solution’ with ‘the solution we are justified in believing (to be true)’. So, on this reading, for any satisfactory solution, the given scientist will be justified in believing it is true. And any such solution will contribute to the solutions of the other, related, problems. And the hypothesis being tested, in contrast, applies to only one problem and does not contribute to the solutions of the other problems. Therefore, the scientist is not justified in believing that the hypothesis being tested for ad hocness is true. However, the fact that the scientist is not justified in believing that the hypothesis is true is not the same as the scientist being justified in believing that the hypothesis is false. There is not the same dichotomy as in the first reading, where the tested hypothesis must be false because it is being contrasted with something that must be true. So, the hypothesis being tested could, on this third
reading of ‘satisfactory solution’, fulfill both the 3rd and 5th criteria, unlike the first reading. I will respond to this reading of ‘satisfactory solution’ in section X.

IV. A Further Puzzle Concerning What Constitutes as Contributing to a Solution

There seems to be two different ways in which we might claim that some hypothesis contributes to a solution to other, related problems. According to the first reading, to contribute to a solution to other problems, the solution laid out by the hypothesis in question must inspire a solution for the other problems, enabling them to be solved by the construction of a solution similar in structure to the solution of the initial problem. For example, my solution to the problem of creating a line drawing from a set of particular dots – connecting them with straight lines - would enable me to create a line drawing from other sets of particular dots. The solutions to these problems are similar in structure, even though the particular lines and the order they are connected might not be the same.

The second reading is more stringent. According to this reading, the solution laid out by the hypothesis in question must actually appear in the solutions to other, related problems. Suppose my solution to some problem is to postulate the existence of an eccentric, as in Ptolemaic theory. In order for this solution to contribute to a solution of other related problems, some solution of these problems must incorporate an eccentric in addition to whatever other modifications need to be made in order to solve these problems.
There is another way in which we could, but ought not, interpret ‘contribute to a solution’. This would be to claim that the given solution must either just be the solution for the other, related, problems or that it will be found, in its entirety and with exactly the same parameters, in the solutions to the other problems. The last part of this is different than the second, acceptable, interpretation because the second interpretation just requires that, e.g., an eccentric is part of the solution for the other, related, problems, while this interpretation would require the specific eccentric found in the solution to the sun’s anomaly to be a part of the solutions to the other problems. This interpretation is unacceptable because it is clear from his Einstein example that Leplin cannot intend what this interpretation requires. As Leplin states,

[T]hese problems [inconsistencies in the Lorenz view] led Einstein to despair of a constructive approach to radiation and to pursue instead the analogy suggested by thermodynamics of the entropy of radiation with that of a gas. The functional form of the expression for the entropy decrement of a gas corresponding to a contraction of its container results from the assumption that the gas is an aggregate of independent, identical elements – elements that can be treated statistically. This led Einstein to consider an analogous structure for radiation. [italics mine] (Leplin 1975 332)

The problem for which a solution had been proposed was how to calculate the entropy decrement of a gas corresponding to a contraction of its container. The solution was to treat the gas as an aggregate of elements that could be treated statistically. Einstein then uses this solution to help solve the problem of getting an adequate theory of radiation. Notice that he considered an analogous structure. Einstein certainly did not use the exact same structure that was used for the gas entropy problem. And Leplin presents both of these solutions as satisfactory solutions: that is, these are problems where this is a good reason to believe they are connected and either of these solutions would contribute to the
solution of the other. Therefore, the final way of reading ‘contribute to a solution’ cannot be what Leplin intends.

Now for the application of Leplin’s account to Ptolemy. As outlined, Ptolemy’s eccentric hypothesis will be ad hoc according to Leplin if and only if it fulfills the 5 criteria outlined above. I will now proceed to determine whether Ptolemy’s theory does, in fact, fulfill these conditions in the case of the orbit of the Sun.

V. Leplin’s Account of Ad Hocness as Applied to the Ptolemy Example

Ptolemy’s astronomical theory, as previously stated, consists in 8 main theoretical commitments and auxiliary hypotheses: 1) the movement of the heavens is spherical; 2) the heavens themselves are spherical; 3) heavenly objects are spherical; 4) the Earth is spherical; 5) the Earth is the center of the heavens; 6) the fixed stars are so far away from the Earth that the Earth is like a point to them; 7) the Earth does not, itself, move in any way; 8) there are 2 motions to the heavens. (Ptolemy 7) Leplin’s first criterion for ad hocness is that a hypothesis must be introduced to Ptolemy’s theory in response to an experimental result where the experimental result is contraindicated by his theory. When looking at the data compiled from observations of the Sun’s orbit, Ptolemy discovers\(^\text{10}\) that, in fact, it does not seem that the Sun is moving in a perfect circular orbit, concentric to the Earth. (Ptolemy 86) This evidence is anomalous for Ptolemy’s theory, consisting of his 8 original theoretical commitments and auxiliary hypotheses, as his theory would

\(^{10}\) Again, I am not trying to make a historical claim, here. If, in fact, there was some other timeline of discovery, or if Ptolemy already knew full well that there were anomalies of the Sun’s orbit is irrelevant to this discussion. It is enough for my purposes to suppose that it happened in the above manner.
have the Sun describing equal arcs in equal times around the Earth and the evidence is indicating that this does not, in fact, occur. Ptolemy introduces the eccentric hypothesis to solve the problem that arises as a result of this evidence. (Ptolemy 93) With the addition of the eccentric hypothesis to his theory, Ptolemy can now claim that, in fact, the sun really does move on a perfectly circular orbit, describing equal arcs in equal times. It is just that it appears otherwise to us because the movement, while perfectly circular, is around a perfect circle whose center is not located at the Earth. Thus, it appears that Leplin’s first criterion of ad hocness is satisfied.

Now, for the second criterion. Since the added hypothesis fulfilled the 1st criterion, we know that the evidence will be support for the hypothesis. Leplin claims, in addition, that the new evidence needs to be the only support that the hypothesis has; that the hypothesis “has no application to the domain of T [the theory] apart from E [the evidence]”; that nothing else – prior theoretical commitments, e.g. – would provide independent theoretical support of the hypothesis. (Leplin 1975 337) Does Ptolemy’s theory fulfill the 2nd criterion?

This will depend on how we are to understand the 2nd part of this criterion. It seems to say that the introduced hypothesis will not do anything other than solve the one particular problem that the hypothesis is supposed to fix for the theory.11 If this is the case, then the addition of the eccentric hypothesis to Ptolemy’s theory will not fulfill this part of the criterion: the eccentric hypothesis will have an application within Ptolemy’s

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11 If, in fact, this is what Leplin means by this part of the criterion, there will be an additional problem for his theory. It would seem that almost no hypothesis would fulfill this criterion because almost all hypotheses will have some sort of additional implications for the theory to which they are introduced.
theory besides making unproblematic the anomaly of the sun’s orbit. That is, the hypothesis will impact future problems for the theory, in that other eccentric hypotheses will be postulated, later.\textsuperscript{12} There are other indications that Leplin does not mean quite this, but just that the hypothesis cannot fix any problem that is \textit{currently} a problem for the theory. One such indication is that “contributions to previous accomplishments are precluded as well, although preclusion of applicability to outstanding problems is the main point of this provision.” (Leplin 1975 318) From this quote, Leplin’s emphasis seems to be that an ad hoc hypothesis cannot be helpful in solving any past or current problems.

If Leplin means just that (1) the hypothesis does not contribute to solving current outstanding problems and (2) there doesn’t seem to be much in the way of additional testable contributions to the theory, then it looks like Ptolemy’s eccentric hypothesis passes the 2\textsuperscript{nd} criterion for ad hocness. Certainly, there doesn’t seem to be any \textit{still viable} theories whose commitments are such that they constitute a good reason to accept the eccentric hypothesis. Aristotle’s astronomy\textsuperscript{13} might lend support to the eccentric hypothesis, inasmuch as its commitment to the heavenly bodies moving along spheres would seem to support the requirement that planets travel in perfect circular orbits. However, Aristotle’s astronomy, if accepted, does not seem to do much more than bolster some of Ptolemy’s original theoretical commitments. Additionally, the theory lending

\textsuperscript{12} I take it that this issue is one that is related to the issue of what Leplin means by a hypothesis contributing to the solution of other, related problems in his 5\textsuperscript{th} criterion.

support to the hypothesis, according to this criterion, must be a still viable theory. Aristotle’s theory was not still viable at the time of Ptolemy.

Now that Ptolemy’s eccentric hypothesis has passed Leplin’s first two criteria for qualifying as an ad hoc hypothesis, let us turn to his third condition of tentativeness. According to this criterion, there ought to be no sufficient evidence that the hypothesis is either true or false. Ptolemy’s eccentric hypothesis fulfills this qualification easily. Ptolemy himself spends a great amount of time demonstrating, via mathematical proofs, that the eccentric hypothesis and the epicycle hypothesis are empirically equivalent, given certain constraints. (Ptolemy 87, e.g.) Given that there is another, empirically equivalent hypothesis that would also explain why the sun’s anomaly is not really a problem for Ptolemy, there does not seem to be sufficient grounds for holding that the eccentric hypothesis is true. Alternatively, given Ptolemy’s own commitments to the perfect circularity of planetary motion and the geocentricity of the heavenly sphere, as well as the fact that these eccentrics are empirically adequate for explaining the sun’s anomaly, there does not seem to be sufficient grounds for holding the eccentric hypothesis to be false.

Next, I will determine if Ptolemy’s eccentric hypothesis is consistent both with accepted theory and with the essential propositions of T. (Leplin 1975 327) For the first part of this qualification, I will stipulate that the eccentric hypothesis is consistent with the theories accepted at the time. Certainly, the thought that the sun has a perfectly

\[\text{That the eccentrics are not supposed to be visible, tangible entities makes it so that the supposed non-observation of the eccentrics not the sort of observation that would give us reason to believe the hypothesis to be false. Their non-observation would not give us reason to believe the hypothesis to be true, either: the observation of the heavens given eccentrics would be no different than the observation of the heavens given no eccentrics.}\]
circular orbit is acceptable according to previous astronomical theories – Plato and Aristotle’s, for example. The main issue would be whether the fact that the perfectly circular orbit is not concentric with the Earth would be inconsistent with accepted theory. While, again, I will resort to stipulation in claiming that perfectly circular orbits centered other than at the Earth are consistent with accepted theory, there seems to be some backing for this stipulation. The notions of perfectly circular orbits and perfectly regular motions of the heavenly bodies were fairly ingrained in astronomy at this time. Heavenly bodies were thought to have to travel in perfect circles because of their uniform constitution. Their uniform constitution, however, does not necessitate their traveling in these perfect circles concentric to the Earth. In other words, it seems more important that the heavenly bodies be moving in perfectly circular orbits – equal times for equal distances – than having their orbits have the Earth as their centers.

So, the eccentric hypothesis is consistent with accepted theory. Now we must find out if it is consistent with the essential propositions of Ptolemaic theory. To do this, it is necessary to be reminded as to what constitutes the essential propositions of a given theory. Leplin thinks that the scientific community determines which properties are the essential ones of a theory. The essential ones are those properties, the abandoning of which would cause the scientific community to claim that the given theory has also been abandoned – even if other parts of the theory are maintained. We can’t know for sure what the scientific community at the time of Ptolemy would determine are the essential properties of Ptolemaic theory. However, what seems to be considered the essential properties of Ptolemaic theory now are its geocentricity and its requirement that heavenly bodies describe equal distances in equal times around circular orbits. If these are the
essential properties of Ptolemaic theory, it looks like the eccentric hypothesis is consistent with them. And it seems quite likely that these are essential properties of Ptolemaic theory. If he were to reject either geocentricity or the perfect circular movement, the theory seems to become another type of theory altogether.\(^{15}\) Rejecting the requirement that the orbits have the Earth as their center does not impact these requirements.

Finally, Ptolemy’s eccentric hypothesis needs to fulfill the conditions found in the 5\(^{th}\) criterion, in order for it to be considered ad hoc. So, there need to be other problems that ‘there is good reason to believe’ are connected to the problem that the contraindicating evidence raises and that are inconsistent with some essential proposition of Ptolemaic theory. Additionally, these other problems cannot be solved\(^{16}\) by the hypothesis, even though a satisfactory solution to one of them ought to help in getting a satisfactory solution to all of them. (Leplin 1975 331)

Concerning the first qualification, it is clear that there are other problems that seem connected to the anomaly of the sun’s orbit. Those problems would be the anomaly of the moon’s orbit and the anomaly of the orbits of the other planets. And, as such, these problems and the sun’s anomaly indicate that there is something within the essential propositions of Ptolemy’s theory that is inconsistent with the evidence. Ptolemy addresses the problem of the Sun’s anomaly by eliminating a non-essential proposition: that the heavenly bodies have perfectly circular orbits *concentric to the Earth*. 5(a) and

\(^{15}\) That is to say, if Ptolemy rejects geocentricity, his theory would appear to transform into something like that of Copernicus. If he were to reject that heavenly bodies describe equal distances in equal times around circular orbits, his theory would appear to transform into some type of Kepler-like astronomical theory.

\(^{16}\) Or, the hypothesis cannot aid in the solution of these other problems.
5(b) indicate that this was the wrong proposition to eliminate. Ptolemy was being too conservative in his solution to the sun’s anomaly. Rather, in order to solve all of the related problems, Ptolemy would have to take a more radical approach and eliminate an essential proposition of his theory - likely, the proposition that the Earth is the center of the heavenly sphere and/or that the Earth does not move in the heavenly sphere. Thus, it appears that the Ptolemy example fulfills 5(a) and 5(b).

The problem arises in fulfilling condition 5(c). 5(c) requires that the solution of the current problem (in this case, the anomaly of the sun’s orbit) will contribute to the solutions to the other problems (the other anomalous orbits). Now, Leplin claims in the 2nd criterion that the hypothesis in question will have no other application to the theory than in solving the problem that arises with the given contraindicating evidence. If the hypothesis in question assisted in solving other problems, it would not fulfill the 2nd criterion. So, it seems clear Leplin thinks that a hypothesis that fulfills the 2nd criterion will not be a satisfactory solution to the problem it is supposed to solve. Thus, here, Leplin is contrasting an ad hoc hypothesis to a satisfactory one that would contribute to the solution of other, similar problems. So, an ad hoc hypothesis cannot make such contributions. Yet, in the eccentric hypothesis case, this hypothesis does contribute to the solutions of the other problems – either way we construe what it means to ‘contribute’ to a solution. This is because the other orbital anomalies can be solved, at least in part, by postulating some kind or kinds of eccentrics.

Suppose we claim that to contribute to a solution is to inspire a solution for the other problems, enabling the others to be solved by the construction of a solution structurally similar to the solution of the initial problem. Ptolemy postulates the
existence of an eccentric for the moon and for other planets in order to help solve the anomalies present in their orbits. It is true that not all of the planets’, nor the moon’s, anomalies are completely solved by the postulation of a specific eccentric but a complete solution is not necessary, according to Leplin’s condition. It is enough that the solution structure of the eccentric hypothesis for the anomaly of the sun can be used in these other cases to create a similar (partial) solution structure. And the eccentric hypothesis does this. Thus, the eccentric hypothesis fails the condition set out in 5(c), according to one interpretation of 5(c).

According to the other possible interpretation of 5(c), the eccentric hypothesis also fails the condition set out in it. The second, more stringent, interpretation requires that the satisfactory solution to any of these related problems must actually appear in the solutions to the other related problems. As it is harder, on this interpretation, for a solution to contribute to a solution for the other problems, it ought to be easier, on this interpretation, for a hypothesis to fail to contribute and, therefore, fulfill this part of the 5th criterion for ad hocness. Postulating an eccentric orbit solves the solution to the anomaly of the Sun. Looking at the solutions to the other anomalies, it is clear that, as part of their solutions, each contains a postulation of an eccentric orbit. Thus, even on the more stringent interpretation of 5(c), the eccentric hypothesis fails the last requirement for an ad hoc hypothesis.

Leplin’s criteria are supposed to be individually necessary and jointly sufficient. (Leplin 1975 332-333) The eccentric hypothesis does not fulfill all of the criteria. Therefore, according to Leplin’s criteria for ad hocness, the eccentric hypothesis is not an ad hoc hypothesis. This, I claim, is a conclusion that is quite counterintuitive and tells
against Leplin’s theory of ad hocness. Ptolemy’s way of dealing with the anomalies of
the heavenly bodies is seen almost universally as bad and bad because it is ad hoc in
some way. Leplin will have to explain why we should think that Ptolemy’s account is
not ad hoc, despite what most people believe about it.

VI. Leplin’s Account as Applied to the Philosopher’s Ptolemy Case

Some might argue that the case of the sun’s anomaly is not the sort of case to
which they would point when identifying a canonical case of ad hocness. In response to
these critics, I will also apply Leplin’s account to the philosopher’s Ptolemy case, which
is arguable a more canonical case of ad hocness. Leplin’s treatment of the philosopher’s
Ptolemy case\(^{17}\) will be very similar in layout to the sun’s anomaly case, as the
philosopher’s Ptolemy case just specifies that the manner in which scientists solved
problem of the sun’s anomaly will be repeated for other anomalies. Therefore, each
iteration of solutions will fulfill the first four criteria, in the same manner as the solution
to the sun’s anomaly did. And each iteration will have the same problem with the 5\(^{th}\)
criterion as did the sun’s anomaly: in fact, the problem is even more apparent in this
version of Ptolemy because, as several related problems were solved similarly, it would
seem more likely that the current solution of the particular anomaly that is being
examined will contribute to a solution to other related anomalies – in either sense of the

\(^{17}\) The summary of which is in Chapter 1, section X.
phrase ‘contributes to a solution’. Applying Leplin’s account, then, to the philosopher’s Ptolemy case will result in the same counter-intuitive outcome as the sun’s anomaly case.

Ending up with a counter-intuitive outcome, although serious, is not a devastating problem for a theory. Thus, my critique of Leplin’s theory does not end with the observation that it produces a very counter-intuitive outcome both Ptolemaic cases. Rather, I have additional fuel to add to the fire, in the form of 5 objections to Leplin’s account.

A note is in order before I turn to detailing my criticisms of Leplin’s theory. I want to emphasize again the sorts of things that Leplin’s theory gets right. Firstly, his discussion of the criterion of direct testability for acceptable solutions to problems addresses the Quine-Duhem-Ayer problem without falling afoul of it. Additionally, Leplin rightly discusses the problem of ad hocness as a methodological, rather than an epistemic, problem. The trend in the literature has been to treat the problem as an epistemic one: Strevens’s account is an example of this. This seems to be both misguided

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18 A proponent of, e.g., the repeated modifications account of ad hocness might object to my treatment of the philosopher’s Ptolemy case. She might say that it is the repetition that makes this case a canonical example of ad hocness, and this repetition is how it importantly differs from the sun’s anomaly case. However, Leplin’s account of ad hocness is insensitive to these repetitions: it must evaluate each repetition individually, not as a set. So, this repeated modifications proponent might have a reason to object to Leplin’s account and, therefore, Leplin’s account’s treatment of the philosopher’s Ptolemy case. But, she does not thereby have a reason to object to my assessment of Leplin’s account as applied to this case. My reasons for rejecting the repeated modifications account of ad hocness are found in Chapter 1, section X.

19 This is in contrast to views such as the independent testability view, which Leplin rightly dismisses as irrelevant. This is also in contrast to views such as that of Strevens’s Bayesianism, which Fitelson and Waterman claim does not solve the Quine-Duhem-Ayer problem, contra to what Strevens asserts. And Strevens himself notes that Howson and Urbach’s Bayesianism fails to address this problem. Therefore, it is a positive that Leplin’s theory does get around the Quine-Duhem-Ayer problem. My account will get around this problem as well.
and problematic, as epistemic accounts will either fall into the Quine-Duhem-Ayer problem or fail to address it altogether. My account is, therefore, like Leplin’s in that it is a methodological account of ad hocness.

Finally, I think that Leplin’s account is, so far, the account of ad hocness that has come closest to the heart of the matter. This being said, there are still points where I have strong disagreements with his account. Thus, the next subject to be addressed is my criticism of Leplin’s account.

VII. The Failure to Account for What Makes Ad Hocness a Vice

Leplin’s account fails to account for what is really problematic with ad hoc hypothesis introductions or acceptances. The very fact that there were two empirically equivalent hypotheses that Ptolemy chose between in the sun’s anomaly case seems to be the motivation for calling the adoption of the eccentric hypothesis ‘ad hoc’ and, therefore, not rationally acceptable.²⁰ Yet, according to Leplin, the introduction of the eccentric hypothesis is not ad hoc.

The fact that Leplin’s view does not consider the eccentric hypothesis ad hoc is not enough to claim that Leplin thinks, contra my view, that no methodological error has occurred in the acceptance of the eccentric hypothesis: the eccentric hypothesis might have other vices and, so, ought not be accepted. In this manner, Leplin might agree that the addition of the eccentric hypothesis was not a satisfactory solution to the problem of the sun’s anomalous orbit, but would maintain that my critique is not effective because

²⁰ This is precisely why I claim that this case is a case where an ad hoc hypothesis acceptance has occurred, in chapter 5.
there are many other ways in which the addition of a hypothesis might be unsatisfactory other than it being ad hoc. Thus, we would not be in disagreement that the addition of the eccentric hypothesis is unacceptable. We would just be in disagreement as to how it is unacceptable.

I argue that, in fact, Leplin and I do disagree as to whether the addition of the eccentric hypothesis is rationally acceptable or not. In order to demonstrate this, I must introduce an additional criterion for the addition of hypotheses introduced by Leplin in his 1982 paper. This criterion – direct testability – determines whether or not the addition of a certain hypothesis in the face of recalcitrant evidence, ought to be considered a real solution to the problem that had arisen for the theory due to this evidence. Leplin discusses the criteria for ad hocness in conjunction with the criterion of direct testability, in part, to address the traditional view of ad hocness that characterizes a hypothesis as ad hoc if it is not independently testable.

According to Leplin, the hypothesis that there is a trans-Uranian planet, introduced in order to explain the perturbations of Uranus, and the hypothesis that there is a intra-Mercurial planet, (Leplin 1982 236) introduced in order to explain the precession problems of Mercury, were both considered fairly poor hypotheses because no such planets had been observed and these planets ought to be observable in principle. (Leplin 1982 243) This point is one that the received view latches onto. Hypotheses are ad hoc, according to this view, if what the hypotheses postulate is observable in practice but has not yet been observed.21 (Leplin 1982 243) Both the postulated trans-Uranian planet and

21 As Leplin clearly points out, this version of the received view of ad hocness is a modification of Karl Popper’s version of ad hocness, where hypotheses are ad hoc if they are unfalsifiable – that is, if they are not testable, even in principle. The reason why the
the postulated intra-Mercurial planet were observable in practice and had not yet been observed when the hypotheses were put forth as solutions to problems in Newtonian gravitation. Bode’s law, a law that predicted the distance of planets from each other and had predicted that a trans-Uranian planet would be too far away to see at the time that the trans-Uranian hypothesis was being discussed, had been discredited and, so, there was no reason why a trans-Uranian planet ought not be observed. Solar intervention – that is, the planet being permanently occluded by the sun, was ruled out in the case of the intra-Mercurial planet, so this planet, too, ought to be observable. (Leplin 1982 243) Leplin agrees in part with this assessment but thinks that the criterion is misleading, as it does not mirror what actually counts in scientific practice when assessing auxiliary hypotheses. As Leplin points out, the fact that we haven’t detected free quarks is not a liability to a hypothesis that postulates the existence of quarks because quarks aren’t ever going to be free. (Leplin 1982 243) Thus, Leplin modifies the received view, changing the criterion from ‘observable in practice’ to ‘directly testable’. As he says:

The general principle appears to be that so long as some better form of evidence than that which supports an hypothesis is theoretically possible, the hypothesis is not credited with solving an outstanding problem, although it may be worthy of investigation in its own right. Conversely, if it can be shown that the available evidence is of the best sort possible, the hypothesis may come to constitute a solution despite the fact that this evidence is of a sort that would otherwise be insufficient. It is this principle which distinguishes direct from indirect testability. A test may be considered direct if no better test is theoretically possible, regardless of how indirect it may appear epistemologically. (Leplin 1982 244)

move was made to a stronger version of testability is, as he points out, because almost any hypothesis can be made testable in principle. No(or almost no) hypothesis or theory can be tested in isolation – we always test a hypothesis/theory in conjunction with its auxiliaries – so the ‘tweaking’ of auxiliaries will make almost any hypothesis/theory testable in principle. (Leplin 1982 243)
Thus, the fact that both the trans-Uranian planet and the intra-Mercurial planet were, in principle, observable and neither had been observed, the hypotheses supporting their existence fail the direct testability criterion. Notice that neither of these hypotheses is considered ad hoc, according to Leplin. In this manner, Leplin’s direct testability criterion separates from the received view because his criterion is pointing out another way in which a hypothesis can fail to be a (good) solution to a certain problem facing a theory besides it being ad hoc.

So, the distinction that Leplin wishes to emphasize is the distinction between direct and indirect tests for the existence of phenomena. He is clearly using ‘directness’ differently than it is commonly used. While a direct test, in common terms, would be something like placing one’s hand on an apple and looking at an apple to make sure that there is an apple on the table, Leplin’s use of ‘direct testability’ is such that, if there were no better test available, examining the radiation emitted from the apple might qualify as a direct test of the apple’s being on the table.22 On Leplin’s account, for example, the Michelson-Morley experiment was considered, at certain points, a direct test of the Lorentz contraction hypothesis because it was thought that there was no other way to test contraction other than by way of the interferometer. (Leplin 1982 245) So, in order to be considered a (good) solution to a problem, it looks like the hypothesis must be directly testable in the way in which Leplin uses the phrase.

The discussion of direct testability, while not speaking directly to the subject of ad hocness, is important for my reply to the argument that Leplin and I agree on the

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22 I wish to stress ‘if no better test were available’ because, in correspondence, Leplin has said that he wishes ‘theoretically possible’ to mean possible within the current theories and commitments of the time and not to mean ever possible. Thus, to the Greeks, it was not theoretically possible to fly to Mars. Now, it is theoretically possible to fly there.
unacceptability of the addition of the eccentric hypothesis. If the eccentric hypothesis passes the direct testability criterion it ought to be considered a good solution to the problem of the sun’s anomaly facing Ptolemaic theory, according to Leplin. If it is a good solution, then there will not be anything else wrong with the addition of this hypothesis to Ptolemaic theory. Therefore, I can claim that Leplin and I come to two different conclusions in this case: not only does Leplin’s view not show that the addition of the eccentric hypothesis is bad because that move is ad hoc, it also can’t claim that the hypothesis was bad for other reasons. What is left, then, is to determine whether or not the eccentric hypothesis passes the direct testability criterion.

To do this, it needs to be determined whether there was some better form of evidence theoretically possible in the case of the eccentric hypothesis. If there is no other, better, test possible, then the eccentric hypothesis will be directly testable and, therefore, a good solution to the problem facing Ptolemaic theory. Did Ptolemy have any other way of determining how the sun’s orbit was anomalous except through observations made from the Earth? In Ptolemy’s time, it was not theoretically possible to travel into space to observe the sun’s movements. And, the eccentric itself is not supposed to be observable. Therefore, it does not look as if there is any better form of evidence possible. According to Leplin, then, the eccentric hypothesis must be a good solution to the problem of the sun’s anomaly. Therefore, according to Leplin, there isn’t anything else wrong with the addition of this hypothesis to Ptolemaic theory. And the claim that Leplin and I do not differ on whether the addition of the eccentric hypothesis is unacceptable cannot hold: this claim relies on there being something else wrong with the eccentric hypothesis besides it being ad hoc. On Leplin’s view, the eccentric hypothesis is not
considered ad hoc and it fulfills the direct testability criterion. Therefore, not only is the hypothesis not ad hoc, it is also to be considered a solution to the problem of the anomaly of the sun’s orbit. Yet, if a solution at all, it does not seem rationally permissible to consider this solution of the anomaly as a *good* solution. This solution fails to give a reason why it is that the addition of the eccentric hypothesis is a solution to the problem facing Ptolemaic theory and the addition of the epicyclic hypothesis is not. At the very least, the addition of the eccentric hypothesis ought to be demonstrated a *better* solution than the addition of the epicyclic for the addition of the eccentric to be considered a good solution. Leplin’s account of ad hocness does not give us this demonstration, yet, on Leplin’s account, the addition of the eccentric hypothesis ought to be considered a good solution to the problem facing Ptolemaic theory.

**VIII. Leplin’s Third Criterion is Misleading**

Leplin’s 3rd criterion is, at best, misleading. The 3rd criterion states that, to be ad hoc, a hypothesis must have no compelling evidence for or against it being held true. Leplin considers this criterion an especial good-making characteristic of his theory because it gives us two different conceptions of ad hocness, depending on how we

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23 The eccentric hypothesis is being supported by the observed anomaly of the sun’s orbit. At the time, there would have been no other way possible to support this hypothesis: certainly, there were no telescopes nor were there ways to go to the sun, for example. Nothing of this sort was theoretically achievable. As Leplin states, “the relevant principle is that an auxiliary hypothesis enables a theory to solve an outstanding problem only if no better form of evidence than that currently supporting the hypothesis is theoretically achievable.” (Leplin 1982 245)
approach this criterion, and that the conception that he advocates is very specific on what is deemed as ad hoc. As Leplin says:

On this formulation an *ad hoc* hypothesis need not remain so. Alternatively, we could say that an *ad hoc* hypothesis is one for which there are no such grounds when it is introduced. Both options have support in normal usage. It is said that the contraction hypothesis ‘was [introduced] *ad hoc*’ and also that it ‘is [an example of] an *ad hoc* hypothesis’. An advantage of my choice is specificity. [Brackets Leplin’s] (Leplin 1975 321)

Thus, Leplin wishes to claim that a hypothesis can be ad hoc but, later, become non-ad hoc. I believe that this is a misleading way of talking about ad hocness. It is misleading because the hypothesis is not being examined in the same context when it is introduced and when there is additional evidence for holding that it is true. When it is introduced, it is part of the periphery of the theory. It is not part of the core of the theory. If it were ad hoc, it ought not to have been accepted into the theory. Suppose it did get accepted. Then, as one’s theoretical commitments develop and more evidence comes to light, this hypothesis becomes more established and, potentially, becomes part of the core of the theory. At that time, the hypothesis is no longer the type of thing to be considered ad hoc because it is established and its role in the theory is no longer in question. It is thus misleading to claim that the hypothesis is no longer ad hoc after it becomes established within the theory because a well-established hypothesis is not the sort of thing that can be ad hoc or not ad hoc. It is only when we are considering whether to accept the hypothesis or not can we deem this acceptance as ad hoc or not ad hoc. And it remains a fact that the hypothesis being considered ought not to have been accepted when it was, even though this hypothesis’s acceptance would not have been considered ad hoc if the
hypothesis had been accepted into the theory at the later time, when there was sufficient grounds for holding that the hypothesis is true.

That is, the account of ad hocness is a methodological one. What it is supposed to label is the introduction/acceptance of hypotheses given specific empirical problems. So, we don’t need to examine a hypothesis for ad hocness after it has been introduced/accepted into the theory, unless we are examining this hypothesis’s previous introduction or acceptance, in order to determine whether that move was rationally permissible or not. Leplin’s 3rd criterion confuses the issue by allowing for a hypothesis to be ad hoc and then, later, be deemed not ad hoc. Part of the way in which the 3rd criterion confuses the issue and is misleading lies in the equivocation in what qualifies to be labeled ‘ad hoc’ between a hypothesis and as the introduction of that hypothesis.

IX. Scientists Can’t Get Advice from the 5th Criterion

As the 5th criterion is an empirical one, it seems that it might not always be possible to see the connection between problems for which the ad hoc hypothesis does not contribute to the solutions. Yet, Leplin believes that his 5 criteria are a methodological account of ad hocness. Therefore, they ought to be advice-giving, telling scientists how to proceed in certain situations. But, if scientists cannot tell how certain problems are connected, this account will not give useable advice on how to proceed.

24 See my discussion of what qualifies to be ad hoc in chapter 1, section XI for my discussion of why that which qualifies ought not to be a hypothesis introduction itself but, rather, hypothesis acceptance.
Leplin could intend his criteria for ad hocness to be doing one of two things: either these criteria are meant to be retrospective, evaluating some event that has already occurred, or these criteria are meant to be advice-giving, telling scientists how to proceed in certain situations. As Leplin has stated that his account is a methodological one, it seems more natural to interpret his criteria as being intended to do the latter. Therefore, Leplin’s account must give useable advice on how scientists should proceed if these scientists wish to maintain a given theory in the face of recalcitrant evidence. And Leplin’s account will not do so because of the externalist, empirical nature of the 5th criterion.

This objection speaks directly to the introduction and the 3rd part of the 5th criterion. The introduction makes, as part of the criteria for ad hocness, the condition that “there are problems other than E confronting T which there is good reason to believe are connected with E in the following respects”. (Leplin 1975 331) The third part of this criterion makes it a condition of ad hocness that “a satisfactory solution to any of these interconnected problems will contribute to the solution of the others.” (Leplin 1975 331) These conditions consist in the empirical claims that there are connections between problems facing a theory and that the ad hoc hypothesis, unlike a satisfactory solution to one of these problems, addresses only one problem and doesn’t address or aid in the solution of the others.

It might seem, from a certain reading of these conditions, that Leplin just wants to include connections between problems that are (fairly) easy to see. After all, Leplin does state that there ought to be a good reason to believe that the problems are connected in
certain specific ways. Yet, Leplin cannot mean this by his criteria of connectedness because of what he says in the following excerpt:

> It is to be emphasized, however, that the respects in which, according to this condition, the problems confronting T are connected do not require that the connection be readily apparent. \( H \) may be charged with ‘ad hocness’ despite an apparent unrelatedness of the problems confronting \( T \) or of \( E \) with other problems thought to be related independently. A charge of ‘ad hocness’ may reflect the belief that such unrelatedness is superficial, masking important relationships at a deeper level. The charge might then be controversial, its vindication requiring that further research bear out the alleged connection. In this way the concept of ‘ad hocness’ can be used to direct attention to neglected or unnoticed relationships, thus opening new lines of research. (Leplin 1975 331)

From these sentences, it is clear that Leplin doesn’t just think that the relatedness of the problems ought to be fairly apparent. In fact, he states explicitly that these problems, instead of seeming related, might actually seem completely distinct from each other. The key, here, is that Leplin seems to be claiming that the criterion is talking about problems that are actually related: notice, at the end of this quote, that he mentions empirical research to solved the question of whether certain problems are related or not. Thus, this relatedness is an empirical distinction and there is a fact of the matter as to whether or not certain problems are related. If the problems are actually related in the relevant ways and, yet, these connections are not available to the scientist modifying a given theory in the fact of disconfirming evidence, then Leplin’s criterion cannot be giving useful advice to this scientist.

Scientists can only use the advice if they can tell how the problems are connected. And, in order to determine how and whether certain problems are actually connected, the scientists must know whether satisfactory solutions will help solve these other problems. But, the scientists will not know what the acceptable solutions are, yet. So, Leplin’s 5th
criterion does not seem to give scientists any useable advice on how to proceed if they wish to maintain a given theory in the face of disconfirming evidence.

The example that Leplin gives for actually related hypotheses – Einstein seeing problems in radiation as related to problems in the thermodynamics of gases - is a good example of a time where Leplin’s criterion would seem especially unhelpful in determining whether or not certain problems ought to be solved in similar manners. (Leplin 1975 331-332) Einstein didn’t think that any successful theory of radiation was possible given the accepted views, which included the Lorentz theory. Seeing a connection between how the problem of determining the reduction in entropy of a gas given the contraction of its container and the problem of the entropy of radiation, Einstein used a structure analogous to the structure used in solving the gas problem for his solution of the radiation problem. In both solutions, the thing whose entropy is being measured is considered “an aggregate of independent, identical elements” (Leplin 1975 332) which can then be treated in a statistical manner. Thus, light came to be understood, according to Einstein, as quanta, which helps to show that the Lorentz theory is non-fundamental, according to Leplin. (Leplin 1987 332) How in the world were scientists to recognize the similarities between a problem in the thermodynamics of gases – where they were dealing with swarms of molecules – and a problem in the theory of radiation – where, at the time, light was considered a type of wave, where there are no swarms of molecules and, in fact, there are no massed bodies at all? The scientists examining these two problems would have had to have already known that acceptable solutions to one of these problems would have to help solve the other. Yet, they certainly didn’t know that the acceptable solutions were, as evidenced by the fact that it took Einstein to realize that
these problems were connected, and the way in which they were connected. This example, among others, ought to convince the reader that Leplin’s 5th criterion causes Leplin’s account of ad hocness to fail to give usable advice for those scientists that are attempting to avoid ad hoc moves.

Of course, this objection only applies to certain of the cases where scientists are trying to solve a problem for their theories. There might well be other cases where scientists can tell that the problems are connected. This does not mean, however, that Leplin’s view is adequate for these cases. The last two objections will speak to other reasons why Leplin’s criteria for ad hocness are inadequate and, ultimately, undesirable.

Notice, as well, that I have outlined the two ways of reading ‘acceptable solution’ in 5(c) of Leplin’s criteria in section III of this chapter. The current objection is treating ‘acceptable’ as a methodological claim and responds to this reading of the term. The other way to read ‘acceptable’ is to read it as an epistemological claim. The next objection will respond to this way of reading the phrase.

X. Why Determining when Problems Ought to Be Solved Together Appeals to the Degrees of Belief of Scientists

In the last critique, I outlined the sort of relationship between certain problems facing a theory that Leplin intends. The relationship is one that, in principle, the scientists ought to see because it is an actual, empirical relationship. However, in practice, how are scientists going to determine whether or not there is an actual relationship between certain problems? It looks like whether or not a hypothesis is deemed ‘ad hoc’ will be, in part, a result of the degrees of belief of the scientists in the
scientific community at the time. Leplin even suggests something like this when he says that: “[a] charge of ‘ad hocness’ may reflect the belief that such unrelatedness [the seeming unrelatedness of certain problems] is superficial, masking important relationships at a deeper level.” (Leplin 1975 331) So, given that scientists might not, in reality, always know about the existence of an actual relatedness between certain problems, the introduction or acceptance of a hypothesis being labeled ‘ad hoc’ and, therefore, vicious will be determined by whether or not scientists believe that there is such an actual relationship. This consequence is bad for Leplin’s theory because the psychology of the scientists should not determine whether or not the introduction or acceptance of a hypothesis is ad hoc. Allowing the degrees of belief of scientists to come into the determination will reduce claims of ad hocness to claims about how confident scientists are about the theories whose problems these hypotheses are supposed to fix. As Leplin himself points out (Leplin 1975 332), Einstein’s connecting the method of determining entropy decrease in gases with entropy decreases in radiation was not something that most other scientists would have even considered - because the other scientists considered these phenomena successfully incorporated into theories that were in pretty good standing and in which scientists had a pretty high degree of belief. Yet, these same scientists might be much more willing to allow a charge of non fundamentality, and to claim that certain problems really are connected, even when they don’t initially appear to be, if, for example, the theory to which the hypothesis is being introduced is quite new and, thus, there are not high degrees of belief about it. The negative aspects of relying on degrees of belief of individual scientists have been

25 I have discussed this problem in a slightly different context in Chapter 1, section VI.
discussed previously, in my discussion of Streven’s view in chapter 2, section III and also in my discussion of Hempel’s view in chapter 1, section VI.  

**XI. Completeness Versus Fundamentality and the Degrees of Belief of Scientists**

This objection is closely related to the last one, in that it deals with the degrees of belief of scientists. Determining whether or not a problem for a theory is one of completeness or one of fundamentality is a matter of the degrees of belief of the scientists working with this theory. As I noted previously, there do not seem to be many (if any) clear-cut cases of either completeness or fundamentality. When a problem is considered to be one of completeness, its solution is attempted without violating any essential propositions of the theory, while for a problem considered to be one of fundamentality, its solution is attempted by replacing essential propositions of the theory, where the solution is seen as incompatible with some or all of the theory. Yet, as pointed out previously, demonstrating that a hypothesis is inconsistent, combined with other, auxiliary hypotheses, with some or all of a given theory is very difficult: a proponent of the theory can always claim that the hypothesis only seems inconsistent with the theory because the theory is not yet complete. Once the theory is complete, the proponents claim, there will no longer be this seeming inconsistency. Analogously, it is hard to demonstrate that a theory is merely incomplete instead of non-fundamental. Thus, considering a problem for a theory a problem of completeness or a problem of

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26 I also talk about this problem in regards to Leplin’s account of ad hocness in footnote 8 of this chapter.
fundamentality will be a result of the problem–solver’s degree of belief in the theory for which the problem arises.\(^{27}\) Treating it as a problem of fundamentality would be a result of having a high degree of belief in the theory. An example of this, as discussed above, would be scientists’ degrees of belief in the prevailing electromagnetic and gas theories around the time of Einstein: these were quite high and, so, the scientists considered the problem of quantifying the entropy of radiation as one of completeness. Einstein, on the other hand, did not have as high a degree of belief in these theories\(^{28}\) and, so, treated the problem as one of fundamentality. This is a problem, as stated in the previous critique and also in my critique of Strevens’s account, because the degrees of belief of the truth or empirical adequacy\(^{29}\) of a theory will depend, in part, on how established the theory facing the problem is. So, problems facing theories that are relatively new and for which scientists don’t have high degrees of belief will likely be treated as problems of fundamentality while problems facing theories that are well-established and for which scientists do have high degrees of belief will likely be treated as problems of completeness. This manner of determining which of the two sorts of problems a theory might have seems quite problematic, as evidenced by the Newtonian theory example

\(^{27}\) This is also related to the discussion in footnote 8 of this chapter.

\(^{28}\) Leplin does not talk about degrees of belief. However, he does note that Einstein did not think that the accepted theories could adequately explain radiation. (Leplin 1975 332) Since Einstein didn’t think that these theories solved a problem that was within their purview to solve, it would seem that Einstein likely had severe misgivings as to whether these theories were true or empirically adequate.

\(^{29}\) Neither truth nor empirical adequacy is doing any real work, here. Insert your favorite term here and the argument still holds.
brought up by Strevens.\textsuperscript{30} (Strevens 535) In this example, it would be unlikely for the hypothesis of a trans-Uranium planet to be considered ad hoc (or at least, not ad hoc in a bad way on Strevens’s view) because Newtonian theory was well established and there were no other viable competing theories at the time. Thus, there was a very high degree of belief in Newtonian theory. This example is a problem, not because the trans-Uranium planet hypothesis should be considered ad hoc (I myself don’t think it is and my methodological account of ad hocness determines that it is not)\textsuperscript{31}, but, instead, because the evaluation is based almost solely on the fact that Newtonian theory was well established and didn’t really have any competitors. This seems wrong because it looks like a well-established theory still has quite a few opportunities to ad hocly introduce or accept a hypothesis and shouldn’t be exempt from the censure that this introduction or acceptance ought to produce just because it is well established. Alternatively, a new theory ought to be able to introduce/accept a hypothesis without it (almost) automatically being seen as potentially ad hoc, because the degree of belief in the theory is lower than that of a well-established theory. Plus, appealing to the psychology of the scientists doesn’t fit well with the claim that ad hocness is a methodological – and not an epistemological – problem. My discussion of Hempel’s account of ad hocness in chapter 1, section VII, discusses the methodological versus epistemological distinction in more detail.

\textsuperscript{30} Strevens, himself, did not think that the Newtonian example was problematic. However, in critiquing Strevens’s view, I demonstrated how the Newtonian example was problematic for a view that included scientists’ degrees of belief.

\textsuperscript{31} See chapter 5, section V for my discussion of this.
Thus, for the reasons outlined above, Leplin’s theory of ad hocness ultimately fails. I will turn now to Sober’s account\footnote{I am calling it Sober’s account for sake of brevity. In the two papers that I will be discussing, Sober teams up with two different people – Malcolm Forster and Christopher Hitchcock – to discuss the view that I will be critiquing.} of ad hocness.
Chapter Four
Sober, Forster and Hitchcock’s Failure to Properly Account for Ad Hocness

I. Simplicity and Curve-Fitting

In this chapter, I will present the account of ad hocness developed by Forster and Sober in “How to Tell When Simpler, More Unified, or Less Ad Hoc Theories Will Provide More Accurate Predictions” and Hitchcock and Sober in “Prediction Versus Accommodation and the Risk of Overfitting”. Their account is a part of a larger project that attempts to give criteria for determining how to trade off between demands of simplicity and demands of precision.

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1 As Sober co-authored both of the papers upon which I base my analysis of this account of ad hocness, I will often use just Sober’s name to identify the account for ease of discussion.

2 I am calling it an account of ad hocness rather loosely, as it is a bit misleading to label it as such. In the Hitchcock and Sober paper, they claim that their theory concerning the accommodation of new data and new information eliminates the need for ad hocness to exist as a distinct criticism. Thus, I will be concentrating on Sober and Forster’s paper, which actually discusses ad hocness. There is nothing in the Hitchcock and Sober paper that contradicts or changes dramatically the phenomenon called ‘ad hocness’ in the Forster and Sober paper.
Sober’s curve fitting theory is based, in part, on work done by H. Akaike. Sober uses Akaike’s work to come up with a methodological rule for determining what theories have more predictive power than other theories. (Forster Sober 6-7) By predictive power, Sober means the expected predictive accuracy of a particular theory. (Forster Sober 10) The amount of predictive power a given theory has has been important for quite a while in philosophical discussions of the qualifications of specific theories, and especially in discussions of ad hocness. The phrase has been used to identify instances where the scientist introducing a given hypothesis is doing so just because his current theory, as it stands, does not fit with some new piece of evidence. These ad hocly introduced hypotheses will have very little, if any, predictive power because they were introduced to patch up one specific problem for a given theory. Therefore, the theory with the ad hoc modification will not be as virtuous because it will be less predictively powerful, or so this argument goes.

In addition to the lack of predictive power of hypotheses introduced ad hocly, there has always been a tension observed between how closely a theory fits the data and


4 For a more detailed discussion of the received view, see the discussion of Hempel and Popper in chapter 1, Sections V - IX.

5 Sober does not use the term ‘theory’ the same way throughout either paper. In addition, he often uses theory and hypothesis interchangeably. I will try to copy his terminology,
the simplicity of the theory. Theories that fit the data excellently have often been accused
of being too complicated, while very simple theories often do not fit the data very well.
(Forster Sober 5, 6, e.g.) There is also the worry that closeness in fit to the data is not the
same as closeness in fit to the truth. Sober’s way of addressing the tension between fit
and simplicity is to look at how closeness-of-fit to the data generated works with
simplicity in order to give us an estimate of a theory’s predictive power, using Akaike’s
statistical modeling method.

Akaike’s theorem deals with families of curves and which families fit given data
sets better. The families of curves represent models. A model is a family of hypotheses
that are all represented mathematically with the same basic equation form – parabolic, for
example – with different co-efficients. (Sober, Forster 12, fn. 20) Any model will consist
in a family of curves because models – mathematical formulas - have adjustable
parameters that can be manipulated to give us different versions of the same equation.6
(Sober, Forster 3) The phrase, ‘adjustable parameters’, in this context, refers to
coefficients in a mathematical formula, where the coefficients can be given different
values. In the case of Ptolemaic astronomy, for example, these coefficients would
represent the values for the angular speed of the sun, the size of the radius of the sun’s

where possible. When I get into more detail with Sober’s account, I will use his term
‘model’, which is much more precise. This term will be defined in the next paragraph.
6 It will often seem, in my discussion of Sober’s view, that it is possible to substitute
‘theory’ for ‘model’. The reason that I am going to speak only of models, unless Sober
specifically says ‘theory’, is both because of a footnote found in the Sober and Forster
paper and because of the lack of consistency mentioned in footnote 5.

In places, such as during the discussion of ad hocness, Sober and Forster seem to
interchange discussion of models with discussion of theories – implying that these two
terms denote the same (or at least similar) concept. Yet, in their footnote 20, Sober and
Forster define what it is to be a model and what it is to be a hypothesis and then make
pains to distinguish models and theories – without defining what the term ‘theory’
denotes.
orbit, etc. By adjusting the coefficients’ values, we can generate curves that fit the data that was actually collected in a given circumstance more or less closely.

Why not just choose the values of the coefficients that result in the curve that fits the data perfectly? We oughtn’t do this because the data collected will not all be accurate. There will be errors and ‘background noise’ – unrepresentative data – included in this data set. If a particular curve fits the data perfectly, the curve will be unlikely to give accurate predictions: it will have factored in all of the errors and noise in the data in making those predictions. (Sober, Forster 5-6) On the other hand, as Sober and Forster point out, a model that is maximally simple likely won’t fit the data well enough. Thus, we ought not use a curve that fits the data too well or too poorly as our model: we need to find the one that fits the data set just right. The family of curves whose best-fitting member fits just right will be the model closest to the true model for the theory. (Sober, Forster 6-7) This curve will best balance closeness-of-fit to the data set and simplicity, according to Sober.

It might seem as if Sober is needlessly adding a step to the argument by starting with families of curves. After all, he eventually wants to match best-fitting individual curves with their actual counterparts. Sober’s starting point is necessary, however. There are major problems that arise when trying to determine the simplicity of individual curves.⁷ (Sober, Forster fn. 18) However, by looking at a family of curves and determining the number of adjustable parameters that family of curves has, we avoid this problem. Sober and Forster never refer to the simplicity of an individual curve. They

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⁷ Sober and Forster refer to Graham Priest’s outline of the problems facing individual curves and evaluation of their simplicity in “Gruesome Simplicity”, especially section V. Op. cit.
always speak of the simplicity of a family of curves, and then the fit of some particular curve within the family.

The two criteria, then, in which Sober and Forster are interested are these:

(a) Larger families generally contain curves closer to the truth than smaller families
(b) Overfitting: the higher the number of adjustable parameters, the more prone the family is to fit to noise in the data

(Sober, Forster 8)

The phrase ‘larger families’, here, refers to ones with more curves found within them: the family of parabolas, e.g., is larger than the family of lines because the family of lines is a subset of the family of parabolas. The reason why larger families generally contain curves closer to the truth is because the larger families have more parameters and, so, more resources that enable them to reduce their average distance from the data. To reduce their average distance from the data is just to reduce their distance from the actual model. (Sober, Forster 8) But, this closeness of fit by itself is not enough, as the closer the fit, the more likely it is that noise in the data will bias the fit: that is, there is a possibility for the curve to fit the data too closely and, therefore, to be not as close to the actual curve as a curve that does not fit the data quite as closely. Simplicity must also be considered. The combination of closeness of fit and simplicity, Sober claims, will provide an unbiased determination as to which family of curves will be closest to the true curve. If this were true, then this view would determine which family of curves would have the most predictive success. As was pointed out earlier, predictive success is a good indication that the particular model that gives us this success is close to the true model. And, a model without predictive success or little predictive success in comparison with
its complexity will likely be overfitting. I will argue, later, that models that overfit will be considered ‘ad hoc’, on Sober’s view.

In order to get a precise, epistemically accessible\(^8\) calculation of closeness-of-fit considerations versus simplicity considerations, Sober and Forster use a modified version of Akaike’s theorem. Here is the modified version:

\[
\text{Estimated}[A(\text{family F})] = \frac{1}{N} [\log\text{-likelihood}(L(F)) – k] \quad \text{(Sober, Forster 10)}
\]

Here, A(family F) stands for the Akaike score of a family of curves. N stands for the number of data points. The k stands for the number of adjustable parameters the family has. The log-likelihood is being used to measure the distance to the data from the family of curves. It is the log of the likelihood that this data set would have occurred, given the best-fitting member of the family of curves being considered.\(^9\) The smaller the log-likelihood, the greater the distance to the data is and vice versa. (Sober, Forster 10) This formulation, Sober and Forster claim, allows the error variance to be treated as one of the parameters and, so, the error variance no longer needs to be known to make the calculation – unlike with the unmodified version of Akaike’s theorem.

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\(^8\) It is this modified version, along with two assumptions upon which Akaike’s theorem relies, that makes the goodness-of-fit versus simplicity calculation epistemically accessible. The two assumptions to which I am referring are the following: (1) certain “mathematically formulated conditions that ensure the ‘asymptotic normality’ of the likelihood function” (Sober, Forster 29); (2) there is enough data so that these asymptotic normalities will be reached. (Sober, Forster 29) This last assumption is fairly self-explanatory. The first assumption consists (roughly) in mathematical parameters such that the error distributions will be normally distributed – that is, that there won’t be some extreme shift to the data such that the error distributions vary greatly to one side or the other of the mean value. (Sober, Forster 30)

\(^9\) See, for example, www.xycoon.com/lsrloglikelihood.htm
This theorem allows for the consideration of both the curve fit (with the log-likelihood) and simplicity (with the k-value). The higher the log-likelihood, the higher the k-value likely will be and vice versa. The goal is, in comparing two or more families of curves, to pick the one with the highest overall Akaike score: that is, the highest number after the number of parameters is subtracted from the log likelihood and multiplied by the inverse of the number of data points. The one with the highest Akaike score will be the hypothesis with the greatest expected predictive accuracy. Thus, the Akaike theorem is a statistical measure of how curves fit the data. The score the best-fitting curve from the family of best-fitting curves gets is an unbiased estimate of predictive accuracy.

As the reader can see from looking at the structure of the theorem, a more complicated family of curves – one with more parameters – will have to add a significant amount of goodness-of-fit in order to be favored over a simpler family of curves. The log-likelihood, being a log function, is not going to go up as quickly as the straightforward addition of the number of parameters. Thus, if a simpler family of curves is equivalent – or close to equivalent – in goodness-of-fit to a more complex family of curves, the simpler family will be preferred over the more complex family. (Sober, Forster 11)

Another element of the theorem that Sober and Forster highlight is that the importance of either simplicity or goodness-of-fit changes, depending on the number of data points involved. (Sober, Forster 11) They claim that, in the case of a large amount of data points, goodness-of-fit is much more important than simplicity (number of
parameters). On the other hand, with small amounts of data points, the simplicity will become much more important.10

II. Ad Hocness and Its Correlation with Closeness-of-Fit

Sober and Forster are mainly concerned with how to determine accuracy of predictions and how to choose a model that will be the most accurate in predicting new data: ad hocness is a more peripheral topic. They speak, among other things, about unified theories and why they are preferable to non-unified theories and of causal modeling. I will not address these issues, here, as they are not within the scope of my project. Instead, I will omit these very interesting discussions and skip directly to Sober and Forster’s discussion of ad hocness.

10 Although they are not explicit as to why the number of data points makes a difference in the importance of goodness-of-fit or of simplicity, looking at a different formulation of Akaike’s result gives some insight into why this might be. Consider this formulation:

\[ \text{Estimated(Distance from the truth of family } F) = \text{SOS}[L(F)] + 2k\sigma^2 \]

(Sober, Forster 9) where SOS [L(F)] stands for the sum of squares of the best-fitting curve in family F (the sum of squares being the sum of the square of the distance from the true curve for each data point, k stands for the number of parameters found in family F, \(\sigma^2\) stands for the degree of spread of the errors around the true curve. (Sober, Forster 9) (I have, here, taken out an added constant because Sober and Forster state that this constant is common to all families and, so, drops out when comparing families of curves.) Then, as the number of data points increases, the SOS [L(F)] will increase. If there are a large number of data points, the addition of 2k\(\sigma^2\) will not matter much in final calculation. If there are a very small number of data points, however, the addition of 2k\(\sigma^2\) will be a large factor in the final calculation for each family being considered.

Sober and Forster seem to support this sort of explanation in the following:

“Suppose that there is a slight parabolic bend in the data, reflected in the fact that the SOS value of L(PAR) [the best-fitting curve of the family of parabolas] is slightly lower than the SOS value of L(LIN) [the best-fitting curve of the family of lines]. Recall that the absolute value of these quantities depends on the number of data points.” (Sober, Forster 11)
Like me and like Leplin, Sober and Forster reject the Bayesian idea that ad
hocness has something to do with the motives of scientists. They also, like me and like
Leplin, reject the idea that the historical background – what theories came before it, for
example – have something to do with ad hocness. Sober and Forster also think that ad
hocness is a vice. (Sober, Forster 17) However, Sober and Forster seem to disagree both
with me and with Leplin as to what sort of thing can be ad hoc. In their discussion of
curve fitting, they have been talking about models, which are families of curves where a
particular curve represents a particular hypothesis. While Sober and Forster speak only
briefly about ad hocness, an argument can be made that they seem to attribute ad hocness
as a vice of theories or research programs, in the spirit of Lakatos. 11 (Sober, Forster 17) I
will here argue for this interpretation of Sober and Forster.

Sober and Forster begin by rejecting the hypothetico-deductive conception of
science because of the Quine-Duhem-Ayer problem. However, Sober and Forster agree
with the hypothetico-deductive conclusion12 that there is a difference between acceptable
revision – as in the case of Leverrier’s postulation of Neptune because of the
perturbations of Uranus – and, as they put it, “ad hoc evasion”. (Sober, Forster 17) Their
example of this sort of ad hoc evasion is a version of the philosopher’s Ptolemy case. As
Sober and Forster describe Ptolemaic theory:

11 See, for example:

Lakatos, Imre. “Falsificationism and the Methodology of Scientific Research
Programmes.” Criticism and the Growth of Knowledge. Lakatos, Musgrave, eds.

12 This is a fairly uncontroversial conclusion: certainly, I would agree with this and so
would all of the others whose views I have critiqued. Where the controversy arises, of
course, is how the distinction between acceptable revisions and ‘ad hoc evasions’ is
characterized.
The classic example of this [preventing refutation of a theory by making adjustments in the auxiliary hypotheses of the theory] is Ptolemaic astronomy, where the model may always be amended in the face of potential refutation by adding another circle – so much so that the expression ‘adding epicycles to epicycles’ has become synonymous with ‘ad hocness’. (Sober, Forster 17)

So, in the case of Ptolemaic theory, it looks like the theory is ad hoc because the additional complexity – the sum of epicycles and eccentrics and equants added to deal with the seemingly anomalous orbits of the planets and of the moon – does not add enough predictive value or gain in fit with the data\(^{13}\). Thus, the theory will have a low Akaike score because it is gaining a higher degree of fit at the cost of simplicity.

Just because a theory has a low Akaike score because of high complexity cannot mean that this theory is ad hoc. ‘Low’ is a comparative term: something has to be lower than something else. So, it looks like Sober and Forster intend ad hocness to be comparative, although they do not discuss explicitly this comparative aspect. Throughout the paper, Sober and Forster have been discussing how to use Akaike’s theorem to decide between two or more families of curves, in order to decide between two or more models of theories for certain data sets. Similarly, for ad hocness, it looks like a theory has to be ad hoc in relation to another theory: some theory’s Akaike score must be (significantly) worse than some other theory’s Akaike score. So, Ptolemaic theory is ad hoc in comparison, presumably, with Copernican theory. And this comparative aspect of ad hocness seems supported elsewhere in the text. When discussing how more unified theories tend to get better Akaike scores for their models, Sober and Forster speak directly about the comparison between Ptolemaic and Copernican theory:

\(^{13}\) Their claim that the gain in closeness-of-fit, after repeated iterations, is not enough to counteract the loss of simplicity is very similar in tone to what Kuhn says in the quote in footnote 16 of chapter 1.
In Ptolemy’s geocentric astronomy, the relative motion of the earth and the sun is independently replicated within the model for each planet, thereby unnecessarily adding to the number of adjustable parameters in his system. Copernicus’s major innovation was to decompose the apparent motion of the planets into their individual motions around the sun together with a common sun-earth component, thereby reducing the number of adjustable parameters. […] We present the maximization of estimated predictive accuracy as the rationale for accepting the Copernican model over its Ptolemaic rival. For example, if each additional epicycle is characterized by 4 adjustable parameters\(^{14}\), then the likelihood of the best basic Ptolemaic model, with just twelve circles, would have to be \(e^{20}\) (or more than 485 million) times the likelihood of its Copernican counterpart with just seven circles for the evidence to favour the Ptolemaic proposal. Yet, these basic models had about the same degree of fit with the data known at the time. (Sober, Forster 14-15)

In other words, Ptolemaic theory is inferior to Copernican theory because it has a worse Akaike score than Copernican theory. It is not that Ptolemaic theory is a bad theory simpliciter. It has a worse Akaike score than Copernican theory because it overfits the data and, as a result, has many more parameters than Copernican theory. And the Akaike theorem is supposed to tell us when overfit occurs – when the added accuracy does not make up for the added complexity of a theory’s model – because these sorts of models will not be accurate in predicting new data or fitting additional data sets.

So, when Sober and Forster discuss reasons to reject or view with suspicion certain theories in the vocabulary of Lakatos, it appears that they are really letting the reader know what it is for a theory to be considered ad hoc. Lakatos speaks not of theories but of research programs: these constitute the ongoing testing of a specific scientific theory. He also speaks of research programs being degenerative, where degenerative means that the program no longer has predictive power and so no longer

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\(^{14}\) Sober and Forster do not say what these additional parameters are. I think that they are speaking a bit loosely, here, because it is not clear that all epicycles will have 4 adjustable parameters. I will discuss what these 4 adjustable parameters might be in section V.
makes novel predictions. (Sober, Forster 17) Sober and Forster adopt this language when they say:

> Our proposal is that a research programme is *degenerative* just in case loss in simplicity is not compensated by a sufficient gain in fit with data. Of course, the fit will always improve, but the improvement may not be enough to increase the estimated predictive value. (Sober, Forster 17)

While they are using Lakatos’s vocabulary, it seems clear that what Sober and Forster are defining, here, is how a theory is determined to be ad hoc. They label Ptolemaic theory as ad hoc and, when identifying it as such, point out the fact that Ptolemaic theory underwent a great loss in simplicity that did not greatly improve the fit. Here, Sober and Forster are implying that the Ptolemaic theory’s improvement in fit is not enough to increase its overall Akaike score and, thus, its estimated predictive value. And they have stated, previously, that something that overfits the data is unlikely to have a high predictive value because something that overfits will include background noise and experimental errors in so closely fitting the data. So, it looks like there are two ways to be a bad model/theory (to have a low estimated predictive value): one is to be very, very simple and, as such, underfit the data. The other is to overfit the data. The latter case is a case of ad hocness on Sober and Forster’s view, I argue. And, this vice is a comparative one, according to the textual indications of Sober and Forster.

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15 Again, they seem to be echoing the sentiments of Kuhn in the quotes cited in chapter 1, footnote 16.

16 In the 2004 paper, Sober and Hitchcock never identify overfitting as ad hocness but they discuss how models can have low expected predictive values. The two ways in which models can have low expected predictive values, according to Sober and Hitchcock are that the model either is too simple and underfits, or that it is too complex and overfits. This distinction mirrors the distinction that Sober and Forster make in the 1994 paper. It is just that, in the 1994 paper, Sober and Forster seem to make a more explicit connection between ad hocness and overfitting. Thus, it looks like ad hocness just is overfitting and nothing more, on their account.
Sober and Forster, thus, seem to claim that the sort of thing that can be ad hoc is a theory-data set pair. Leplin does not agree with this and neither do I. However, this is not an objection that I am going to pursue contra Sober and Forster because I will demonstrate that there is a way in which Sober and Forster’s view can be reconciled to the idea that it is the introduction or acceptance of a hypothesis that qualifies as possibly ad hoc. I will describe this modification of Sober and Forster’s theory next, when I apply their view to the Ptolemy example discussed previously. I will, however, critique the comparative conception of ad hocness, as I think that this element of Sober and Forster’s view of ad hocness is inescapable for them and also undesirable.

III. How to Evaluate Hypothesis Introductions According to Sober’s Account of Ad Hocness

Sober wants to discuss the predictive accuracy of models/theories. In this way, my interpretation of what he considers ad hocness to be does not bear directly on the previous discussion of ad hocness. Instead of discussing the possibility of theories as a whole being ad hoc – such as the ad hocness of the whole Ptolemaic theory in comparison with the whole Copernican theory – I have been discussing the possibility of a hypothesis introduction or acceptance being ad hoc. However, there is no reason to think that Sober can’t talk about the predictive accuracy of individual hypotheses. Nor does it seem implausible that he could also talk about whether the introduction or acceptance of a certain hypothesis into a theory ought to have happened. It looks like, given recalcitrant evidence, Sober can claim that a hypothesis introduction or acceptance ought to be labeled ad hoc if the family of curves that represent the modified model (the
model with the inclusion of the new hypothesis) do not achieve as high an Akaike score as either the original model or some otherwise modified model that includes a different new hypothesis. Given a set of possible hypotheses, upon adding them individually to the original model, there will always be at least one modified model that achieves the best Akaike score out of the others, although there might be a set of modified models that all achieve equally high Akaike scores. The models not in this set will likely have less predictive power and will have lower total Akaike scores. Thus, the hypothesis that, when combined with the original model, received the highest Akaike score, will be the hypothesis that ought to have been chosen, according to Sober.

**IV. Whether, on Sober’s Account, a Model Ought to Be Modified At All**

It will be another matter as to whether *any* hypothesis ought to be added to a model given recalcitrant evidence. Remember, Sober deems the model with the most predictive value in comparison with other models to be the one that we ought to use. Suppose recalcitrant evidence arises. Now suppose there are three possibilities: (1) keep the model as is, even though its closeness-of-fit will be damaged by the recalcitrant evidence; (2) consider the model in combination with a hypothesis that improves the closeness-of-fit because it takes into account the recalcitrant data but has a higher complexity of, say, 4 or 5 adjustable parameters; (3) consider the model in combination with a hypothesis that improves the closeness-of-fit – although not as well as the second option – but which has fewer added parameters than the second option. It could very well be the case that the option that has the best Akaike score is option (1), where no
hypothesis is introduced. Then, adopting one of the other two options would be ad hoc. However, if the 2nd option’s Akaike score is the highest, that is the option we should adopt, and similarly for the 3rd option. So, there are two different questions that Sober’s theory might answer: (1) out of these hypotheses, which one would give me the highest predictive accuracy when added to my theory? (2) Out of the possible options, should I add another hypothesis to deal with recalcitrant data or should I keep my theory as it was before the recalcitrant data appeared? In other words, we need to compare the proposed revisions, in combination with the particular model we are using, to each other. Then, we need to compare whichever modified model has the highest Akaike score to the unmodified, original model to see which of those has the highest Akaike score. If we don’t choose the one with the highest overall Akaike score, our model is ad hoc.

This version of Sober’s criterion of non-ad hocness that I have just outlined will be the one that I test in order to see how Sober’s predictive accuracy view deals with the specific Ptolemy example that I have been using. As I stated, this version seems in the spirit of Sober and will allow me to compare his conception of ad hocness with the others that have been discussed, as this version puts the criterion of non-ad hocness in terms of the introduction or acceptance of hypotheses instead of theories as a whole.

V. Sober Versus Ptolemy

Ptolemy finds that his theory of the movements of the heavenly bodies does not fit the data very well. This is because of the observed anomalies of the sun’s orbit. So, he decides to add an additional hypothesis that will enable his theory to better fit the data.
According to Sober and Forster, the best-fitting curve in one family of curves will be superior to that of another family of curves, and, thus, will have greater expected predictive accuracy, if the best-fitting curve of the first family scores better according to the Akaike theorem than that of the second. The two considerations that are in tension with each other will be closeness-of-fit and the number of adjustable parameters for each family of curves. In the case of the eccentric and the epicyclic hypotheses, they are empirically equivalent in their closeness-of-fit to the data set of the sun’s positions in the sky. So, the closeness-of-fit consideration will come out the same for the models of both the eccentric and the epicyclic hypotheses. Thus, the important parameter in deciding between the two hypotheses will be the complexity of their respective models.

In the case of the eccentric hypothesis, it looks like the model will have four adjustable parameters. The first two will consist in the distance between the center of the Earth and the center of the ecliptic and the angular coordinate locating this center against the zodiac. The final two parameters will be the radius of the eccentric and the angular speed of the sun. Yet, the model of the epicyclic hypothesis looks to have 4 adjustable parameters as well: the radius of the orbit that is concentric to the earth, the radius of the epicyclic orbit, the angular velocity of the sun, and the angular velocity of the orbit around which the epicycle moves. Therefore, it looks like the model of the epicyclic hypothesis is equal in complexity to the eccentric hypothesis. Since both their closeness-of-fits and their number of parameters look to be equivalent, then the models that contain

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17 In the case of the sun, this orbit must have the same period as the epicycle and, so, is constrained. However, in general, an epicyclic orbit doesn’t need to exhibit the properties of the sun’s orbit. And, in the case of some of the planets, there is an epicyclic orbit that is not constrained in this way. For the sun, the two angular velocities are the same but this is a contingent result of the empirical data and is not required by the structure of the epicyclic hypothesis.
one or the other of these hypotheses ought to get equivalent Akaike scores. Thus, they
should be equally successful in prediction and either model may be adopted as the best-
fitting one if it is determined that these models have higher Akaike scores than the
original, unmodified model (Ptolemaic astronomy in its original form).

Ptolemy himself uses the ideas of simplicity and closeness-of-fit when he decides
to adopt the eccentric hypothesis. He notes that both of these hypotheses are empirically
adequate. Yet, Ptolemy comes to a different conclusion than does Sober. According to
Sober’s calculations, both are equally suitable for adoption. Ptolemy, however, picks the
eccentric hypothesis, claiming that, though both are empirically adequate, the eccentric
hypothesis is simpler. It is possible that Ptolemy means something like metaphysically
simpler, here. But, it’s also possible that he means simplicity in terms of number of
parameters. Sober’s view is silent on the matter of a theory or hypothesis being simpler
metaphysically, as his view deals only with simplicity as evaluated by number of
adjustable parameters.

Applying Sober’s account, we come to the conclusion that both the eccentric and
the epicyclic hypothesis are equally acceptable for adoption. He doesn’t give any
additional reasons for choosing between the two. And it might well be that metaphysical
simplicity is a perfectly valid reason to choose one hypothesis over another. However, if
Ptolemy is saying that the eccentric hypothesis is simpler in terms of the number of its
adjustable parameters, then there is a tension between Ptolemy’s decision and Sober’s
view. On Sober’s view, as we saw previously, both hypotheses are equally simple in
terms of number of parameters and, so, greater simplicity is not a reason to choose one of
these hypotheses over the other.
This tension is not, of course, a definitive reason to condemn Sober’s view. An argument could certainly be made that Ptolemy was concerned with metaphysical simplicity: the displacement of the sun’s orbit from being centered at the earth to being centered elsewhere might have been less metaphysically onerous than the postulation of multiple circles to account for the sun’s movements. And, even if Ptolemy was talking about simplicity in the way that Sober does, Ptolemy could simply be wrong in choosing the eccentric over the epicyclic because of a concern for this sort of simplicity. What Sober’s view fails to capture is the seeming arbitrariness of adopting one hypothesis over the other, instead of merely using one for its convenience but withholding judgment as to whether it ought to be part of the theory as a whole or keeping both hypotheses as live options.18

There is more to consider, though, than the Akaike scores of the two hypotheses in comparison with each other. It is not immediately evident, in the Ptolemaic case, whether or not the theory’s overall Akaike score would improve given the adoption of one of either the eccentric or the epicyclic hypothesis. That is, it might be better to choose the eccentric over the epicyclic, but it might be better19 to add neither, or to add some other hypothesis, to the main theory. However, not much rides on this: we could conceive of a case exactly like the Ptolemy case in the relevant ways, except that the

18 Of course, on other views, simplicity could be an additional virtue in favor of the eccentric hypothesis and, thus, Ptolemy might be justified in accepting it over the epicyclic hypothesis even though they are empirically equivalent. The point, here, is that Sober cannot make this sort of distinction that others might because, for him, simplicity is just the number of parameters and is a part of the calculation for a given model’s Akaike score. Simplicity cannot come in again after the Akaike scores have been calculated.

19 That is, the model with the highest Akaike score might not be one that contains either the eccentric or the epicyclic hypothesis.
theory’s Akaike score definitely did improve with the adoption of one of these two hypotheses. And, certainly, in the Ptolemy case, it looks plausible that its model’s Akaike score will improve, given the addition of one of these hypotheses. With neither of them is there a great deal of additional parameters, in comparison to the number of parameters already found in Ptolemaic theory, and both would make the closeness-of-fit to the data much better. On Sober’s view, then, it seems that Ptolemy’s theory is not ad hoc in this case: its model’s Akaike score improved with the addition of the eccentric hypothesis. So, Ptolemy did nothing wrong in this case by adopting the eccentric hypothesis: the Ptolemaic model’s Akaike score, arguably, would become higher, and higher to the same degree, with the addition of either the eccentric or the epicyclic hypothesis and, so, Ptolemy was permitted to adopt either of the two.

I do think that Sober et al attempted an ingenious solution to the curve-fitting problem. It is just that I don’t think that it works completely, and does not work as a characterization of ad hocness. There are two ways in which I will criticize Sober’s view of ad hocness. I will turn to those criticisms next.

VI. The Problem With Considering Ad Hocness a Comparative Characteristic

On Sober’s view, ad hocness is comparative. This comparative nature is problematic because something can’t be ad hoc in absence of other viable options and because, in the case of multiple options, (at least) one option is always going to be not ad
hoc. This is because, for a hypothesis\textsuperscript{20} to be ad hoc, according to Sober’s view, the model containing it must have a lower Akaike score than the model(s) containing some other hypothesis(es). The model will have a lower Akaike score because it overfits the data and, in doing so, has too many adjustable parameters. This conception of ad hocness, as was noted earlier, is essentially comparative. As Sober and Forster themselves noted, for example, Ptolemaic theory is ad hoc because it has a worse Akaike score than Copernican theory, as a result of its extensive number of adjustable parameters. (Sober, Forster 14, 17) This seems adequate for situations where there are more than one (viable) hypotheses to compare to each other. However, what will happen in situations where there really is only one (viable) hypothesis? It looks like, then, that this hypothesis cannot be ad hoc because the model containing it has the highest Akaike score.

Consider the example of the plenists.\textsuperscript{21} The plenists believed that there could be no vacuums in nature – a stronger claim, even, than those who believed that nature merely abhorred a vacuum. (Hempel 28-29) Along came Torricelli and his mercury bath experiment.\textsuperscript{22} In this experiment, Torricelli filled a tube with one closed end and one

\textsuperscript{20} Sober and Forster talk only about theories being ad hoc, when they explain their view of ad hocness, although they mention ad hoc hypotheses in section 6 (the sub family problem) when they use the term ‘ad hoc’ in a more traditional manner. I have already demonstrated that Sober and Forster’s view of ad hocness can be applied also to hypothesis introduction or acceptance during the discussion of Ptolemaic theory and its solution to the sun’s anomalous orbit. So, in this discussion, I will speak of their view being applied equally to theory-data sets and to hypothesis-data sets although Sober and Forster do not.

\textsuperscript{21} Hempel, among others, discusses the plenist example as one that is definitively ad hoc. See Philosophy of Natural Science. Op cit.

\textsuperscript{22} Hempel also discusses this experiment.
open end full of mercury. Keeping the open end covered, he immersed this tube in a pool of mercury, open side down. He then released the seal on the open end of the tube. There was no way in which air could have gotten into the tube, because the one open end is surrounded by this pool. Yet, the level of the previously full tube of mercury dropped, leaving a seemingly empty space at the closed end of the tube. In the face of this seeming evidence for the existence of a vacuum, the plenists postulated the existence of funiculi: invisible strands, rather spring-like in nature, that connect the mercury to the top of the tube. These strands completely fill up the space between the top of the tube and the mercury and, so, the seeming vacuum is not an actual one. Looking only at the plenists, it does not appear as if they advanced any other hypotheses to account for the disconfirming evidence. Let us suppose, for our purposes, that the plenists did not advance any other viable hypothesis. Now suppose that the plenist theory’s Akaike score went up as a result of adopting this new hypothesis: that is, that the unmodified model’s Akaike score was lower than the score of the model containing the funiculus hypothesis. Then, neither the funiculus hypothesis itself nor the adoption of this hypothesis would be considered ad hoc. They couldn’t be because there are no other alternatives to the funiculus hypothesis to which the model containing this hypothesis can be compared, and the Akaike score of the plenists' theory went up when this hypothesis was adopted. Yet, this sort of hypothesis seems to be exactly the sort of hypothesis that we do not want to accept as legitimate. The plenists took option B when, if they wanted to retain their theory, option C – the ‘wait and see’ option – was the only appropriate course of action.23

23 When a scientist’s theory is faced with recalcitrant evidence, there are always three options that the scientist may take. (A) throw over their theory; (B) hold on to their theory and add a hypothesis that will reconcile it with the evidence; (C) do nothing,
In fact, it is this sort of hypothesis that has traditionally been called ad hoc because it seems to have been formulated solely to save the theory from recalcitrant evidence.\(^\text{24}\) Hempel himself characterizes the adoption of the funiculus hypothesis in these very terms.

A critic might want to object to my last conclusion. I was comparing only the unmodified plenists' theory with the theory modified by the funiculus hypothesis. Maybe I am comparing the wrong models. Perhaps the plenists’ model containing the funiculus hypothesis is considered ad hoc because it has a lower Akaike score than Torricelli’s model that contains the hypothesis that vacuums exist. *This* comparison, the critic might argue, is the appropriate comparison. However, there is something wrong with adding the funiculus hypothesis, regardless of whether we compare it to Torricelli’s theory or not. And we should be able to see that the addition of the funiculus hypothesis is ad hoc without Torricelli’s theory.

Consider a different sort of case: suppose, instead of having no rivals, a certain theory has several rivals. Yet, all of these hypotheses are some sort of variation of the funiculus hypothesis. Either all of the models that contain one of the hypotheses being considered will have the same Akaike score or one (or more) will have a better Akaike score than the others. In the first situation, it looks like it would be acceptable to choose recognizing that there is a tension or even an inconsistency between the evidence and his theory plus the background but deciding to hold off doing anything about it for the time being. I discuss these options in chapter 1, section X.

\(^{24}\) It is true that I am not willing to adopt this formulation of ad hocness because of several major problems that it faces. However, while not adequate for defining ad hocness, it is a pretty good intuitive guide for making determinations of ad hocness. On my view, the model containing the funiculus hypothesis would also be considered ad hoc, even if there were no other (viable) hypotheses to which it could be compared.
any of these hypotheses. In the second situation, it looks like at least one of the models – the one or ones that scored higher than the others – will not be considered to be ad hoc. Yet, neither of these analyses seems right for the reason stated above. All of these hypothesis acceptances ought to be considered ad hoc.

An example of this sort of situation might be imagined when considering the *horror vacui* proponents after Pascal executed his Puy-de-Dome experiments. The *horror vacui* proponents were similar to the plenists in that they believed that nature abhorred a vacuum and, so, vacuums did not exist in nature. (Hempel 28-29) Pascal measured the height of a column of mercury in a sealed tube when this measurement was taken at sea level and when it was taken, amongst other places, at the top of a mountain. He demonstrated that the mercury’s level dropped in relation to the top of the tube when measured at the top of a mountain, creating what appeared to be a vacuum. Pascal also demonstrated that, in bad weather, the height of the mercury dropped in comparison with the height of the mercury in fair weather. In response, a *horror vacui* proponent might come up with several different hypotheses that might explain the actions of the mercury in these different situations. One might be that, while Nature does abhor a vacuum, it abhors it less on the tops of mountains and in bad weather than at sea level and in fair weather. The second hypothesis might be that there is an invisible and weightless

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25 I introduce this example and the Torricelli experiment in chapter 1, section V.

26 This view was famously derided in Pascal’s treatise on air pressure. I have quoted part of Pascal’s rhetorical rant in chapter 1, section V.

27 This hypothesis would be plausible for the *horror vacui* theorists and not for the plenists because, on the former view, nature just abhors a vacuum. That leaves room to say that a vacuum might occasionally exist. The plenists, on the other hand, were committed to the non-existence of vacuums.
ether that can permeate glass. This ether exists in more quantity in bad weather and at the
tops of mountains. The third might be something along the lines of the funiculus
hypothesis adopted by the plenists: there is some sort of invisible and weightless material
that connects the mercury to the top of the tube. This material connects it more closely at
sea level and in good weather than on the tops of mountains and in bad weather. When
added to the *horror vacui* model, any of these hypotheses will raise the model’s Akaike
score because they do not add many (if any) adjustable parameters and they all greatly
improve the closeness-of-fit to the data. Now suppose that the model that includes the
first hypothesis achieves a better Akaike score. Then, the acceptance of this hypothesis
will be rationally permissible, according to Sober. Or, suppose that all three models that
contain one of these hypotheses had equal Akaike scores, all of which are higher than the
original, unmodified model’s score. In that case, any of the three hypotheses would be
acceptable to adopt, according to Sober. And this just does not seem right: all three of
these hypotheses seem like the sort of hypothesis that ought not to be adopted in this
situation.

In short, we want to be able to make judgments of ad hocness on one theory or
hypothesis standing alone. We also want to be able to claim that all of our possible
options are ad hoc. Sober’s view of ad hocness does not permit us to do either of these
things, because of its essentially comparative nature.

*VII. Conclusion*
While there are other objections that could be raised against Sober’s view, I have outlined only the two strongest. Both of these are real problems for Sober. Having discussed and discarded several of the most promising accounts of ad hocness, I will now turn to my own proposal for defining ad hocness, an account which will sidestep the problems that face these other theories and that will also sidestep the Quine-Duhem-Ayer problem that faces the traditional account of ad hocness.
CHAPTER 5
A New Account of Ad Hoc Acts

I. What Can Be Ad Hoc, Revisited

Having discussed previous definitions of ad hocness and the problems inherent to them, I will turn to my positive account of ad hocness. In order to set up this account, I will first indicate the types of acts\(^1\) that qualify as potentially ad hoc.

A scientist’s theory is always set against a background, which consists in the currently accepted scientific theories and assumptions that impact on the theory.\(^2\) This will be true for all theory modifications. However, there must be certain conditions fulfilled in order for the specific modification to be one that can be labeled ‘ad hoc’. Firstly, a theory must face disconfirming evidence. This is not enough, however, as the scientist can act in different ways after disconfirming evidence arises. Additionally, the scientist must decide to accept a new hypothesis into her theory in order to reconcile this theory with the disconfirming evidence.

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\(^1\) I will only discuss ad hocness in terms of ad hoc acts. As I discussed in chapter 1, part XI, the term ‘ad hoc’ is applied only to acts, despite it sometimes appearing to apply to hypotheses or theories themselves.

\(^2\) There is not a sharp line between the background and the theory itself, which is to be expected: the theory’s auxiliaries blend into the background in which the theory is situated.
I discussed, in chapter 1, section XI, the difference between the introduction of a hypothesis and the acceptance of a hypothesis. However, it is worthwhile to emphasize the distinction here, as I argue that scientists who merely introduce a hypothesis because of disconfirming evidence will not be committing an act that qualifies to be judged ad hoc. The introduction of a hypothesis is an act by which that particular limitation or explanation comes to be entertained as a potential addition to a theory. To accept a hypothesis is to take an additional step. A scientist will have to think that the hypothesis is true\(^3\) in order to accept a given hypothesis. Without this additional step, there will be too many acts considered ad hoc that should not even be in the running for ad hocness. For example, we might claim that the introduction of a hypothesis – even one that does not make additional predictions, or corroborating evidence for it, or that is not even suggested by our theory – is perfectly satisfactory. We might introduce a hypothesis because we think that other hypotheses are not fully satisfactory without thinking that what the introduced hypothesis claims is true or empirically useful, or even a good claim. We might just need something to enable us to continue investigating the other consequences of our theory, for example.\(^4\) So, the charge of ad hocness cannot be

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\(^3\) Or, that the hypothesis is well-supported, or something like that.

\(^4\) Here is an example where the hypothesis is being used as a placeholder. Dr. Georgia has a patient, Wendel, whom she thinks has disease R. Wendel then develops a symptom that would seem to contradict Dr. Georgia’s theory that he has R. Dr. Georgia still wants to maintain that Wendel does, indeed, have disease R. Dr. Georgia could reconcile her theory with the disconfirming evidence by supposing some hypothesis B. Suppose Dr. Georgia knows that hypothesis B is false. However, in previous cases where there has been a diagnosis of R and the same type of symptoms arose, it has been helpful to treat the patient as if he had R plus B. Dr. Georgia might well be justified in treating Wendel in this manner, while acknowledging that B cannot really be what is going on with Wendel: she just doesn’t know at this time why Wendel has R plus the symptoms that he does. And, she would be justified in treating Wendel in this manner because she has an
considered for such an act: we are not committing ourselves to anything that ought to be
judged in the way that ad hoc acts are judged. In the case of hypothesis acceptances,
however, we are making a commitment and this commitment can be judged as ad hoc or
not ad hoc.

If none of these conditions is fulfilled, the charge of ad hocness will not be
appropriate for this action. There will be other conditions that must be fulfilled, as well.
I will highlight them as they arise in the discussion.

Suppose disconfirming evidence arises for a given theory. The proponent of this
theory either directly observes this disconfirming evidence, or reads about it in a journal
that she hadn’t seen, or hears about it in some other way. And, the proponent accepts the
evidence as true. This is an additional condition that must be fulfilled for the charge of
ad hocness to be appropriate, as ad hoc acts arise only in cases where the proponent of a
given theory accepts the disconfirming evidence yet wants to maintain her theory in the
face of this evidence. I will defend this claim below in section VI.

As it is disconfirming, the evidence is evidence against both her theory and its
auxiliaries together being true\(^5\). In this type of situation, there are always three choices
for the scientist: those that I first discussed in chapter 1, section X. Option A: The
scientist can discard her theory. Option B: The scientist can accept a hypothesis that

\(^5\) Again, I am using ‘true’ here for ease of discussion. Substituting ‘empirically
adequate’, or some other preferred phrase, will not affect the discussion. I discuss my
account’s neutrality between empirical adequacy and truth in section XVI of this chapter.
makes the evidence no longer disconfirming. Option C: The scientist can do nothing, recognizing that there is a tension, or even an inconsistency, between the evidence and her theory plus the background but deciding to hold off doing anything about it for the time being. Our scientist must take option B in order to commit a potentially ad hoc act. Otherwise, she would not be fulfilling the second criterion, above.

Notice that to discard the theory itself is to take a different option than the option taken when a potentially ad hoc act is committed. This is because just throwing over one’s theory, or changing to a new theory, is a different kind of act than ad hocness. As it is a different kind of act, it might well have different criteria that need to be fulfilled in order for this kind of act to be done appropriately or inappropriately.6 So, I need to distinguish between option A and option B. This will involve determining what counts as moving to a new theory versus what counts as theory modification.

In cases where a charge of ad hocness can be appropriate, the scientist must keep the core of her theory intact. Change or modify part of the core of the theory, and she will be moving to a new theory. The core of the scientist’s theory consists in the part of the theory that, if eliminated, would cause the scientific community to regard the modified theory not as a version of the original but, instead, a new theory.7 And rejecting

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6 I discuss a related subject in the reply to one of the objections raised in section VIII of this chapter.

7 Jarrett Leplin speaks of this sort of distinction in his 1975 paper, in discussing problems of completeness and problems of fundamentality. The distinction that he makes is between ‘essential’ and ‘non-essential’ propositions of a given theory. The essential propositions constitute the core of a given theory. I do not wish, here, to concern myself with the difference between problems of completeness and problems of fundamentality (for the reasons discussed in chapter 3, sections VII and VIII). However, Leplin’s distinction points to the sort of distinction that I would like to make concerning what constitutes the core of a given theory.
the old theory or moving to a new theory is the kind of act where a charge of ad hocness is not appropriate.

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

It is always possible to take option A, and this act might be permissible or impermissible, good or bad. However, taking option A isn’t an action where a charge of ad hocness could be appropriate. So, examples like the Newtonian one above might be evaluated in some other manner but cannot be evaluated for ad hocness.

II. A Rough Definition of Ad Hoc Acts

Sometimes it will be appropriate for a scientist to take option B, and sometimes it won’t be. Suppose a scientist holds a particular theory to be true. She wishes to maintain her theory and, so, attempts to reconcile it with the newly arisen disconfirming evidence. She can’t continue to maintain all parts of her theory plus auxiliaries: if she did, this
conjunction would entail the falsity\textsuperscript{8} of the disconfirming evidence statement. However, as she is taking option B, she will still maintain the core of her theory, which is consistent with this evidence. She does remove the auxiliary hypotheses that conflict with the hypothesis to be accepted, making sure that she has previously removed such auxiliaries relevant to the theory (the hypotheses needed in order for the theory to be rendered testable) that, in conjunction with the core of the theory, were in conflict with the evidence itself. She will be left with the remaining auxiliary hypotheses and the core of her theory. The set of auxiliaries is consistent with the hypothesis to be accepted and, in conjunction with the new evidence, is also consistent with the core of this scientist’s theory. The new evidence, too, is no longer disconfirming for the core of the theory plus the auxiliaries.

The scientist has committed an ad hoc act if, and only if, the core of her theory, in conjunction with the pared-down set of auxiliaries and the (previously) disconfirming evidence, does not entail (the high objective probability of) the hypothesis. In this case, the scientist would be choosing option B when she ought to be choosing either option A or option C. In this instance, it was not appropriate for the scientist to take option B.\textsuperscript{9}

\textsuperscript{8} Or, if not falsity, the theory will entail the probability of the evidence, given the accepted theory, is extremely low. I will be using, for ease of discussion, only the first type of example. However, I think that my account of ad hocness also addresses situations where the probability of the evidence given the accepted theory in conjunction with its auxiliaries is extremely low but not zero. See especially section XVII of this chapter.

\textsuperscript{9} So she could have, for example taken option C, and used the hypothesis in conjunction with her theory and auxiliaries in order to make predictions, or to investigate some other phenomenon. What she ought not to have done is actually accept the hypothesis into her theory.
On the other hand, this act is not ad hoc if, and only if, the core of our scientist’s theory, the pared-down auxiliaries and the evidence together entail (the high objective probability of) the hypothesis being accepted.

When the discussed entailment does not occur, there is a sense of arbitrariness in the scientist’s decision to accept the hypothesis that she did. Why accept this one and not some other? Ad hocness, then, is a type of arbitrary act that seems methodologically suspect. The scientist decides to accept one response to a problem, when the available evidence and the scientist’s theoretical commitments do not warrant this choice over other possible hypotheses that would also resolve the problem. And, it is enough that the theory, with the appropriate auxiliaries and the evidence, admits of other responses, whether our scientist has articulated them or not. ¹⁰ In these cases, it is appropriate either to choose A or C, and not option B.

III. A More Detailed Definition of Ad Hocness

Now that I have given a rough sketch of my account, I need to describe it more formally:

Let M be an agent, T be a theory, B be a body of background knowledge, and t be a time, such that M accepts both T and B at t. Let Tc be the core of T and let T include a set of auxiliary hypotheses, used in tests of Tc, in addition to Tc itself. Let E be an evidence statement that is recalcitrant for T, given the background B. Let M accept E. Let H be a hypothesis that is inconsistent with some set of auxiliaries in T. Let X be the set of (relevant) auxiliary hypotheses in T that remains after (1) shrinking the set to make E be consistent with Tc and the remaining auxiliaries, and (2) then shrinking the set to

¹⁰ So, as a result of my view, a person might commit an ad hoc act without realizing it; it would be up to the scientific community to figure that out.
make H be consistent with Tc and the remaining auxiliaries.\textsuperscript{11} Let A (= \langle M, T, B, E, H, t \rangle) be the act in which agent M, at time t, encounters the problem to T posed by E, and attempts to solve it by accepting the hypothesis H\textsuperscript{12}, while retaining his commitment to Tc, B and X.

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

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

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

When M accepts T and, in T, the core of the theory plus the pared-down auxiliaries does not entail H, there is a sense of arbitrariness in M’s decision to accept the hypothesis H. Ad hocness, then, is a kind of arbitrariness that seems methodologically suspect. It involves choosing one response to a problem, at a time when one’s commitments and the available evidence do not favor that response over other possible responses to the same problem, even if there are no other responses that have been thought of by M.

One of the elements of my view that I wish to stress is that it is in accord with scientific practice because the charge of ad hocness, on my account, carries negative connotations.\textsuperscript{13} While ad hocness is always a vice, it is not an indefeasible vice. If there

\textsuperscript{11} See section XVII of this chapter for a more detailed discussion of these requirements.

\textsuperscript{12} In saying that A is the act made to solve a problem posed by E, notice that I am not saying that A is made solely in response to the problem posed by E. The latter formulation is reminiscent of the traditional view of ad hocness, where the psychological motivations of the scientists come into play. Instead, my formulation is just meant to be a methodological comment. This is the public justification for accepting the hypothesis H: that is, to solve the problem for T + B caused by E.

\textsuperscript{13} This is another reason why accepting is the appropriate act, instead of introducing. One of the problems raised by identifying the introduction of a hypothesis as something that can be ad hoc is that ad hocness can no longer just be a vice. It might be perfectly o.k. to entertain a certain hypothesis in a situation where it would be inappropriate to
were virtues of a hypothesis – such as simplicity or explanatory power – there might be reasons\textsuperscript{14} to accept the hypothesis \textit{despite} its being ad hoc. Additionally, if, according to my view, the acceptance of a hypothesis is not ad hoc, I do not wish to claim that it is rationally required to accept this hypothesis: rather, I claim that the acceptance would be rationally permissible if there are not other vices to which the acceptance succumbs.\textsuperscript{15}

Let me pause to clear up a potential misunderstanding of my account. My account might seem to fall prey to the same problem that Leplin’s 5\textsuperscript{th} criterion does. In this criterion, Leplin maintains that an ad hoc hypothesis is a solution for one specific problem, where any satisfactory solution of this problem must also provide help in solving other, related problems. This criterion rests on an empirical claim: that there accept that same hypothesis. When discussing Strevens’s view, I have already argued as to why ad hocness ought only to be a vice, and never a virtue (see chapter 2, section VIII and have argued against the artificial union of ‘good’ ad hoc cases and ‘bad’ ad hoc cases. I have also argued related points in chapter 1, section XI.

\textsuperscript{14} In section XIII of this chapter, when discussing how it might seem that ad hocness admits of degrees, I discuss why there might be epistemological or pragmatic reasons to accept a hypothesis, despite its being ad hoc to accept it.

\textsuperscript{15} I wish to stress the last part of this sentence because I do not think that ad hocness is the only vice out there. Suppose, for example, that a certain hypothesis acceptance is not ad hoc. There might still be something wrong with it that would give use a reason to think that we ought not accept it into our theory: its known falsity, for example. I also wish to stress that it is the \textit{acceptance} of a given hypothesis that is the sort of thing that can be labeled ad hoc or not ad hoc. I think that this is true of all of the vices. So, in the case where the hypothesis is known to be false, it might well be rationally permissible to use the hypothesis as a placeholder – to act \textit{as if} the hypothesis were a part of the theory – while acknowledging that, ultimately, this hypothesis ought not actually be a part of the theory at the given time. For the acceptance of this hypothesis would not be satisfactory at this time. And this is the same with ad hoc hypothesis acceptance. An ad hoc hypothesis may still be entertained, used, et cetera. It just ought not be accepted – at least, not at \textit{that} time with \textit{that} particular version of the theory and \textit{that} background. Unless, of course, there are other virtues that it has that give a scientist epistemological reasons to accept it. In which case, the hypothesis might be accepted \textit{despite} its being ad hoc.
exist certain connections between certain related problems, whether the given scientist sees them or not. I have discussed, in chapter 3, section IX, why the empirical nature of this claim is a problem. Basically, the problem is that a scientist might not be in a position to know whether something is ad hoc or not, because the scientist might not know about the connections between certain related problems. Therefore, methodological advice is not possible in this sort of case. Yet, on Leplin’s account, that is what was being promised.

The entailment requirement states that, when the disconfirming evidence plus the core of the theory and the relevant auxiliaries entail a given hypothesis, \textit{whether the scientist is aware of this entailment or not}, the acceptance of this hypothesis is not ad hoc. The italicized phrase might seem to be vulnerable to the same objection to which Leplin’s criterion 5 is vulnerable. However, this is not the case. The entailment requirement is not dependant on empirical claims. Thus, commitment to the entailment found in my account does not raise the same problems as found in Leplin’s criterion. For this entailment to hold is just for M to be committed to a specific theory relative to background B and for the core of the theory plus the pared-down auxiliaries and the evidence to have this particular entailment relationship to H. It is the fact that Leplin’s criterion depends on empirical claims that gives rise to the problem for his criterion.

In other words, Leplin’s account of ad hocness depends on \textit{empirical} facts – facts that the scientist introducing the hypothesis may have no way of knowing. My account, on the other hand, depends on whether a \textit{logical} relationship holds, which the scientist
may or may not have noticed. Therefore, there is a disanalogy between Leplin’s 5th criterion and my entailment requirement, despite a superficial similarity.  

IV. Why Arbitrariness is the Vice of Ad Hocness

Suppose we consider the lab-break-in case introduced in chapter 2, section VI. In this case, Brady accepts the hypothesis that someone has broken into his lab and changed the records. He accepts this hypothesis in order to reconcile his theory concerning the boiling point of mercury with the evidence that the temperature written down was different from what the theory maintains it should be. So, Brady takes option B. Yet, in this situation, option B does not seem to be the appropriate option to take: there are other ways to reconcile the new evidence with his theory and it is underdetermined which, if any, of these ways should be accepted. His assistant could have accidentally written the temperature wrong, for example. Or, the room might not have been at standard atmospheric pressure. Or, the substance being tested might not have really been mercury. So, Brady is jumping to conclusions. If some other scientist’s situation is appropriately like Brady’s, she, too, will pick one hypothesis rather than another, when there are insufficient grounds for doing so. It is arbitrary, a case of jumping to conclusions, to pick that specific hypothesis. It is clear that Brady’s action was inappropriate given the

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16 There is an initial criterion in my account that depends on empirical facts. In order for an act to qualify as potentially ad hoc, the scientist committing the act must keep the core of her theory intact. What determines the core of the theory is the consensus of the scientific community. However, this dependence on empirical facts in this criterion is not problematic in the way that the dependence on empirical facts is problematic in the criterion just discussed.
circumstances: Brady did something wrong when he accepted the lab-break-in hypothesis in order to reconcile his theory with the disconfirming evidence.

The arbitrariness in this sort of situation in accepting one hypothesis over another shows why this sort of move is a vice. Now, I need to show that this vice is ad hocness. I now turn back to the traditional view of ad hocness to motivate this claim. On the traditional view, a hypothesis is ad hoc if it is introduced solely to save a theory from recalcitrant evidence and has no additional testable implications.\textsuperscript{17} There seems to be something paradoxical going on, here. On the one hand, it seems right to say that you shouldn’t accept a hypothesis \textit{just because} it saves your theory or \textit{just because} it has no additional test implications. On the other hand, according to good scientific practice, we are \textit{supposed} to modify our theory to fit the facts. However, the seeming paradox becomes unproblematic when we turn to the intuition underlying the received view: that certain types of modifications are unwarranted, and that they are unwarranted because there is no real reason to accept this hypothesis rather than another.

On my view, an ad hoc acceptance contends with the recalcitrance of the new evidence in one particular way, by accepting one particular hypothesis. However, there are other ways to reconcile the new evidence with the given theory and it is underdetermined which, if any, of these ways to reconcile the new evidence with the given theory should be accepted. So my view is in keeping with what underlies the traditional view of ad hocness: the idea that to do something ad hoc is to do something arbitrarily. It does so while avoiding the traps that the traditional view does not: so, for example, my view does not involve independent testability and does not require

\textsuperscript{17} This formulation should sound familiar from my discussion of Hempel in chapter 1, section V.
knowledge of the psychology of individual scientists in order to determine whether a hypothesis acceptance is ad hoc.\textsuperscript{18}

\textit{V. Examples of Non-Ad Hoc Acts}

Here are two examples that are in contrast to the lab example. Both of these are examples of non-ad hoc acts. Here’s the first:

\textbf{Neptune case:} Observations of Uranus made by the mid-1800’s seemed to conflict with the predictions of Newton’s laws of motion and gravity. Adams and Leverrier modified auxiliaries to accommodate the observations, predicting Neptune’s existence.

This is an example of a non-ad hoc hypothesis acceptance because the core of Newtonian theory plus the pared-down auxiliaries and the perturbations of Uranus does entail the Neptune hypothesis\textsuperscript{19}. Notice, however, that the acceptance of the Neptune hypothesis is the sort of act that can be tested for ad hocness. There is recalcitrant evidence: Uranus is not moving in a regular, elliptical orbit as predicted by Newtonian theory and its

\textsuperscript{18} I have discussed why these, and related consequences for related views, are problems in chapter 1, sections VI-IX.

\textsuperscript{19} Of course, I am using the name of the planet for ease of discussion. A theory (although not this one) might entail, given such and such evidence coming to light, the hypothesis that ‘Saturn must be farther away from the Earth than Venus”, even though, in the language of this theory, Saturn is labeled ‘Ares’ and Venus, ‘Aphrodite’. In fact, the theory might not have names for these entities at all. It is enough that the hypothesis would identify these entities in enough detail as to be fairly clear to what phenomena the hypothesis applies.

Even more than just the name, it is not that the theory plus the relevant auxiliaries and the evidence needs to entail the exact shape and size and mineral composition of, e.g., Neptune. It is enough that a body or bodies of a certain mass in a certain location, or a specific center of mass are entailed.
background. Adams and Leverrier did wish to maintain Newtonian theory in the face of this recalcitrant evidence, while accepting the veracity of this evidence. The auxiliary hypothesis being discarded before accepting the Neptune hypothesis is the hypothesis that there are only seven planets. What is being discarded is not part of the core of Newtonian theory. So, this hypothesis acceptance fulfills all the criteria for an act that can be considered ad hoc.

Adams and Leverrier both independently modified Newtonian theory. They both modified the auxiliary hypothesis that said that there were only seven planets. In its place, both postulated that there was an eighth planet whose gravitational interaction with Uranus was such that Uranus’s orbit would be compromised in the way that it actually had been. This hypothesis acceptance was not ad hoc because Newtonian theory plus the evidence concerning Uranus’s orbit being eccentric in the specific manner that it was, entailed the existence of another planet (or some amalgamation of planets with a center of mass located in the same area) with these general characteristics. The theory was committed to this because Newtonian laws of gravitation and of planetary motion entail that such a planet must exist if certain eccentricities were to be observed.20

Here is a second example, also illustrating an act that, after being evaluated for ad hocness, is found to be non-ad hoc:

**Vulcan case:** Observations of Mercury made by the mid-1800’s seemed to conflict with the predictions of Newton’s laws of motion and gravity. Leverrier and others modified auxiliaries to accommodate the observations, predicting the existence of a large intra-Mercurial mass sometimes known as Vulcan.

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20 I will return to this example when discussing two potential problems for my theory, in the formulation of X, in sections XX and XXI.
The acceptance of the existence of intra-Mercurial matter is an act analogous to the acceptance of the Neptune hypothesis. Newtonian theory and auxiliaries plus the evidence concerning Mercury’s eccentric orbital behavior entailed the existence of a certain amount of mass in a certain location – in this case, closer to the Sun than Mercury.

One example might seem to be enough to illustrate what makes a qualifying hypothesis acceptance not ad hoc. I bring up the second example for two reasons. Firstly, the Vulcan case highlights ad hocness’s independence from other vices and virtues, such as falsity and truth. Scientists were equally justified in both of these cases to postulate a planet or a certain amount of mass existing in a certain location, or mass with its center of gravity in a certain location. The reason why we now acknowledge the existence of Neptune and do not acknowledge the existence of Vulcan is because we have reason to believe that the intra-Mercurial hypothesis is false and that the Neptune hypothesis is true. Yet, both cases are examples of non-ad hoc hypothesis acceptances.

Secondly, in Chapter 1, section X, I critique the idea that repeated modifications of a theory of the same kind are the sorts of acts that can be labeled ‘ad hoc’. Having these two examples, where the same theory is modified in a similar manner, and yet neither hypothesis acceptance is ad hoc, is another demonstration of why this conception of ad hocness does not capture the nature of ad hocness. Of course, these examples are not conclusive evidence against the repeated modifications view. A repeated modifications advocate might recognize the non-ad hocness of both of these modifications and still hold her view of ad hocness. She might just modify her view to require more than, e.g., three modifications of this type for the set of modifications to be
deemed ad hoc. Or, she might point out that both the Vulcan modification and the Neptune modification could be verified through observations, while the sorts of repeated modifications that she deems ad hoc are those that cannot be verified in such a way. However, these examples, taken together, at least make one question the repeated modifications view.

**VI. What If The Evidence Itself is Rejected?**

One might claim that there is an additional type of ad hocness: that is, that a charge of ad hocness can be appropriate either when the conditions stated in section I of this chapter are fulfilled, or when all but the condition requiring the scientist to accept the recalcitrant evidence are fulfilled. The second type of ad hocness, on this view, occurs when the scientist rejects the evidence, claiming that the evidence is false, or faulty, or something like that, where the scientist has no other evidence for this claim other than the tension that will arise for her theory. Suppose, for example, that Ludmilla is a Newtonian and part of her (non-core) commitments includes the hypothesis that there are seven planets in the solar system. Evidence comes to light that one of the planets has an anomalous orbit, given Newtonian laws of gravitation and motion, and given the hypothesis that there are seven planets in the solar system. Ludmilla, who wishes to remain a Newtonian, dismisses this evidence as specious, without any real evidence to do so. Someone that wants to claim the additional sense of ad hocness detailed above would

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21 I discuss both of these possibilities in Chapter 1, section X and discard them as unacceptable characterizations of ad hocness. I am bringing up these considerations, here, to acknowledge that the Neptune and Vulcan examples are not a set of decisive counter-examples to the repeated modifications view.
want to say that Ludmilla’s dismissal of the evidence is ad hoc. There are four reasons
why I wish to resist acknowledging this putative type of ad hocness. Firstly, and most
importantly, these sorts of cases do not seem to be cases of ad hocness. There might well
be something methodologically suspect going on: Ludmilla might be rightly accused of
being dogmatic, or intellectually dishonest or lazy, but these are problems other than the
problem of committing an ad hoc act.22

Secondly, treating this type of evidence rejection as ad hoc would cause ad
hocness to sometimes be a virtue. Suppose theory T is faced with disconfirming
evidence E. Charles, a proponent of T, rejects the evidence only because it conflicts with
T. Yet, T is really well confirmed. So, a lot of the evidence for T is evidence against E.
It might well be the case that Charles’s rejection of E is actually the appropriate move to
make. Yet, according to the second version of ad hocness, this rejection would qualify as
ad hoc. So, it would have to be the case that certain types of ad hoc acts are
methodologically justified and, therefore, not always vices. And, I claim, this is
problematic.

Thirdly, if I were to claim that a charge of ad hocness could be appropriate in
situations where the scientist rejects the evidence, I will get into one of two problems.
Suppose I were to read the claim that there is no other evidence for the rejection
hypothesis to mean that there is no other reason for the scientist to accept the hypothesis
that the evidence is false or faulty other than it will save her theory from disconfirming
evidence. That will import all the psychological concerns that I discussed in Chapter 1.
This reading would also create two different types of phenomena that could potentially be

22 See, for example, footnote 15 in this chapter.
charged with ad hocness: a psychological problem and a methodological problem. Or, I might read the no other evidence claim as stating that there is no evidence in the theory plus auxiliaries plus theoretical background for the rejection of the evidence. However, there will likely not be any such evidence because the new evidence is disconfirming for the theory plus auxiliaries plus theoretical background. The only way that there might be additional evidence would be if some other theory held by the scientist were already in tension with the theory for which the evidence was disconfirming.

Fourthly, it seems a virtue of my theory that there is only one vice of ad hocness. If I were to add another, it might seem arbitrary to stop at just two kinds. Why not twelve? And, the more kinds of ad hocness that are characterized, the less it seems that there is any such thing as a definitive vice of ad hocness: rather, it would seem as if people are just using it as a catchall term. And I do not think that this is the case. This last point is the least important of the three, as I already think that accepting the evidence rejection hypothesis, while sometimes potentially unwarranted, is a different sort of act than the sort of act that can be labeled ‘ad hoc’.

VII. Vagueness is to be Embraced – Or, At Least, to be Accepted

The fact that my account of ad hocness claims that the core of the theory must still be intact upon revision for that revision to qualify as potentially ad hoc might seem to be problematic. After all, you might say, aren’t there going to be cases where it is not clear whether the acceptance of a hypothesis in the face of disconfirming evidence eliminates part of the core of a given theory? Or, to use Leplin’s characterization of ‘essential
propositions’ in a theory (Leplin 1975 325), might not there be a time when the scientific community is split as to whether the modified theory counts as the same as the original? In response to worries like this, I agree that there is no clear-cut line between non-essential and essential, or core and non-core: what constitutes the core and what doesn’t can be vague. However, in many cases, a given element will clearly be part of the core. Geocentrism is definitely a part of the core of Ptolemaic astronomy, for example. And, there are many cases where a given element will just as clearly not be a part of the core. As the line is vague, there will be times when it is indeterminate whether a proposed move qualifies as something that could be ad hoc or whether the move cannot so qualify because it is really a move between two different theories. As vagueness, if it is even a problem, is a problem for almost all accounts of everything, I do not see it as a particular problem for me.

While the previous argument ought to be enough to convince the reader that vagueness is not unique to my account, I will take an even stronger line. There are two ways to characterize this vagueness. That the line between what qualifies as a potentially ad hoc act is vague may be compared to whether or not there are similar issues of vagueness in other philosophical theories. Or, the vagueness of my account of ad hocness may be compared to the actions being labeled. In the first case, there is reason to accept such vagueness. In the second, there is reason to embrace it.

My view does have an element of vagueness to it, according to the first characterization of vagueness. The line between the core and the non-core parts of a theory is vague. However, many – if not all (philosophical) theories – involve vagueness in some manner. Is an object observable or is it not? Is this animal definitely part of one
family or is it definitely part of some other? Just because there is vagueness in all of these examples does not mean that there is not a real distinction between objects that are definitely observable and those that are definitely unobservable, or animals that are definitely part of one family and animals that are definitely part of some other. There might be some difficulty, for example, in determining whether a platypus is part of one particular family but it is easy to determine whether a cat and a beetle are a part of the same family. And, I claim, there is a real distinction between hypothesis acceptances that can be evaluated for ad hocness and those that cannot. So, this type of vagueness is no more a problem for my view than for pretty much any other philosophical view.

Let us consider the second characterization of vagueness. It is true that what counts as ad hoc on my view will sometimes be vague. However, what counts as ad hoc is vague. There is only a vague border between what counts as a modification of an existing theory as a response to recalcitrant evidence, and throwing over a theory for a new one in the face of recalcitrant evidence. Yet, potentially ad hoc acts are the adoptions of hypotheses to reconcile a theory with recalcitrant evidence. So, if the division between the sorts of acts that can be considered ad hoc and those that cannot is vague, so much the better that my view is vague in the same way as what it is categorizing. A vague concept needs a vague analysis. And, I have done this. Thus, this vagueness is actually a virtue of my account.

There is an additional way in which my account is vague, but where this vagueness is a virtue for my account. In footnote 8 of this chapter, I claim that my account will also settle situations where the probability of the evidence given the accepted theory in conjunction with its auxiliaries is extremely low but not zero. There
will be a vagueness in the statistical threshold for these cases: what counts as close
enough to zero, what clearly doesn’t count as one of these cases. This vagueness is a
virtue for my theory because questions of ad hocness only arise when there is recalcitrant
evidence. In cases where, instead of logical inconsistency, there is statistical
improbability, there will be a vagueness in what counts as statistically improbable. My
theory mimics this vagueness.23

VIII. How Can New Theories Ever Come To Fruition Without Committing Ad Hoc Acts?

I have previously discussed the criterion for acts that can potentially be ad hoc.
The following two actions fail to meet these criteria. However, it is worth looking at
these in more detail, as they are actions that, at first blush, appear to be counter-examples
to my characterization of ad hocness. 1. A very general statement is accepted into a
theory in the face of disconfirming evidence. It seems impossible that a theory entail
such a general statement. Therefore, any time a very general statement is accepted into a
theory under the right circumstances, this acceptance must be ad hoc. But, of course, we
wouldn’t want to label all of these acts as ad hoc. 2. In the case of new or vague
theories, all hypothesis acceptances will be labeled ad hoc under my theory. This is
because such theories are not committed to much and, therefore, will entail little or
nothing. I will describe the objections in a bit more detail and then show why they are
not really objections to my view at all.

23 I discuss these sorts of cases in more detail in section XVII of this chapter.
Take the first part of the objection. Either the acceptance of these really general, law-like statements requires discarding part of the core of the theory, or it does not. Suppose that it does require this. If so, accepting this general statement is to take option A, an act that is not a candidate for ad hocness. An example: suppose a theory consists in two commitments. 1. All violets are purple. 2. All violets grow only in Africa. Then, suppose Isaiah accepts the law-like statement, ‘All temperate zones contain a species of violet that grows there’. In order to accept this statement, Isaiah must discard (2) and, therefore, Isaiah is really taking option A, as he is throwing over his original theory for a new one.

If the acceptance of a law-like statement does not require discarding part of the core, then it becomes a matter of determining whether or not the proper entailment is there. It is true that we would need a very strong set of postulates on the side of the theory plus auxiliaries to entail a general, law-like statement. However, the theory itself contains law-like statements (for example, Newtonian theory contains statements such as ‘all changes in a body’s inertia will be a result of a force applied to that body’) and law-like statements are certainly strong enough to entail other law-like statements. The proper entailment won’t always exist. However, due to the strength of the statements already found in the theory, neither will it be the case that the proper entailment never occurs or too rarely occurs.

Now turn to the second claim. Suppose the theory in question is one in its initial stages or one that is very vague in its commitments. The theory is not committed to much and the theory, in conjunction with its auxiliaries, does not predict much at all. The worry is, then, that if a hypothesis is accepted into such a theory, it will be highly
unlikely that the theory will entail (the high probability of) this particular hypothesis. This is because it will be highly unlikely that the theory entails (the high probability of) much of anything. So, all such hypothesis acceptances would be labeled ad hoc on my account, according to this claim.

There is a fairly straightforward answer to this claim, as well. Given such a vague theory, it is highly unlikely that any evidence that comes to light will be recalcitrant for the theory. A charge of ad hocness can appropriate only where a hypothesis is accepted as a result of recalcitrant evidence. In this sort of case, such a hypothesis acceptance is not the sort that can be judged to be ad hoc or not.

There is another response to this second claim. Suppose the theory, while quite nebulous, is firmed up enough that the evidence is actually in tension with it and its auxiliaries. If a theory is simplistic or vague enough, the modifications in the face of this evidence will not fulfill the criteria for potentially ad hoc acts. Suppose the theory currently has only two main parts. Now suppose that recalcitrant evidence arises. This evidence will cause a proponent to discard at least one of these two parts. And, if a theory only has two parts, discarding one would seem to radically modify the theory. Yet, radically modifying a theory is to really choose option A, while option B is the only sort of act that may be ad hoc. As an example, consider the following theory: 1. The Earth is the center of all the fixed stars and 2. All of the planets revolve around it. For this vague theory, evidence would need to point to the Earth’s not being the center, or that which the planets revolve around, e.g., for it to be considered recalcitrant. In order to

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24 The response to be outlined is similar in nature to the response to the first claim. The main difference is that, in response to the first claim, we did not have to suppose evidence recalcitrant for the theory.
make the theory coherent in the face of evidence such as this, a proponent must introduce
the following sort of hypothesis: the Earth revolves around the Sun. But, the addition of
this sort of hypothesis radically changes the theory. No longer is it a geocentric theory:
now it is a heliocentric one. And, this sort of radical change is the sort of change that
amounts to throwing over the first theory for a new, heliocentric theory.

IX. How Does My Account of Ad Hocness Decide the Ptolemaic Example?

Now that I have addressed the most immediate objections to my account, I will
apply my test to the Ptolemy case. In my characterization of Ptolemy’s original view,
Ptolemaic theory had the eight commitments discussed in Chapter 1, section IV. These
commitments included, among other things, the commitment to heavenly bodies’ orbits
being concentric with the earth and the commitment to heavenly bodies describing
perfectly circular orbits, where the bodies describe equal arcs in equal times in reference
to the center of the Earth. Then, the contraindicating evidence arises that seems to
indicate that the sun’s orbit is not a perfectly circular one with the orbit’s center located at
the center of the Earth.

The act being evaluated is Ptolemy’s act of accepting the eccentric hypothesis in
response to the sun’s anomalous orbit. The agent is Ptolemy and the time t is whatever
time it was after the appearance of the contraindicating evidence that the eccentric
hypothesis was accepted. The background B consists in the remnants of Aristotelian
astronomy that were still lying about, as well as the then-current views on movement,
gravity, vision, etc. The disconfirming evidence is the observations that the sun’s orbit did not appear to be perfectly circular around the Earth: instead of its apparent seasonal movement being a regular back and forth, it appeared to speed up and slow down.\textsuperscript{25} The question then arises as to whether the core of Ptolemaic theory, the pared-down auxiliaries and the (formerly) recalcitrant evidence together entailed that the sun’s orbit must lie on the eccentric postulated by Ptolemy.\textsuperscript{26}

In the face of this recalcitrant evidence, Ptolemy could have taken option A – he could have discarded his theory – or he could have taken option C – he could have lived with the tension between the recalcitrant evidence and his theory. Instead, it looks like he took option B – he accepted a hypothesis into his theory in order to reconcile the theory with the recalcitrant evidence.

Let us make sure that Ptolemy really did take option B. So, we need to make sure that this hypothesis acceptance fulfilled the initial criteria for ad hocness. Firstly, there must exist disconfirming evidence to the theory. It seems clear that this criterion is fulfilled: the sun’s orbit did not appear to be moving in the manner postulated by Ptolemaic theory. Secondly, Ptolemy must have accepted a new hypothesis into his theory. As discussed in Chapter 1, section IV, there is reason to believe that Ptolemy did

\begin{footnotesize}
\textsuperscript{25} For a reminder of the details of the sun’s anomaly, refer back to Chapter 1, section IV.

\textsuperscript{26} Just like in the discussion of Neptune and Vulcan, I am stating the hypothesis as the ‘eccentric’ hypothesis for ease of locution. This characterization of the hypothesis is not meant to imply that the theory already needed to have a definition of the term ‘eccentric’, for example. Again, as in the discussion of Neptune and Vulcan, neither is this hypothesis intended to be exceedingly (overly) specific. This characterization of the hypothesis is supposed to be shorthand for something like, ‘the sun must be on a perfectly circular orbit in relation to the center of the orbit, of a certain general size, whose center is not coincident with the center of the Earth.’
\end{footnotesize}
do this. However, let us stipulate that he was so doing. Thirdly, Ptolemy must have accepted the recalcitrant evidence. Ptolemy did seem to be taking the recalcitrant evidence seriously and did take it as a real problem for his astronomical theory.27

Additionally, the core of Ptolemaic theory must have remained intact: that is, the recalcitrant evidence must not directly threaten the core of his theory. Part of Ptolemaic theory was that the sun and the other heavenly bodies revolved around the earth on a perfectly circular orbit. This appears to be an essential element of Ptolemaic theory: the reason that the sun and the other heavenly bodies were supposed to move in perfectly circular orbits was because they were made of homogeneous materials and were, thus, spherical and moved within the ether, which was also homogeneous and, therefore, spherical. And at least three of Ptolemy’s theoretical commitments involved the perfect and homogeneous nature of the fixed stars and the heavenly bodies, to whose composition Ptolemy attributed the nature of these bodies’ movements. However, Ptolemaic theory also contained several non-essential hypotheses: one of these, for example, explained why the sun and the heavenly bodies similarly appear not to move in perfectly circular orbits around the center of the Earth by postulating the ecliptic – the angled plane on which these bodies’ orbits are situated. So, as Ptolemaic theory already had non-core components that reconciled the core with some seeming anomalous, non-circular motion. Therefore, it appears that the recalcitrant evidence in this case is not a direct threat to the core of Ptolemaic theory.

27 Again, for the quotational support and additional details, I refer the reader back to Chapter 1, section IV.
Given the core of Ptolemaic theory plus the pared-down auxiliaries and the (previously) disconfirming evidence, what (if anything) was entailed? It looks like Ptolemaic theory had, as a consequence, some sort of modificatory hypothesis of the following sort: even though the sun really does move in a perfectly circular orbit, it does not appear to do so, as a result of some sort of sleight of hand or trick of the eye, or difference in plane.\(^{28}\) The next question that arises is whether this is enough of a commitment that we might claim that Ptolemy was committed to the following hypothesis: the sun must be moving on an eccentric orbit with a given radius whose center is at a given position. Did the core plus auxiliaries and background plus the evidence entail this hypothesis? The answer to this question is ‘no’. There is a difference between one’s theory entailing that \textit{something} must be going on and entailing a specific hypothesis.\(^ {29}\) There are other options, here, and it seems arbitrary as to which one Ptolemy accepts to reconcile his theory with the sun’s anomaly. He could posit epicycles, or eccentrics, or some combination of both, or some other geometric device available to him.

\(^{28}\) Ptolemy himself says:

Since the next thing is to explain the apparent irregularity of the sun, it is first necessary to assume in general that the motion of the planets in the direction contrary to the movement of the heavens are all regular and circular by nature, like the movement of the universe in the other direction. That is, the straight lines, conceived as revolving the stars or their circles, cut off in equal times on absolutely all circumferences equal angles at the centres of each; and their apparent irregularities result from the positions and arrangements of the circles on their spheres through which they produce these movements, but no departure from their unchangeableness has really occurred in their nature in regard to the supposed disorder of their appearances. (Ptolemy 86)

\(^{29}\) To accept the hypothesis that \textit{something} must have happened is really the same as taking option C – acknowledging that the evidence is a problem for one’s theory and living with the tension. I will discuss this point in more detail in section XI of this chapter.
And Ptolemy himself did not think that maintaining both the disconfirming evidence and his theory committed him to the eccentric hypothesis. When a planet exhibits only one anomaly, like the sun:

The hypotheses [of epicycles and eccentrics] are interchangeable when, in the case of the hypothesis of the epicycle, the ratio of the epicycle’s radius to the radius of the circle carrying it is the same as, in the case of the hypothesis of eccentricity, the ratio of the line between the centres (that is, between the eye and the centre of the eccentric circle) to the eccentric circle’s radius; with the added conditions that the star move on the epicycle from the apogee in the direction of the movement of the heavens with the same angular velocity as the epicycle moves on the circle concentric with the eye in the direction opposite to that of the heavens, and that the star move regularly on the eccentric circle with the same angular velocity also and in the direction opposite to the movement of the heavens. (Ptolemy 88)

Yet, Ptolemy did accept the eccentric hypothesis. Therefore, Ptolemy committed an ad hoc act, on my account.31

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30 See, e.g., Ptolemy 93.

31 One might take issue with the conclusion that Ptolemy’s acceptance of the eccentric hypothesis is ad hoc and, so, arbitrary and a vicious act. After all, there are other vices or virtues that a hypothesis acceptance might have. And, in this case, the eccentric hypothesis was simpler, according to Ptolemy, than the epicyclic hypothesis. So there was a reason – simplicity – to choose the eccentric hypothesis over the epicyclic one. However, thinking in this manner is to confuse separate issues. Of course hypothesis acceptances can have all sorts of different virtues and vices. Of course these virtues and vices have to be weighed against each other in order to determine an appropriate course of action. However, just because some hypothesis has, say, a certain virtue, that does not mean that an already identified vice disappears. To say that would be similar to claiming that, since I held the door for you and bought you flowers, I magically was no longer late to our date. You might overlook my lateness because of the politeness and the flowers. However, my being polite cannot reverse time so that I actually was not late.

That is, there might be reasons to end up accepting a hypothesis despite the acceptance being ad hoc. I wish to stress the ‘despite’ because the acceptance is still ad hoc: the hypothesis was not appropriately entailed. I will discuss hypotheses having other virtues and vices when responding to the objection found in section XIII of this chapter.
X. A Final Illustration of the Ways in Which One Can Fail to Commit an Action Where the Charge of Ad Hocness Can Be Appropriate

In order to motivate the reader to think about her intuitions concerning ad hocness, I introduced the example of Planck’s constant in chapter 1, section II. I will return to it here, as I think that this example helps underscore the differences, discussed in the first section of this chapter, between the introduction of a hypothesis and the acceptance of one, as well as the difference between discarding a non-core component of a theory and discarding a core one.

Planck’s Constant: At the beginning of the 20th century, scientists were committed to either Rayleigh-Jeans statistical-mechanical theory of blackbody radiation or to Wien’s thermodynamical theory, or to some combination of both. There arose disconfirming evidence for both: shorter wavelengths did not produce the infinite amount of energy predicted by Rayleigh-Jeans and the observed emissions from longer wavelengths were not in accord with Wien’s theory. In order to reconcile theories of blackbody radiation with the disconfirming evidence, Planck postulated a constant, which ended up limiting the wavelengths possible.

Both Rayleigh-Jeans and Wien’s theories contained a presupposition that there could be no minimum unit: so, for example, Rayleigh-Jeans theory presupposed that there could be no minimum wavelength for the electromagnetic radiation being emitted by a body. And both, as was noted, encountered major disconfirming evidence in one part of the spectrum or another. Planck successfully solved this blackbody problem by introducing a constant, $h$, which enabled him to reconcile what had been postulated about electromagnetic energy with the observed data concerning the intensity of the radiation emitted by a blackbody at certain wavelengths. Yet, this constant, in that it limits the energy being emitted to discrete amounts proportional to the frequencies, leads to the implication that energy itself is quantized and that energy cannot be emitted at every possible wavelength but, instead, only at certain wavelengths.
It has been claimed that Planck’s explanation was not considered by Planck himself to *really* be an explanation of the phenomena but, rather, a mathematical formula that coincided with the observed phenomena.\textsuperscript{32,33} Suppose this is the case, and that Planck did not treat $h$ as anything more than a mathematical ‘fix’ to the blackbody problem and, so, did not accept the consequence that electromagnetic energy is quantized. In this case, Planck *introduced* the hypothesis of $h$ but did not *accept* it as part of his theory of energy.

On this reading of the Planck example, Planck didn’t accept the hypothesis when he introduced $h$. This makes its situation very different from Ptolemy’s because Ptolemy *accepted* the eccentric hypothesis in order to reconcile his theory with the sun’s anomaly. Merely *introducing* some hypothesis, as Planck did, without committing oneself or one’s theory to the consequences of that hypothesis, is not the sort of act where the charge of ad hocness is appropriate. He was simply using the hypothesis as a placeholder, so that scientists could get accurate results when estimating blackbody radiation at specific frequencies – something that was difficult to do, previously.

*Accepting* the hypothesis to reconcile one’s theory with the disconfirming evidence is one criterion for ad hocness. And, according to the reading of the Planck

\textsuperscript{32} See, e.g., Thomas Kuhn.


\textsuperscript{33} The way that Planck introduces his constant, $h$, is quite suggestive of this reading of Planck’s actions. When discussing stationary energy distributions and how to determine them, Planck says: “after the stationary energy distribution is thus determined using a constant, $h$, […]” (Planck 82) The reason why this quote is suggestive is because Planck speaks of $h$ as a constant, not *the* constant, and because he speaks of it only as a constant, not as the more fundamental number which it has come to be understood.
example just discussed, it did not fulfill this criterion. However, many people have thought that Planck did something ad hoc in the Planck example. In keeping with their intuitions, let us suppose that Planck really did accept the \( h \) hypothesis in order to reconcile blackbody radiation theory with the evidence. Would this have been a hypothesis acceptance where the charge of ad hocness could apply? In order to answer this question, we first need to make sure that this example fulfills the other criteria for ad hocness. There is disconfirming evidence: the actual blackbody radiation curve, that peaks at a certain, middle frequency and vanishes to zero on both the very short and the very long frequencies. Neither the Rayleigh-Jeans theory nor Wien’s theory predicted this particular curve. Next, we need to determine what theory Planck held, so that we may determine what constitutes the core of the theory.

It is not clear as to whether Planck held Wien’s view or Rayleigh-Jeans’s, or both, or neither. However, suppose he was committed to Rayleigh-Jeans, which held that blackbody radiation consisted in standing waves. This would have been held against a background that included, among other things, Maxwell’s wave equation. Part of the core of Planck’s theory, then, is that electromagnetic energy is wave-like in nature – hence, the standing waves of blackbody radiation. Is some part of the theory’s core going to be discarded, in this example? Yes. The modified theory includes the claim that electromagnetic energy is quantized. This replaces the central claim just discussed. So, Planck’s modified theory replaces the previous theory/theories of blackbody radiation. So, again, this example fails a criterion for ad hocness. Discarding a part of the core of a
theory is the same as taking option A, whereas the charge of ad hocness can be appropriate only when the scientist takes option B.34

XI. The Philosopher’s Myth of Ptolemy and My Account of Ad Hocness

I have been speaking throughout this work about Ptolemy. There is one important Ptolemaic example that I need to address, as it is the sort of example used by the repeated modifications proponents. In fact, I introduce the philosopher’s Ptolemy case in chapter 1, section X as an example amenable, at first blush, to the repeated modifications account.

A further examination of this case demonstrates not only that my test for ad hocness will be in accord with intuitions concerning ad hocness, but also that this example, properly fleshed out, shares a feature found in my account with the lab-break-in case introduced in chapter 2, section VI: that the acceptance of a given hypothesis is arbitrary, so that scientists are jumping the gun in accepting it.

There are two ways of reading the Ptolemy case, as the case is underspecified. Either each additional epicycle is entailed given the core of the theory, the (previously) disconfirming evidence and the pared-down auxiliaries, or they are not. When this case is gestured towards as a canonical case of ad hocness, I argue that it is the latter case they are implying. This is because there is a difference between unwisely being dogmatic

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34 The example would not have turned out differently had Wien’s, or a combination of Wien and Rayleigh-Jeans’s, theory been held. In order to claim that energy is quantized, a central claim for each of these possibilities must be discarded.
about one’s theory and committing an ad hoc move, as in the second case. A scientist may hold onto a theory when it seems clear that that theory has been seriously disconfirmed, or has become too unwieldy to use, or is likely to be false, or does not deal with evidence as well as some other theory, and, in so doing, non-ad hocly continue to modify her theory in the face of disconfirming evidence. In cases such as these, the scientist is not at fault for her theory modifications (barring other vices), while potentially being at fault for holding onto her theory. I argue that, if we are to examine the two possibilities in detail – the first, where the hypotheses are entailed and the second where they are not – we will want to call the latter ad hoc and the former not.

Let us examine the case where each additional epicycle is entailed. According to the Ptolemaic system, Mercury is located on an epicycle, which is itself orbiting on an equant – the center of motion that is not the center of the planet’s orbit – where its rotation is counter to that of the epicycle’s upon which Mercury moves. Now suppose that, at its apogee, Mercury is found to be further away from the Earth than this model predicts. Correspondingly, it is closer to the Earth at its perigee than the model predicts. And when it is between the apogee and perigee, Mercury appears to be moving back and forth in the sky more than it would given its one epicycle and its equant. This evidence is disconfirming for Ptolemaic theory, as the actual planetary positions are different than those postulated by the theory.

35 The epicycle and equant are needed to explain the two anomalies found in Mercury’s movements, that seem to prevent it from following a perfectly circular orbit: 1. The elongation and shortening of the angle between the sun and the planet; 2. The unequal time that the planet appears to take to move through the different parts of the zodiac. (Ptolemy 291)
Given that all heavenly bodies must move along circular orbits, and given that Mercury is correspondingly nearer and farther away than it ought to be, it looks like the core of Ptolemaic theory, in conjunction with its auxiliaries and this new evidence, entails an additional epicycle: one that orbits around the previous epicycle in the opposite direction and that has a certain diameter. Therefore, Ptolemy’s proponent, in adopting the additional epicycle, did not commit an ad hoc act.

Ptolemy’s proponent then makes more observations of Mercury’s movements. He notices that the addition of the new epicycle seems to make the model fit the data much better. However, he now has a better telescope and realizes that there are now some additional anomalous data points. Mercury appears to have, in places, too much or too little retrograde motion. Also, Mercury is not quite in the same plane at its apogee than at its perigee. These differences are all quite small but are real differences in Mercury’s actual motion from its predicted motion through the heavens. Ptolemy’s proponent still wants to save Ptolemaic theory from this disconfirming evidence. The core of the theory, in conjunction with the auxiliaries and the newly found evidence, entails a very small additional epicycle, traveling in the same direction as the first, upon which Mercury orbits. According to my account, this epicycle addition was not ad hoc, nor was the previous. And suppose that this scenario unfolds however many more times that you might wish it to. Upon reflection, what do we think about this account? We might think that Ptolemy’s proponent ought to change to a new theory, as this theory seems quite complicated to use for prediction. We might think that Ptolemy’s proponent

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36 That is, it does not appear to be far enough in one direction between the apogee and the perigee at certain points, and at others, it appears to be too far in one direction than it should be.
is being overly dogmatic in his insistence on maintaining this theory – like we might think overly dogmatic the person who refuses to recognize evidence just because it disconfirms her theory. However, given the commitment to Ptolemaic theory and given the evidence, I claim that these hypothesis acceptances would not be seen as ad hoc, nor does my view so label them.

In the second way of filling out the Ptolemaic example, there is no entailment, nor the entailment of a high probability for each additional epicycle. As opposed to the case above, suppose the movements were such that there were no unique solutions to each of the anomalies discovered in Mercury’s orbit. That is, the core of Ptolemaic theory, in conjunction with its auxiliaries and the new evidence, did not entail even the high objective probability of one particular hypothesis because there was more than one way to add epicycles, or other geometric apparati. Now suppose that Ptolemy’s proponent still insists on accepting some set of additional epicycle hypotheses in order to explain the anomalous evidence. In this case, the proponent adding these epicycles is jumping to conclusions. There is no reason to accept these additional epicycles. And, on my account, these hypothesis acceptances would be considered ad hoc. I argue, too, that we would agree that these acceptances are, in fact, ad hoc.

Notice that my account does not need to have multiple examples of these epicycles in order to label one acceptance of one epicycle as ‘ad hoc’. This seems more in accord with our intuitions: doesn’t it seem like something is wrong in this case before we add some (large) number of epicycles? Again, there is a distinction to be made between keeping a bad theory while accepting what the theory entails (as in the first reading) and making ad hoc moves (as in the second).
What do the second reading of the Ptolemy example and the lab-break-in example have in common? They are both clear examples of ad hoc hypothesis acceptances. And they are both cases where the scientists involved jumped to conclusions. This is to be contrasted with the Neptune case, or the Vulcan case, or the first reading of the philosopher’s Ptolemy case, where scientists did not jump to conclusions because the hypotheses acceptances were not arbitrary. The fact that the scientists jumped to conclusions in the cases that we consider ad hoc is the reason for that judgment and also support for my view that ad hocness just is the vice of arbitrariness or jumping to conclusions.

XII. ‘Something Must Have Happened’, or, Engineering Our Savior Hypothesis to Get Around the Entailment Requirement

In order to motivate this objection, let us turn back to Brady and the lab break-in. This seems a clear example of an ad hoc hypothesis acceptance. Yet, maybe Brady’s mistake was in accepting such a specific hypothesis. What if, instead, Brady had accepted the hypothesis that ‘something must have happened’, or the hypothesis that there was some sort of unspecified action that caused the temperature written in the notebook not to be 357°C? Wouldn’t the core of the theory, the auxiliaries and the temperature evidence entail this sort of hypothesis? Suppose a charge of ad hocness can be appropriate for the acceptance of the ‘something must have happened’ hypothesis. It looks like Brady will circumvent my standards by accepting a hypothesis of this type, when it looks like Brady is still doing something wrong in accepting such a hypothesis.
To address this objection, we need to contrast this scenario with what Brady did actually do, which was to accept the hypothesis that someone broke into his lab and changed the information. In the latter case, Brady is accepting the hypothesis that someone broke into the lab in order to explain the anomalous evidence. In the former, there is a conspicuous failure to explain anything at all. Brady, in accepting the hypothesis that ‘something must have happened’, is really just accepting that he does not know what happened: that is, he is taking option C. Brady knows that the temperature evidence is disconfirming for his theory but his theory, given its state at the time and the state of its background evidence, does not have the resources to resolve the issue. He is accepting that there is a problem for his theory but is living with the tension for the time being. Taking option C might be a problem in and of itself; however, deciding not to fix anything is not to do something ad hoc. It is to decide not to act at all, or to decide not to act for the time being.\textsuperscript{37}

XIII. But Doesn’t Ad Hocness Admit of Degrees?

I have already discussed, in section VII, why vagueness is not an issue for my account: there will be, for example, gray areas of whether a charge of ad hocness can be appropriate and there will also be areas where it is clear whether or not such a charge could be appropriate. However, there is a related objection that goes as follows. Suppose issues of vagueness are tabled. Still, there seem to be degrees of ad hocness that my

\textsuperscript{37} In Chapter I, section XI, I show that ad hocness is defined in terms of an \textit{action}: a commitment of some illicit move. For this reason, it would seem additionally strange to think of the non-action described above is a case where ad hocness could be considered.
account doesn’t address. On my account, hypothesis acceptances are either ad hoc or they are not. Yet, the objection continues, this dichotomy does not capture the notion of ad hocness because there are some putative hypothesis acceptances that seem to be more ad hoc than others. So, for example, the hypothesis that the substance wasn’t really mercury, or that Brady’s assistant wrote the temperature down wrong, or the hypothesis that the lab was not at standard atmospheric pressure, might seem a lot more likely than the break-in hypothesis, or the hypothesis that Brady’s evil nemesis changed the datum. On my account, if any of these hypotheses were to be accepted into the mercury-boiling-temperature theory, the act would be ad hoc. However, according to the objection, it seems natural to think that accepting either of the last two hypotheses ought to count as more ad hoc than accepting one of the first two.

It is true that my account does not admit of degrees in the way that the objection seems to desire. However, the reason that certain hypotheses seem to be more far-fetched than others is not a result of them being more ad hoc, and to think otherwise is to be not careful enough in distinguishing between potential problems. As I discuss in chapter 1, the label ‘ad hoc’ denotes a failure in procedure. The scientist, in accepting an ad hoc hypothesis, is committing a procedural error. One either commits a procedural error or one doesn’t. So, a hypothesis acceptance is either ad hoc or it is not. However, in

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38 The thought that certain hypothesis acceptances seem to be much more ad hoc than others is related to the following worry. I claim that ad hocness equates with arbitrariness: why accept this hypothesis when there are other (equally as) likely hypotheses that would reconcile the theory with the evidence. Yet, not all potential hypotheses seem equally as likely – or, even, almost as likely. And, therefore, there might well be a good reason to accept a hypothesis that seems much less ad hoc than its competitor, which, as it seems especially ad hoc, is not really a competitor at all. The answer to the worry just stated should be found in my discussion of the worry that ad hocness ought to admit of degrees.
individual cases, there may be *epistemological* or *pragmatic* reasons for certain hypotheses to seem more or less likely. That is to say, a scientist might have a higher *subjective degree of belief* in one hypothesis over another, while recognizing that both would be ad hoc to accept. There are many vices and virtues, of which ad hocness is only one vice. After being judged for ad hocness, a hypothesis acceptance can be found guilty of some other vice, or of some virtue. And the presence of these other vices and virtues are the cause of the scientist’s greater or lesser subjective degrees of belief in certain hypotheses.

In the Ptolemy-sun’s anomaly example, I note that it could have been rationally permissible for Ptolemy to accept the eccentric hypothesis – *despite* the ad hocness of this act – if the simplicity of this hypothesis outweighed its ad hocness. If this were the case, and the eccentric hypothesis really was much easier to work with, the simplicity makes it a *pragmatically* better candidate to accept than the epicyclic hypothesis: the two are no longer equal in their candidacy (according to pragmatic concerns) for reconciling Ptolemaic theory with the evidence. Now, suppose that we have an analogous situation to the eccentric-epicyclic case, with one major exception: there is no added virtue for one of the candidates. Instead, suppose we know the eccentric hypothesis to be false. A similar situation arises: to accept either hypothesis would be to commit an ad hoc act, because neither hypothesis is entailed by the theory and the evidence. However, the hypotheses are no longer equal candidates epistemologically because the eccentric hypothesis has the additional vice of being false. This combination of virtues and vices in addition to ad hocness or non-ad hocness is what causes the inequality of hypothesis acceptances to arise. What is important to note, however, is that it is not through degrees
of ad hocness that the inequality arises: rather, it is through the addition of virtues or vices to some of the candidate hypotheses.

This sort of answer might seem strange. After all, wasn’t the greater simplicity of the eccentric hypothesis an additional reason for Ptolemy to prefer it to the epicyclic hypothesis? And so, perhaps, it is no longer really arbitrary to pick the eccentric over the epicyclic. If it is no longer arbitrary, how can my account still label its acceptance as ad hoc? In response to the question, ‘why accept this one and not that one?’, one might respond, ‘because this one is simpler’.

However, this thinking misinterprets my view. There is no difference, methodologically, in accepting one hypothesis over another if the core of the theory, the pared-down auxiliaries and the evidence entails neither. Either one will be entailed or none will be. The fact that the eccentric hypothesis might be simpler does not speak to its being entailed or not. If a hypothesis is not entailed, then it is arbitrary to pick that one over another that is also not entailed. To accept one is to commit a procedural mistake: it is always a procedural mistake, in the relevant circumstances, to accept a non-entailed hypothesis such as the eccentric.

It is only in particular circumstances, as described in the Ptolemy-sun’s anomaly example described above, that it might end up being rationally permissible to accept a hypothesis – despite its being ad hoc on my account. A given hypothesis with a certain additional virtue in a certain circumstance might boost the scientist’s degree of belief in that hypothesis such that it might be permissible to accept that hypothesis over others. Even in cases where the scientist does not accept a hypothesis but, instead, uses it as a place-holder, the presence of these additional virtues and vices will make certain
candidates seem more or less appropriate for the job – even though the acceptance of any of them would be ad hoc. A good example of this sort of circumstance is the Planck case. When I discussed the Planck case in section X, I entertained the idea that Planck might have merely introduced the constant $h$ into his blackbody radiation theory. In this case, he would have been using the hypothesis as a placeholder, so that he and other scientists could get accurate results when estimating blackbody radiation at specific frequencies. Previously, it was very difficult to get such accurate results at all frequencies. And, this additional ease and accuracy was a very good reason for Planck to use such a constant as a placeholder.

In sum, accepting a hypothesis in the appropriate situation where this hypothesis is not entailed in the appropriate way is analogous to not following the proper procedures when flying a plane off a runway. One either follows the proper procedures or one doesn’t. This sort of methodological point does not admit of degrees. Where the seeming degrees of acceptability come in is in examining whether a given hypothesis has some additional virtue or vice that, for that specific circumstance, might give the scientist some epistemological or pragmatic reason for embracing that hypothesis, even though accepting said hypothesis would be an ad hoc act because the scientist would not be following the proper procedures in so doing.

XIV. An Objection Concerning the Use of Objective Probability

So far, the cases discussed where the charge of ad hocness could be appropriate have either ended up being non-ad hoc because the appropriate strict entailment existed,
or ad hoc because it didn’t. I haven’t discussed any cases where the core of the theory, the auxiliaries and the evidence entailed the high probability of a hypothesis. Yet, I have allowed that a hypothesis acceptance can be non-ad hoc if its high objective probability is appropriately entailed. I did not allow for this probability to be a subjective one because of the worries that arose for subjective probabilities when discussing Strevens in chapter 2, as well as for the psychology worries that arose when discussing Hempel’s definition of ad hocness. However, it has been notoriously hard to explain what it is to have high objective probabilities in a completely deterministic theory and it has been notoriously hard to come up with a comprehensive theory of objective probabilities.39

I make no claims about whether there might arise a plausible, comprehensive theory of these probabilities, although I remain cautiously optimistic. Even if there were no such comprehensive theory, there are still individual cases where theories do entail objective probabilities, such as certain interpretations of quantum mechanics that assign objective probabilities to events.40 And this sort of objective probability is fairly unproblematic. The reason why I allow the entailment of the high objective probability of a hypothesis is to allow for the testing for ad hocness in the case of a non-deterministic theory. Take the GRW interpretation of quantum mechanics, for example. There is, for macroscopic systems, a high objective probability of a nearly immediate collapse at any


40 In addition to collapse theories of quantum mechanics, such as GRW theory, where there are objective probabilities assigned to collapses at given times, there are the even more unobjectionable objective probabilities found in atomic decay: a given particle’s chance of decaying in the span of its half-life is 50%.
given time. The collapses are essentially non-deterministic and, so, can’t be entailed simpliciter. However, their high probability can be.

**XV. A Further Objection Concerning Probabilities**

It might be thought that, in limiting my account to situations where there must be either entailment or entailment of a high objective probability, I will mislabel the following sorts of cases: those where the core of a deterministic theory, in conjunction with the pared-down auxiliaries and the evidence, does not entail the hypothesis being considered for acceptance, yet we would want to say that the hypothesis being considered is highly likely. Here is an example of such a case: Dr. Morton has a theory a concerning what is wrong with Cary, her geriatric patient. She thinks that Cary has lung cancer. Some disconfirming evidence arises for this diagnosis: an analysis of the fluids taken in a needle biopsy does not show the appropriate levels of inflammation in the lung. However, Dr. Morton still thinks that Cary has lung cancer. She thinks the hypothesis that the needle biopsy happened to have produced a non-representative sample is highly likely – perhaps because, with similar diagnoses where similar disconfirming evidence of low levels of inflammation has arisen – it has ended up that a non-representative sample really was the appropriate modification to the diagnosis of lung cancer. Dr Morton’s determinate theory does not entail the non-representative sample hypothesis because there is some chance, although not great, that the anomalous evidence could have been a result of the patient having had undiagnosed TB that caused cysts that, when biopsied,
would not have a high level of inflammation. But, in 287 previous cases, it has turned out that, in cases similar to Cary’s, the biopsy was unrepresentative.

Yet the chance that either the biopsy produced a non-representative sample or the chance that the patient has had TB without being diagnosed with it is not some *objective* chance. This is a deterministic theory, and past history is not enough to produce an objective chance for this particular situation. So, the fact that there have been unrepresentative samples produced via needle biopsies in similar situations is not enough to claim that the theory, in conjunction with the evidence and the auxiliaries, entails even the high objective probability of there having been an unrepresentative sample produced. So, it seems like accepting this hypothesis would be ad hoc, on my account. Yet, Dr. Morton seems perfectly within her rights to start treating Cary for lung-cancer-with-an-unrepresentative-biopsy-sample.

This situation is not actually a problem for my account. Cases such as these are forced-choice cases. Dr. Morton has to do something, as she needs to treat her patient. Because, in other situations, the sample is often unrepresentative when there are low levels of inflammation present in the sample biopsy, Dr. Morton treats her patient *as if* she has lung cancer with an unrepresentative biopsy. It is a pragmatic choice for treatment because an unrepresentative biopsy is, in general, much more common than the presence of undiagnosed TB in geriatric female patients. Notice, however, that Dr. Morton is not, on this formulation of the case, accepting that the biopsy sample was unrepresentative as part of her theory. Instead, she is using it as a placeholder until she figures out what is really going on – if she ever does.
Suppose Dr. Morton really does accept that the biopsy was unrepresentative. If this is the case, then she has accepted the hypothesis ad hocly. However, she had to act because it is a forced-choice situation, so there are mitigating circumstances and these circumstances might be a reason not to impugn her as much for this act – despite the fact that she committed an ad hoc act.41

XVI. Is Neutrality Concerning the Meaning of Acceptance Still Possible?

The correct choices in cases such as Dr. Morton’s, where there is no entailment, are to either not do anything or to use some hypothesis as a placeholder. I have discussed, in section XIII, what might be grounds to introduce a certain hypothesis as opposed to some other as a placeholder. And I distinguish the criteria for accepting a hypothesis from the criteria for introducing a hypothesis, in part because I distinguish these two types of acts from each other. However, allowing for placeholders, and differentiating them from accepted hypotheses, brings up another putative problem for my account. In order to motivate the objection, let’s look at the reasons that Planck might have had for introducing the constant $h$. The main reason for its introduction seemed to be that he thought that it would make reliable predictions. And, in fact, it looks like one of the main reasons to have a placeholder is in order to enable a theory to

41 One might object to this sort of case on the grounds that both the biopsy sample being unrepresentative and the undiagnosed TB are both easily checkable. In order to assuage such worries, imagine the not implausible additional complications to the scenario: Dr. Morton has a limited amount of money that she can use to run tests and Cary needs immediate treatment. Dr. Morton cannot do an open-chest biopsy because it is too expensive and the complicated tests needed to rule out all infections that result in lowered white-blood cell production and, therefore, lower levels of inflammation, are too expensive and too time-consuming.
make predictions in a practical application: that is, in order to make particular predictions about particular situations. A scientist might be justly confident that she will get reliable predictions from a placeholder: after all, there were certain criteria that she used in order to choose the placeholder that she did. Planck was justly confident in the predictions that arose as a result of using $h$ as a placeholder. Yet, in these cases, the scientists still shouldn’t accept the hypothesis: placeholders are different than an accepted part of the theory.

So, it looks like using a hypothesis as a placeholder is to claim that the placeholder is empirically adequate but not (necessarily) true. When I discussed the difference between introducing a hypothesis and accepting one, in section I, I said that the additional step that is taken in accepting a hypothesis is to believe that it is true. So far, this seems to be in accord with the conclusion just stated. However, in this same section, I noted that I was using “true” for ease of discussion, and that I was remaining neutral between acceptance being tied to truth and acceptance being tied to empirical adequacy. And this looks to be a problem. It appears as if I no longer have a nice distinction between introducing a hypothesis and accepting one unless I take a side and claim that acceptance is just belief in the truth of the hypothesis. For, suppose I don’t take a side and, therefore, acceptance can be tied to empirical adequacy. Then, the constructive empiricist\textsuperscript{42} might claim that, according to his view, introducing a hypothesis or using it as a placeholder is no different than accepting it. Therefore, any

\textsuperscript{42} I am using the phrase “constructive empiricist” to identify the followers of van Fraassen’s constructive empiricism. See, for example:

van Fraassen, Bas. \textit{The Scientific Image}. Op cit.
placeholder needs to be tested for ad hocness, as it is the sort of act for which a charge of ad hocness can be appropriate. And, in cases where I claim that it is only appropriate to use a hypothesis (in the relevant circumstances) as a placeholder, the judgment will always be that the scientist has committed an ad hoc act. Dr. Morton, for example, was justified only in treating her patient as if she had lung-cancer-with-an-unrepresentative-biopsy, because even the high objective probability of the non-representative sample hypothesis was not properly entailed.

One possibility would be for me to bite the bullet and say that all of these (relevantly constructed) cases where someone used a hypothesis as a placeholder are cases where, actually, he committed an ad hoc act. However, this approach would make an enormous amount of acts ad hoc that do not seem to be ad hoc according to our intuitions and would also make for a strange account of ad hocness. Entertaining a hypothesis for the purposes of a discussion might well count as committing an ad hoc act on this approach, for example.

Another possibility would be for me to cede ground and say that my account is actually non-neutral with respect to what acceptance means: really, my account makes acceptance mean ‘belief that ____ is true’. Neither of these options is particularly attractive. I do think that there is a real difference between introducing a hypothesis and accepting one, and I do want my account to apply regardless of whether scientific realism is being assumed or not. However, I do have a reply that does not commit me to either of these options. The constructive empiricist is not being true to her own view if she makes the argument attributed to her concerning empirical adequacy. The assumption of

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43 Which would be to take the hypothesis even less seriously than using it as a placeholder, as there would be no real expectations for this hypothesis.
empirical adequacy – the confidence in reliable predictions – that arises in the placeholder case is not the sort of empirical adequacy that is the intended contrasted between constructive empiricism and realism. In constructive empiricism, the empirical adequacy must be global, in the sense that the hypothesis or theory that is being labeled ‘empirically adequate’ must give accurate predictions in all relevant cases. Contrast this notion of empirical adequacy with the assumption of empirical adequacy in the case of a placeholder. In the latter situation, all that is needed is the belief that the hypothesis will lead to the right empirical predictions for certain applications or situations. So, for example, when Dr. Morton treats Cary as if she has lung-cancer-with-an-unrepresentative-biopsy, Dr. Morton believes that doing so will give her the appropriate treatment for Cary. Dr. Morton is not, however, committed to believing that the hypothesis that the biopsy was unrepresentative is globally empirically adequate. So, my account is able to remain neutral as to the nature of acceptance, despite such objections.44 However, there arise other potential problems for my theory, including what counts as a member of X (the set of appropriately pared-down auxiliaries).

XVII. A More Detailed Definition of X

A more detailed definition of X is in order, as the following objections deal primarily with attempts to illicitly manufacture X. In section II, I claim that X is the

44 I am confident that my account can remain neutral in the realist-constructive empiricist debate. However, there might be some version of anti-realism that my account won’t support. If this is the case, I will bite the bullet that my account is neutral only with respect to the realist-constructive empiricist debate and, thus, rules out other forms of anti-realism. I do not think that this is a great sacrifice for my account to make, as van Fraassen’s constructive empiricism is the strongest challenger to the realist position.
pared down set of relevant auxiliaries left after removing the hypotheses in conflict with the evidence itself and then removing those that conflict with the hypothesis to be accepted. As there is a lot of information packed into this sentence, I need to make explicit what constitutes X and what ought to be in it.

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

Instead, I conceive of the theory as a list of discrete hypotheses accepted by the scientist at the time that the disconfirming evidence comes to light. These hypotheses are individuated in some sort of natural manner. X will be a pared-down list of propositions created by starting with T and many hypotheses. First, Tc will be removed. Then, those hypotheses that are, in conjunction with Tc, in conflict with the evidence itself will be removed. Then, those hypotheses that, in conjunction with Tc, are in conflict with the hypothesis to be adopted will be removed.

All hypotheses that, in conjunction with the core of the theory, conflict with the evidence must be eliminated. I will be discussing the hypotheses to be eliminated in terms of minimal sets of auxiliaries in conflict with the evidence, given the core of the theory. Here is what I mean by minimal set of auxiliaries inconsistent with Tc and E. Let \( \Gamma = \{A_1, A_2, \ldots A_n\} \) be a set of auxiliaries in T. \( \Gamma \) is a minimal set inconsistent with Tc + E.
if and only if \( \Gamma \) is inconsistent with \( T_c + E \) and removing some \( A \) from \( \Gamma \) will cause the set \( \Gamma \cup \{T_c, E\} \) to be not inconsistent.\(^{45}\) These minimal sets must be eliminated from \( X \): we can’t just pick certain hypotheses to eliminate from these sets and not others. To do so would be arbitrary in just the way that I define ad hocness. Instead, all such minimal sets must be crossed off the list of \( T \)’s auxiliaries in order to create \( X \). For example, suppose \( \{\neg V\}, \{\neg B\} \) and \( \{\neg G\} \) are all minimal sets. Then, in order to manufacture \( X \), the scientist must eliminate \( \neg V, \neg B \) and \( \neg G \) from \( X \).

In footnote 8 of this chapter, I mentioned that my account also handles cases where the probability of the evidence given the accepted theory in conjunction with its auxiliaries is extremely low but not zero: that is, when the core of the theory, in conjunction with its auxiliaries, are not inconsistent with the evidence but very unlikely. In these cases, there is an analogous way to eliminate auxiliaries that, in conjunction with the core of the theory, are in serious tension with the evidence. In these cases, the minimal sets of auxiliaries that need to be removed in order to construct \( X \) would be those that, in conjunction with the core of the theory, make the probability of the evidence close to zero. These sets will be minimal in the sense that removing one of the auxiliaries in this set will cause the probability of the evidence, given the remaining hypotheses in the set and the core of the theory, to dramatically increase. For example, suppose that Maya’s theory is that, when a specific coining machine \( C \) is in good working order, then the coins that it makes are fair. Disconfirming evidence arises: a coin made by \( C \) is flipped 1000 times and comes up heads 999 times. The auxiliaries that are being

\(^{45}\) By the set \( + T_c + E \), I mean \( \Gamma \cup \{T_c, E\} \)

\(^{46}\) Again, \( T \) is not deductively closed. Removing \( \Gamma \) will be a matter of ‘crossing off’ hypotheses from the list of hypotheses already affirmed by the theory.
considered are the following: 1. This coin was made by C. 2. C was in good working order when the coin was made. 3. The coin was not later tampered with. 4. Maya’s method of flipping the coin was fair. Clearly, these auxiliaries in conjunction with the core of the theory are not inconsistent with the evidence, as it is possible that a fair coin fairly flipped may land heads 999/1000 times. However, the probability of this evidence, given the auxiliaries and the core of the theory, is extremely low. The minimal set of auxiliaries that need to be crossed off in order to form X will be \{1,2,3,4\}. Take any of these away, and the probability of the evidence, given the rest of these auxiliaries and the core of the theory, will greatly rise. So, all of these auxiliaries will be in a minimal set to be crossed off and, therefore, not a part of X.

Finally, any hypothesis that, in conjunction with \(T_c\), is in conflict with \(H\) must be removed. \(\neg H\) needs to be removed (if it hasn’t been already), as well as any other set of hypotheses that are in conflict with \(H\). This is to ensure that \(\{T_c, X, E, H\}\) will be consistent. Then, \(X\) will be properly constructed. \(X\) just is the pared-down set of auxiliaries relevant to \(T\) after eliminating those in conflict with the evidence and with the hypothesis to be accepted. I will return to my definition of \(X\) in the following objections.

**XVIII. A Potential Problem With \(X\) (The Set of Pared-Down Auxiliaries): Smuggling Entailment**

Suppose Terry is a scientist who wants to maintain her theory \(T\) in the face of disconfirming evidence, so she considers accepting a certain hypothesis. She also wants to avoid a charge of ad hocness and, at this time, \(T_c + X + E\) does not entail the hypothesis that she wishes to accept. So, Terry constructs \(X\) to include a material
conditional, where she believes (or knows) its antecedent to be false. Terry can construct this conditional such that its placement in X will (wrongly) categorize her hypothesis acceptance as not ad hoc: she will just make her proposed hypothesis the consequent of the conditional. Since the antecedent is false, the conditional will be true. Then, since this true conditional will be a part of X, and its consequent is just the hypothesis she wants to accept, Terry can accept this hypothesis without making an ad hoc move because Tc + X + E will now entail her proposed hypothesis. And this seems to be a big problem for my account because, if X can be constructed in just this manner, no hypothesis acceptance need ever be ad hoc.

The problem can be characterized as follows: X, as stated previously, is some set of those auxiliary hypotheses Terry accepted before learning about the disconfirming evidence. Suppose one of the hypotheses Terry accepted at that time was not-E. Not-E (trivially) entails the material conditional, ‘if E, then H’, because to believe not-E is to believe the antecedent of this conditional false, which makes the conditional itself true. So, Terry builds this conditional into X. Then, the evidence comes to light. After this happens, Terry believes E. The conditional, ‘if E, then H’, is a part of her X. There now has arisen E, so H is entailed by Tc + X + E. And H just is the hypothesis that Terry wanted to accept in the first place.

Think of the lab break-in case as an example. Suppose Brady builds ‘if the temperature that I see written down is not 357°C, then someone broke into the lab and changed what was written’ into X in the manner just described. It then looks like Brady’s hypothesis will pass through the ad-hocness filter when he accepts it after discovering
that the temperature written in the book was not, in fact, 357°C. And this is certainly not a conclusion that we wish to draw from this example.

However, Brady cannot construct X in this manner. He is not allowed to include ‘if the temperature that I see written down is not 357°C, then someone broke into the lab and changed it’ as a part of X, simply because the conditional is trivially true based on what Brady believed before the disconfirming evidence came to light. Of the auxiliary hypotheses that Brady then accepted, every one that (or some combination that), with the core of the theory and the background, entails not-E will have to be discarded. These auxiliaries cannot be included in X because X includes only those that do not conflict with the disconfirming evidence. So, ~E needs to be discarded. But note that E ⊃ H is in the set of auxiliaries in the first place only because it was believed that ~E was true and this conditional was a trivial consequence of it. Recall that the auxiliary hypotheses in T are not a deductively closed set, but rather a discrete list of hypotheses already accepted by the theory. ~E was not needed to render T testable (as T with the appropriate hypotheses will still be testable after E comes to light) and, so, any consequence of E that is in the set of auxiliaries only because ~E was in the set of auxiliaries will also not be needed to render T testable. Therefore, E ⊃ H will not be a part of X.

Suppose Terry really does accept the proposed hypothesis, while keeping the core of her theory and keeping an illicitly constructed X of the type discussed above. Although Terry constructed her X so that she might avoid the charge of ad hocness, she has not succeeded in her attempt. Her hypothesis acceptance is still ad hoc. For any hypothesis acceptance where a charge of ad hocness could be appropriate and where X

47 By ‘consequence of E’, I mean any consequent of a conditional statement where E is the antecedent.
has been constructed illicitly but, if X had been constructed appropriately, the hypothesis would not have been entailed, this hypothesis acceptance is ad hoc.

XIX. Another Sneaky Way to Illicitly Construct X and How X’s Definition Prohibits It

Without smuggling in the type of illicit material conditionals discussed in section XVIII, it still looks as if X can be gerrymandered in such a way that certain hypothesis acceptances will come out as non-ad hoc, though they look to be clear examples of ad hoc acts. The following example will illustrate the problem clearly. Suppose Maria holds theory T. Then, disconfirming evidence E arises for T. Maria comes up with two (equally) possible emendations for T: K and H. T, before being modified, includes ~K and ~H. And, let us suppose that T + E entails the disjunction H or K. Now, Maria decides that she wants to accept H and she doesn’t want to commit an ad hoc act. So, she constructs her X carefully. The test for ad hocness states that X is the set of relevant auxiliaries left in T that remains after first removing all auxiliaries that, in conjunction with the core of the theory, are in conflict with the evidence and then removing all auxiliaries that are in conflict with the hypothesis being accepted. Maria gets rid of ~H but leaves ~K in X. X seems to be constructed acceptably because Tc + ~K is not in conflict with either the evidence or the hypothesis to be accepted. But, since ~K is still found in X, Tc + X + E entail H. In accepting H, then, Maria does not appear to be committing an ad hoc act. And, of course, the example would have gone similarly had Maria decided to accept K. Yet, both H and K seemed equally likely solutions when the
initial conditions were described: it seems arbitrary, therefore, to accept one and not the other. This outcome does not look good for my account because it looks like, whenever an acceptance ought to be labeled ad hoc, it will always be possible to construct $X$ in the manner that Maria did above.

I will illustrate the case by means of a concrete example. Suppose Maria’s theory is a general theory about how cars work. Suppose $E$ is the observation that her car won’t start. Suppose that $\neg H$ is the hypothesis that her car does not have a bad starter. Suppose that $\neg K$ is the hypothesis that her car does not have a dead battery. If Maria eliminates the hypothesis that her car does not have a bad starter, then the core of her theory plus the remaining hypothesis that her car does not have a bad battery entails that she must have a bad starter. And the example will run similarly if Maria wants to accept the hypothesis that her car has a dead battery.

To address this objection, I will return first to the way in which $X$ is supposed to be constructed. When speaking about $X$ non-formally, I state that $X$ contains the auxiliary hypotheses remaining that conflict with the hypothesis to be accepted, after the auxiliaries relevant to the theory were removed that, in conjunction with the core of the theory, were in conflict with the evidence itself. So, when $X$ is being constructed, the hypotheses in conflict with $E$ given $T_c$ must be eliminated. A scientist must eliminate the minimal sets of auxiliary hypotheses inconsistent with $T_c + E$ from $X$. So, in Maria’s case, a minimal set would be $\{\neg H, \neg K\}$. This is because $T_c + \neg H + \neg K$ is inconsistent with $E$, while $T_c + \neg H$ is consistent with $E$ and $T_c + \neg K$ is consistent with $E$.

Maria then needs to eliminate the minimal sets from $X$, of which $\{\neg H, \neg K\}$ is one. So, neither $H$ nor $K$ will be entailed by $T_c + X + E$. In terms of the example, Maria must
cross off both the hypothesis that she does not have a bad starter and the hypothesis that she does not have a dead battery in her construction of X. So, her theory plus X plus the observation that her car won’t start will not entail either the hypothesis that her car has a bad starter or the hypothesis that her car has a dead battery.

**XX. A Potential Problem with The Neptune and Vulcan Examples**

Instead of objecting that my account will allow acceptances to be not-ad hoc when they ought to be deemed ad hoc, as in the previous objection, someone might object that my account causes far too many hypothesis acceptances to be labeled ad hoc, including those that ought not to be. Take the Neptune case, for example. In section V, I presented this case as one that is a clear example of a non-ad hoc hypothesis acceptance. Yet, one might argue, this is actually a case where, on my account, the acceptance of the Neptune hypothesis would be ad hoc because the Neptune hypothesis would not be entailed. There might be some conglomeration whose net gravitational pull act on Uranus in such a way that Uranus’s orbit contains the observed perturbations. Therefore, the Neptune hypothesis is not entailed. And, continues the objection, if a case such as this is to be labeled ‘ad hoc’ by my account, so much the worse for this account.

In footnote 19 of this chapter, I briefly discussed exactly what needs to be entailed in order for a hypothesis acceptance in the relevant circumstances to be considered non-ad hoc. Certainly, it cannot be the actual planet Neptune that needs to be entailed. If that were the case, almost no hypothesis acceptance would be ad hoc. There would be way too much specificity and detail needed in such a hypothesis. It is not appropriate, for
example, to require the entailment of a planet that has the exact shape and size and mineral composition of Neptune in order for the acceptance of Neptune hypothesis to be non-ad hoc. Here, as in other places, an element of vagueness must be embraced. It is enough, for example, that the Neptune case entailed the existence of a body or bodies of roughly a certain mass whose center of mass is in a specific location for the acceptance of the Neptune hypothesis to be non-ad hoc.

What is entailed is that the center of mass be roughly at Neptune’s location. This hypothesis would avoid the objection stated above, where there is some set of planets jointly causing Uranus’s perturbations. While this is a hypothesis that is more vague than the Neptune hypothesis, it is certainly very different than, say, the hypothesis ‘something is going on’, discussed in section XII of this chapter. It is pointing to a specific sort of fix in order to reconcile Newtonian theory with the recalcitrant evidence of Uranus’s perturbations. And the Neptune hypothesis is a consequence of the center of mass hypothesis. It seems appropriate to allow for some leeway in what version of a hypothesis to accept, such as accepting the Neptune hypothesis version instead of the (more vague) center of mass version. Similarly in the Vulcan case, it seems appropriate to accept the actual Vulcan hypothesis instead of some other version of it. And these cases are quite different than the Ptolemy-sun’s anomaly case. In the Ptolemy case, all that could possibly be seen to be entailed would be something like the hypothesis that there is some set of circles positioned somewhere that cause the sun’s anomaly. Contrast this to the hypothesis that there is a specific center of mass in one specific location. The first seems analogous to ‘something happened’ while the second is actually giving us information that would help reconcile Newtonian theory with the anomalous evidence.
In short, there is and ought to be some amount of vagueness allowed in the entailed hypothesis. There are clear cases where whatever might be entailed is no more than ‘something happened’, and to take this is to take option C. And there are clear cases, such as the Neptune and Vulcan cases, where what is entailed is much more specific than ‘something happened’. Requiring incredible levels of detail and specificity in the entailed hypothesis is not desirable because to do so would be to set the standards too high for non-ad hoc hypothesis acceptances. It is enough that some version of the hypothesis being accepted is entailed by the theory and the evidence – even if it is a somewhat more general version of that hypothesis.

XXI. A More Nuanced Version of the Neptune Example and the Difference between It and Maria’s Case

Here’s another way in which it might seem as if the Neptune example ought to come out as ad hoc on my account. One might claim that, in the Neptune case, \( T_c + X + E \) will entail either the center of mass hypothesis or the hypothesis that a comet has hit Uranus in such a way so as to cause its perturbations. It looks, on the face of it, like the Neptune case is just another version of Maria’s H-K case, where there are two different possible hypotheses that might be accepted and there is no real reason to choose between them.\(^{48}\) If this conclusion were true, it would be quite bad for my theory. Firstly, the

\(^{48}\) And, this case does not have the redeeming feature of the cancer case, where there is a forced-choice situation. No one is going to die or get seriously ill if scientists decide to
acceptance of the Neptune hypothesis seems a prime example of a non-ad hoc act. Secondly, if we can construct this sort of situation in a case that clearly looks non-ad hoc, mightn’t it be possible to do so in other, seemingly canonical non-ad hoc cases?

However, the Neptune case and Maria’s H-K case are importantly different. Here is how the analogy is supposed to go: in each case, there are two auxiliaries, of which either one could be dropped. The minimal sets that need to be eliminated from X will include the hypothesis that there is no other planet and will include the hypothesis that there is no comet that will strike Uranus in a particular way. Eliminating both of these hypotheses from X, it looks like $T_c + X + E$ won’t entail the Neptune hypothesis, nor will it entail the comet hypothesis, because it allows for the possibility of either. In this way, it is supposed to be similar to Maria’s case, where neither the hypothesis that her car’s starter was bad nor the hypothesis that her car’s battery was dead were entailed, given the core of her theory and the evidence. So, it would seem, we would have to come to the same conclusion in the Neptune case as we did in Maria’s case, and claim that the acceptance of the Neptune hypothesis was an ad hoc act.49

The no-comet hypothesis may be construed more broadly, such as the Neptune hypothesis was in section V of this chapter. Instead, the two hypotheses to be compared to those in Maria’s case might be the more general additional-gravitational-source hypothesis and the hypothesis that there are non-gravitational forces acting on Uranus. Again, the analogy is supposed to be that, in both the Neptune case and in Maria’s case,

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49 For purposes of brevity, I am omitting any discussion of the Vulcan example. The purported problem can be constructed similarly for the Vulcan example, as can my rejection of the purported problem.
there are two hypotheses, either of which could be dropped. It would then seem arbitrary to accept one hypothesis over another in order to reconcile the theory with the evidence.

There is an important disanalogy here, however. Newton’s theory of planetary motions has, as a central concept, the notion that gravitational force is what determines the orbits of the planets, as it determines the attraction of objects on Earth towards its center. Newton begins Book Three of the *Principia* with the following passage:

In the preceding books I have presented principles of philosophy that are not, however, philosophical but strictly mathematical – that is, those on which the study of philosophy can be based. These principles are the laws and conditions of motions and of forces, which especially relate to philosophy. I have illustrated them with some philosophical scholiums [i.e., scholiums dealing with natural philosophy], treating topics that are general and that seem to be the most fundamental for philosophy, such as the density and resistance of bodies, spaces void of bodies, and the motion of lights and sounds. It still remains for us to exhibit the system of the world from these same principles. [brackets original] (Newton 793)

Newton, shortly thereafter, lists the propositions he holds true concerning the system of the world. They are propositions concerning the forces upon which all objects in the heavens are acted. Proposition 5, theorem 5, states the common thread of all of these propositions quite clearly:

The circumjovial planets [or satellites of Jupiter] gravitate toward Jupiter, the circumsaturnian planets [or satellites of Saturn] gravitate toward Saturn, and the circumsolar [or primary] planets gravitate toward the sun, and *by the force of their gravity* they are always drawn back from rectilinear motions and *kept in curvilinear orbits*. [brackets original, italics mine] (Newton 805)

That is, it is *gravity* and the gravitational forces between planets and celestial objects that keep these objects in their orbits. Gravitational forces are the reason that planets move in the way that they do. Finally, in the preliminary introduction, Newton states the following:
In the first two books I dealt with forces in general, and if they tend toward some center, whether unmoving or moving, I called them centripetal (by a general name), not inquiring into the causes or species of the forces, but considering only their quantities, directions, and effects. In the third book I began to deal with gravity as the force by which the heavenly bodies are kept in their orbits. (Newton 52)

It seems clear that part of the core of Newtonian planetary theory is that there are no non-gravitational forces at work on the planets that would determine their orbits. So, the Newtonian case is different from Maria’s case, because the second hypothesis is actually part of the core of the theory. As such, it is not a candidate for elimination when paring down the auxiliaries to create X. So, the more general version of the Neptune hypothesis is entailed by $T_c + X + E$. Therefore, the Neptune case ought not have the same verdict applied to it as Maria’s H-K case.

My critic might claim that I have just gerrymandered the Neptune example so that it still fulfills the criteria for a non-ad hoc hypothesis acceptance by pushing the hypothesis that there are no mechanical causes for planetary motion into the core of Newtonian planetary theory. However, it is not my decision alone as to what constitutes the core of some theory. There is an independent scientific test for this. The scientific community must think that the removal of this part of the theory causes it to become a different theory than the original, unmodified one. For Newtonian theory to suddenly allow mechanical causes, including, for example, different comets repeatedly hitting a planet to account for all the evidence, would be, I argue, for scientists of the time to claim that the theory being discussed no longer was Newtonian theory. Newton was trying to
deny that many mechanical causes are needed to keep planets in orbit, against Cartesian vortex theory, which claimed precisely that.\textsuperscript{50} As he states the denial:

\begin{quote}
The hypothesis of vortices is pressed with many difficulties. That every planet by a radius drawn to the sun may describe areas proportional to the times of description, the periodic times of the several parts of the vortices should observe the square of their distances from the sun; but that the periodic times of the planets may obtain the 3/2th power of their distances from the sun, the periodic times of the parts of the vortex ought to be as the 3/2th power of their distances. That the smaller vortices may maintain their lesser revolutions about Saturn, Jupiter, and other plantes, and swim quietly and undisturbed In the greater vortex of the sun, the periodic times of the parts of the sun’s vortex should be equal; but the rotation of the sun and planets about their axes, which ought to correspond with the motions of their vortices, recede far from all these proportions. The motions of the comets are exceedingly regular, are governed by the same laws with the motions of the planets, and can by no means be accounted for by the hypothesis of vortices; for comets are carried with very eccentric motions through all parts of the heavens indifferently, with a freedom that is incompatible with the notion of a vortex.

Bodies projected in our air suffer no resistance but from the air. Withdraw the air, as is done in Mr. Boyle’s vacuum, and the resistance ceases; for in this void a bit of fine down and a piece of solid gold descend with equal velocity. And the same argument must apply to the celestial spaces above the earth’s atmosphere; in these spaces, where there is no air to resist their motions, all bodies will move with the greatest freedom; and the planets and comets will constantly pursue their revolutions in orbits given in kind and position, according to the laws above explained[.] (Newton 939-940)
\end{quote}

Clearly, Newton is denying vortex theory and, I argue, he is clearly demonstrating that requiring multiple mechanical causes to maintain the orbit of a given planet would constitute a change to a different theory than that of Newtonian astronomy. Thus, the analogy between the Neptune case and Maria’s cases falls apart.

\textsuperscript{50} See, e.g.:


XXII. Conclusion

I have presented an account of ad hocness that, I argue, correctly captures the essence of ad hocness. Hypotheses accepted in the face of disconfirming evidence are ad hoc if their acceptance is arbitrary. This type of unacceptable hypothesis acceptance should be held in distinction from introducing some hypothesis, as different criteria might well apply for using a hypothesis as a placeholder than for accepting it. The benefits of this account are several fold: it captures the notion that ad hocness is always a vice, which is how the term is used in scientific practice; it does not appeal to the psychology of the individual scientists accepting the hypothesis; it accurately diagnoses canonical examples of ad hocness, given the details of the cases. Not only does it correctly diagnose canonical examples of ad hocness and non ad hocness, but it does so for the right reasons. It captures the arbitrariness of accepting one hypothesis when others might also reconcile the theory with the evidence. It correctly identifies the sorts of situations where a charge of ad hocness can be appropriate and separates these from occasions where a theory is being discarded or the problem is not being resolved. This account also avoids the problems to which other accounts of ad hocness have succumbed because it does not appeal to additional testability and, so, does not fall prey to the Quine-Duhem-Ayer problem. It has been demonstrated that the putative objections discussed in the latter part of this chapter are not actually problems for this account. All of these are good reasons to accept my account of ad hocness.
Works Referenced


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