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Cognitive grounds for choice among equally well confirmed alternative physical theories

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COGNITIVE GROUNDS FOR CHOICE AMONG EQUALLY WELL CONFIRMED ALTERNATIVE PHYSICAL THEORIES

by

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As members of the Final Examination Committee, we certify that we have read the dissertation prepared by Donald Arthur Fawkes

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Final approval and acceptance of this dissertation is contingent upon the candidate's submission of the final copy of the dissertation to the Graduate College.

I hereby certify that I have read this dissertation prepared under my direction and recommend that it be accepted as fulfilling the dissertation requirement.

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SIGNED: Don Fowles

To Siu-Chu

Cold hearted orb that rules the night
Removes the colors from our sight.
Red is grey and yellow white
But we decide which is right,
And which is an illusion.

The Moody Blues
Days of Future Past
(1967)

ACKNOWLEDGEMENTS

Santayana (1967 p84) once noted that a philosopher who is a teacher is more concerned to give a right start than a right conclusion. It has been my tremendous good fortune that my teachers, Henry Byerly, Robert Caldwell, Joseph Cowan, Richard Healey, Keith Lehrer, and Wesley Salmon have given me a right start and much good help along the way. I have no measure for the huge intellectual and moral debt I owe them. As Santayana (1967 p35-36) also once noted that the true philosopher wanders alone like a rhinoceros, I can only hope that my teachers will find my wanderings reasonable; wandering alone, alone a philosopher doth not make.

I wish also to thank my spouse, Siu-Chu, who supports and encourages me, who patiently and cheerfully did the text processing of the many drafts of this dissertation, and who always believes in me, as do I in her. It is to her that this work is dedicated.

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ABSTRACT

Chapter 1 employs an analysis of the ad hoc to argue that there are cognitive grounds (grounds properly and reasonably connected with knowledge) for choice between empirically equivalent alternative physical theories, and that there are cognitive grounds for theory transition, like the transition from ether theory to relativity theory. Chapter 2 examines the indefinitely large class of empirically equivalent alternatives to standard General Relativity Theory (GTR), and argues that there are cognitive grounds that favor standard GTR. Chapter 3 describes two fairly recent attempts to resolve the problem of an indefinitely large class of empirically equivalent alternatives to standard GTR, posed in chapter 2, and argues that these recent attempts fail. Chapter 4 examines and argues for an epistemological principle employed in chapter 1, and then concludes with a summary of some general consequences for the philosophy of science posed by this study.

INTRODUCTION

Before Einstein developed the General Theory of Relativity (GTR), Henri Poincaré argued that *if* some future theorist should develop a theory of physics that included a nonEuclidean geometry as the description of physical space, *that* there would be also an empirically equivalent alternative theory describing physical space in a Euclidean way. The only possible grounds for choice, Poincaré argued, between such empirically equivalent theories would be pragmatic grounds of elegance, simplicity of description, ease of application, *etc*. The GTR is just such a theory with a variable nonEuclidean geometry, and an indefinitely large class of alternative empirically equivalent theories are possible. The alternative theories are said to be empirically equivalent descriptions of the cosmos because each predicts exactly the same observational and experimental consequences. No empirical result can confirm one such theory while disconfirming the others; there is no possible "crucial test." A number of philosophers have addressed this issue. Reichenbach, Carnap, Quine, and Salmon, among others, have offered similar accounts of the status of such alternative theories. Grunbaum has emphasized the relevance of the point that physical space is metrically amorphous. Nagel has emphasized the postulation of universal physical forces. The received view has been that there are no grounds for choosing among such alternative general theories of physics other than pragmatic grounds of elegance, simplicity of description, ease of application, etc. Recently Glymour and Friedman have offered counterarguments against the received view. The project of this work is to describe some historical background, to address the received view and recent responses to it, and to find solutions to these perplexities.

Richard Feynman once said,

> I wanted very much to learn to draw, for a reason that I kept to myself: I wanted to convey an emotion I have about the beauty of the world. It's difficult to describe because it's an emotion. It's analogous to the feeling one has in religion that has to do with a god that controls everything in the whole universe: there's a generality aspect that you feel when you think about how things that appear so different and behave so differently are all run "behind the scenes" by the same organization, the same physical laws. It's an appreciation of the mathematical beauty of nature, of how she works inside; a realization that the phenomena we see result from the complexity of the inner workings between atoms; a feeling of how dramatic and wonderful it is. It's a feeling of awe--of scientific awe--which I felt could be communicated through a drawing to someone who had also had this emotion. It could remind him, for a moment, of this feeling about the glories of the universe. (Feynman 1985 p237-238)

A gentle irony of this passage (which no doubt was not missed by Feynman) is that the passage does express these feelings in words rather well. Now it is on account of similar sentiments that I wished to learn to do philosophy.

Philosophy must be technical and precise because it must stay close to the phenomena of a complex world, but if the present work does not at least on occasion prompt such sentiments for the reader, if instead it seems only a collection of technicalities, then put it away and find instead something interesting. But let me know. Perhaps I can take up drawing.

CHAPTER 1

AD HOC HYPOTHESES

> *...a wheel that can be turned though nothing else moves with it, is not part of the mechanism.*
>
> Wittgenstein
> (1958 p95)

1. A Preliminary Account of the Ad Hoc

In teaching college courses in ethics one occasionally encounters a curious phenomenon. Here is an example. The theory of 'psychological egoism,' viz., the view that 'all persons always act in such a way as to acquire or achieve what they believe is in their own self interest,' is presented. As soon as this view is introduced a sizeable portion of the class immediately is prepared to assert that it is true. Other students disagree, and begin producing accounts of human actions to disconfirm the theory. They produce stories of parents who save their children from comparatively minor harm at great risk to themselves; of complete strangers who save people from burning buildings at their own peril; of soldiers in war who drop to the ground and cover a "live" grenade with their bodies, saving their fellows; and so on. Yet these disconfirmations have a curious effect on the "true believers"; they seemingly believe psychological egoism more strongly than before. For, they say that the parents know that the psychological pain of knowing that their child has suffered even a minor harm, which the parents might have prevented, is greater than the physical pain the parents risk in the act of prevention; they say that the stranger who rushes into the burning building knows of various social rewards, and wants those rewards more than to avoid the risk of injury; they say that the soldier always wanted to be a tragic hero, or had suicidal tendencies, and saw this as likely his only chance... and so on. From here discussion predictably proceeds to the plausibility of the accounts on both sides. Experience teaches that discussion proceeds from here without resolution. Lines of disagreement have been drawn, and each side simply continues to attack the other.

But there is a way to resolve this sort of dispute and, so far, I have found it to work in every case. At least this is what my students tell me. At some point in the discussion I say, "Let me tell you a (probably apocryphal) story about Galileo." Briefly, the story goes like this: Galileo invented a telescope and looked at the moon. To his surprise he observed mountains and valleys and craters on the surface of the moon. This was a surprise because the accepted theory of the time held that the surface of any extraterrestrial body _must_ be perfectly uniform, smooth, and spherical. (This belief was connected with theology and with Aristotle's works.) When the results of his observations became known, Galileo came under attack. It was said that he had gone mad (had become a lunatic!); that he had placed false images inside his telescope; that he was using a devil's instrument (a charge to be taken quite seriously at the time)... and so on. But finally, a learned scholar of the time hit upon a way to resolve the issue: "Galileo's observations are quite right. But the previously accepted theory is true as well, for there is an invisible (or undetectable) substance which covers the surface of the moon from the top of the highest mountain to the bottom of the lowest crater." Galileo's response on hearing this account is said to have been, "I agree with my learned colleague with the exception of one minor detail: I hold that the invisible substance on the surface of the moon is twice as thick as he says it is."[1]

Now in the present context the points of this story to which I want to draw attention are the obvious one that intellectual progress sometimes takes place at the expense of an accepted theory, but more importantly, the less obvious one that it is in fact the case that we have

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[1] I have this story from Professor James M. Smith, of California State University at Fresno. He has it from a colleague, who has it from a lecturer he heard at Harvard more than twenty years ago. I have no idea whether it is true, and I have been, as yet, unable to find it in print. For all I know, it is an oral tradition going back to Galileo. But most of the points of the story (and surely those addressed here) are the same, whether it is fact or fiction.

identified a cognitive liability of an accepted theory if we show that it includes an ad hoc hypothesis[2] even if there is no alternative competing theory at the time. Galileo did not offer an alternative theory at the time of his report. In order to support more fully this less obvious claim I must (i) indicate the various accounts of the ad hoc and (ii) establish the thesis of this chapter which I will state below in Section 2. But for now let me just state that the general notion of an ad hoc hypothesis is one which is added to a theory only in order to "save" the theory or some part of it from some problem, and that when I speak of a cognitive liability or a deficiency or a pejorative point, these do not refer to elegance, simplicity, explanatory power, etc., but instead they refer to a reason to reject a theory, a reason to think that the theory is incorrect. A complete account of liabilities for theories is beyond the scope of this project.

2. A Taxonomy of the Ad Hoc

A review of recent literature on ad hocness reveals, I believe, the following taxonomy:

a. Testability accounts:

(1) Falsificationist

(2) Inductivist

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[2] I.e. if we show this on the pejorative accounts as argued below.

b. Heuristic account

c. Derivability account

'Ad hoc' is a predicate applied to both theories and hypotheses. Theories are ad hoc if they contain an ad hoc hypothesis. Hypotheses are ad hoc in the senses labeled above and described as follows. The term 'testability' is meant to indicate that the falsificationist and inductivist accounts share a common ground. That common ground is the overriding importance they place on testable consequences. Testable consequences of an hypothesis are those of the hypothesis in the context of (in conjunction with) the amended theory; hypotheses rarely, if ever, have testable consequences on their own. An hypothesis is ad hoc on the falsificationist account (i) if it is an added auxiliary hypothesis which has no testable consequences in the context of the revised theory, or (ii) if it has no testable consequences other than its prediction in the context of the revised theory of the kind of result that motivated its formulation in the first place. (Popper 1935, Zahar 1973I p96-97) The inductivist account accepts the falsificationist account as a "core" but interprets this to mean that such an ad hoc hypothesis cannot be confirmed as well as not falsified.[3] One inductivist approach to dealing

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[3] Zahar 1973I p96-98. Zahar cites Born 1962, Kompaneyets 1962 and Reichenbach 1958.

with <u>ad hocness</u> among alternative theories is through the introduction of Bayes' Theorem. Bayes' Theorem here is intended to model rational decision-making in cases of differences in degree.[4] I shall have little further to say about the falsificationist and the inductivist accounts, and I shall simply assume that if science is to be empirical, then the testability accounts of <u>ad hocness</u> must identify cognitive liabilities.

It is with regard to the 'heuristic' and 'derivability' accounts that I have the most to say. An hypothesis is <u>ad hoc</u> on the <u>heuristic</u> account if "it does not accord with the spirit of the heuristic of the [research] programme."[5] The

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[4] Schaffner 1974 p45-78. A Bayesean inductivist position is yet more complicated. The following paragraph from p72-73 is especially revealing:

> There are two types of considerations which are different from direct empirical support which significantly affect a theory or hypothesis. These represent (i) The effect which other well confirmed (or corroborated) theories have on a particular theory in question, and (ii) the simplicity which a theory possesses relative to its competitors.... [A] new hypothesis... will be judged as more or less acceptable depending on (i) the theoretical accord or theoretical discord which it introduces, and on (ii) the simplicity of its entities and logical form. If intertheoretic accord/discord and simplicity can be quantified, as I believe they can, then we possess a means of judging the seriousness of an <u>ad hoc</u> hypothesis. This becomes particularly important if all competing theories which are attempting to explain a set of experiments are thought to be <u>ad hoc</u> in certain respects.

[5] Zahar 1973I p101. See also Lakatos 1970 p175 n2&3.

basic idea is that a research program contains "a heuristic which consists of a set of suggestions and hints which govern the construction or modification of the auxiliary hypotheses." (Zahar 1973I p100) An hypothesis, then, is heuristic-ad-hoc if it is inconsistent with these suggestions or hints. Finally, an hypothesis is ad hoc on the derivability account if it does not in some sense "follow naturally from" the revised theory. (Miller 1974 p42, Leibowitz 1979 p83) Briefly then, we can say that the center of the heuristic notion of ad hocness has to do with a lack of consistency with a heuristic[6]; while the center of the derivability notion has to do with derivability from theory. It is also worth noting that it is possible to

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[6] Examples of possible heuristics are the following:
- There [are/are not] instantaneous-action-at-a-distance-forces.
- Forces [are/are not] mechanical.
- Matter [is composed of discrete units/is continuous].
- Forces [decrease/increase/otherwise vary/do not vary] with distance.
- Forces [are/are not] transmitted [through/by] a medium (e.g. the Stoic's nouma, e.g. Lorentz's ether, e.g. electromagnetic and gravitational fields).
- Alternative kinds of forces transmitting through a medium [transform/do not transform] in [the same/similar] way[s].
- The cosmos [is/is not] completely constituted of [continuous and/or discrete] [matter and/or energy and/or fields].
- [Matter and/or energy and/or fields] [can and/or does/do] convert into each other [under/not under] conservation laws.

(Compare Leibowitz 1979 p97.)

elaborate more fully the possible conceptions here by pointing-out that we can speak of consistency with, or derivability from a theory; or, consistency with, or derivability from a heuristic; and, of course, derivability entails consistency but not vice versa, and inconsistency entails nonderivability but not vice versa. But note that no rational theorist would propose to preserve a theory by offering an auxiliary hypothesis inconsistent with the theory. Note also that the heuristic notion of the ad hoc includes both lack of derivability from, and lack of consistency with a heuristic, because lack of consistency entails lack of derivability. Hence, as the discussion proceeds I will follow the usages of the literature, so that the derivability account of the ad hoc has to do with a lack of derivability from theory, and the heuristic account of the ad hoc has to do with a lack of consistency with a heuristic. Finally, regarding the heuristic account, the arguments presented here are intended to function even if the heuristic account of the ad hoc is interpreted only as a lack of derivability from a heuristic, though again, I will stay with the usage from the literature.

In examining the various notions of the ad hoc it will be helpful to introduce an example concerning ether theories which will be developed more fully below.

> A very common criticism of ether theories is that they are ad hoc and that Lorentz's in particular

> is so.... The claim that ether theories are ad hoc usually is made in connection with discussion of how the Michelson-Morley interferometer experiment is to be explained. It is claimed that Lorentz's electron theory could not account for the null result of the Michelson-Morley experiment without appending to the original electron theory hypotheses which are ad hoc, and therefore unacceptable. (Leibowitz 1979 p75-76)

In particular, the Lorentz-Fitzgerald contraction hypothesis usually is considered to be ad hoc. When Minkowski criticized the Lorentz-Fitzgerald contraction hypothesis (LFC) he appeared to do so with the derivability account of ad hocness in mind. For he says that

> the contraction is not to be looked upon as a consequence of resistances in the ether, or anything of that kind, but simply as a gift from above -- as an accompanying circumstance of the circumstance of motion. (Minkowski 1907 in Perrett & Jeffery 1952 p81)

In the remainder of this chapter I will argue that neither the heuristic account nor the derivability account of ad hocness identify a cognitive liability, but that there is an interesting type of ad hocness, not previously noticed, which does identify such a liability; I will term this the ontological[7] account. In order to do this, the

[7] A review of the entries in several dictionaries of philosophy for 'ontology' and for 'ontological' reveals a number of usages. The senses employed here can be shown by the following passages from the Flew and Lacey volumes. In Flew we find in one entry for ontology, "The assumptions about existence underlying any conceptual scheme or any theory or any system of ideas." In Lacey there is, "A particular theory about what exists, or a list of existents, can be called an ontology." The sense of 'ontology' employed here is that the ontology of a physical theory is

remaining sections of this chapter will address the following issues: Section 3 will describe some of the results of Flora Leibowitz' dissertation, Aspects of Ether Theory (1979). Section 4 will further explicate 'derivability'; show a kind of two-sided consequence for the Lorentz ether theory and the derivability account of the ad-hoc; define and apply the ontological account of the ad hoc; explore testability; and, produce a criticism of both the derivability account and at least some cases of the heuristic account of ad hocness. Section 5 will give another kind of critique of the heuristic account of ad hocness, and Section 6 will suggest a revised taxonomy, and tabulate some examples.

3. Problems For the Derivability Sense

Leibowitz points out that the LFC is criticized as being ad hoc on the derivability account. She also points out that it is a commonplace among physicists to criticize the Schroedinger equation for quantum mechanics by

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the individuals, properties, and relations in the physical system which is a model of the theory. Ontological claims may be direct or implicit. A direct ontological claim is a claim about the ontology of the theory. An implicit ontological claim presupposes a direct ontological claim; so, for example, if it is claimed that the physical resistance of the ether accounts for observations of length contraction, this is an implicit ontological claim which presupposes the direct ontological claim that there is an ether which is part of the physical world. These are the senses of 'ontology' and 'ontological claim' that are employed in this work. For these and other senses see, Flew 1979, Lacey 1976, and Runes 1962.

comparison to the Dirac equation, because terms for certain known states of the hydrogen atom (i.e. the "stationary" or "nonradiating" states) must be appended as independent hypotheses to the Schroedinger equation, but can be derived from the Dirac equation. These equations, nevertheless, are said to be "observationally equivalent" in nuclear contexts.[8] Yet Leibowitz goes on to show that two accepted laws in Quantum Mechanics, viz., conservation of baryons and conservation of strangeness are ad hoc on the derivability

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[8] Giere 1988 p179-226. Giere recounts recent attempts to find and test observable differences between the Schroedinger and the Dirac equations in nuclear physics. Giere also notes (p223-224) that the charge of being ad hoc, on what is called here the testability account has been raised against reformulations of the Schroedinger approach. But as Giere carefully notes early in his discussion (p184-185), "No nuclear physicist seems to doubt that, in principle, the correct model of the nucleus would be a relativistic model based on the Dirac equation. The issue is whether interactions at energies typical of research in nuclear physics [by contrast to high energy particle physics] involve fundamental relativistic processes to an extent great enough to detect experimentally. If not, the research can proceed happily using nonrelativistic [Schroedinger] models." [my edit] This is like pointing out that we do not need to use general relativistic calculations in sending a spaceship to the moon. Dirac models are confirmed and Schroedinger models are not confirmed by experiments in high energy particle physics. So, whatever may be the ramifications of testability-ad-hoc criticisms in this context, the correctness of the theories at hand is not at issue. For the purposes of the present work an account of Leibowitz' discussion of derivability will be more useful than further pursuit of this sense of "observational equivalence" which is not connected with assessing the correctness of alternative theories.

account.[9]

Leibowitz' findings regarding conservation of baryons and of strangeness may be summarized as follows. Baryon numbers and the principle of conservation of baryons were introduced because conservation of energy and of electric charge are not sufficient to provide a prediction that certain decays which are never observed will not occur. For example, a proton does not decay into a positron and a gamma ray. Baryon numbers are assigned to nucleons, and the failure of certain decays to occur is explained on the ground that there would be a "baryon imbalance" (a violation of baryon conservation) if they did occur. The principle involved to account for this is one whose sole function is to explain the failure to observe such decays. (Frauenfelder & Henley 1974 p163) Thus the conservation of baryons is *ad hoc* in the derivability sense.

The conservation of strangeness presents a similar situation. There remained a number of decays which were not observed, though their occurrence would not violate any of the conservation laws including the conservation of baryons. (Marion 1971 p626) The principle proposed by Gell-Mann (1953), and independently by Nakano and Nishijima (1953), the conservation of strangeness, was another

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[9] Leibowitz 1979 p107-126. The summary which follows also is based partially upon private communications with Leibowitz.

conservation principle, one which the occurrence of these decays would violate. However, just as in the case of baryon conservation, there was not, at the time the conservation principle was proposed, an antecedently accepted principle of quantum mechanics from which it followed. Nakano and Nishijima did support conservation of strangeness on the basis of empirical findings and the conservation of baryons. (1953 p581) However, the use of the conservation of baryons to support the conservation of strangeness as derivable (and hence not derivable-*ad*-*hoc*) obviously cannot be maintained: The reason for formulating strangeness conservation was the failure of the conservation of baryons (and of energy and of electric charge) to rule out certain decays which do not occur; and, conservation of baryons is itself derivable-*ad*-*hoc*. In these circumstances, we must choose between dropping the derivability-*ad*-*hoc* criticism of the LFC and of Schroedinger Quantum Mechanics, *or* of criticizing the laws of conservation of baryons and of strangeness as derivable-*ad*-*hoc*.

Though it would go too far afield to deal much further with these issues regarding quantum mechanics, I do wish to offer the following speculations: What is the motivation in quantum mechanics for the positing of laws that predict that we will *not* get some things that we don't seem to get, at least so far? That is, the fact that we never observe certain decays is compatible with the accepted body of theory (without conservation of baryons and of strangeness), but not predicted by it. Now just why in these circumstances do we want laws to predict that what we have not observed to happen will not happen? In other words, what is the point of the principles of conservation of baryons and of strangeness? Perhaps an analogy will help. Suppose that there were no albinos, and that the laws of genetics were consistent both with there being some albinos and with there being no albinos. Just what would be the point of searching for genetic laws (by contrast to specific conditions) to predict that there will be no albinos? Just what is it that needs to be explained? Contrast with this a case in which there are, say, no human albinos, but there are albinos of other species.

I can think of five considerations relevant to this issue. First, conservation of baryons and of strangeness may be postulated in a context more like the no-human-albino-case, than the no-albino-case. There does seem to be a difference to be explained in the former kind of context. Second, in either sort of context such laws may be connectable in interesting ways to other parts of the theory or related theories. Third, the fact that certain events do not happen may be a consequence not of any prohibiting law, but rather a consequence of de facto boundary conditions in conjunction with permitting laws. Fourth, conservation of baryons and of strangeness are not posed in response to a disconfirmation nor in response to a conceptual problem in accepted theory. Rather they are apparent responses to a perceived need to improve the scope, the predictive power, of accepted theory. Finally, in Gell-Mann's rationale offered in support of the conservation of strangeness there is the suggestion that it may be viewed as a kind of generalized Pauli principle,[10] i.e. the Pauli exclusion principle according to which no two electrons of an atom can have the same set of quantum numbers. Now the Pauli exclusion principle is a well accepted and non-pejoratively-ad-hoc[11] principle of quantum mechanics. So it may be possible to use this suggestion to form an argument in support of the conclusion that conservation of strangeness is derivable from accepted theory and hence not derivable-ad-hoc. Such an argument even may be possible for conservation of baryons. But what does seem clear is that Leibowitz has posed at least a prima-facie serious problem regarding the cognitive status of the derivability account of the ad hoc, and that if conservation of baryons and of strangeness are derivable-ad-hoc, it is highly questionable

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[10] Gell-Mann 1953 p834. This was first pointed out to me by Leibowitz in private communication.

[11] See the further discussion of this point below in section 4.

that this is a cognitive liability.

4. Further Analysis of the Ad Hoc

Although this issue has not been addressed explicitly in the literature, in order to develop further criticism of the derivability account of the *ad hoc* it will be useful to note that 'derivability' may mean *any* of the following:

- *a*. Deducibility from the "saved" theory, or from some part of it.
- *b*. A conclusion reached inductively (*e.g*. by argument from analogy) from the "saved" theory.
- *c*. A conclusion reached deductively or inductively from other theoretical considerations relevant to the domain, or from such other theoretical considerations in conjunction with the "saved" theory, or some part of it.[12]

This approach seems to capture the idea of following naturally from a theory and it also seems to capture Minkowski's intuition that there must be *some* rationale for an hypothesis other than merely preserving a theory, and that it is natural to look to the theory for such a rationale or to look to other relevant theoretical considerations. As we will see below from Zahar, Lorentz apparently did have such a rationale. But before moving on it is important to note that although the account above (*a*, *b*, *c*) does capture the appropriate senses of derivability, it would be a mistake to employ sense *a* in a *derivability-*

[12] This analysis of derivability results in part from private communications with Leibowitz. See also, Scriven 1976 p33-34.

ad-hoc criticism of any theory. For to do so amounts to holding that any logically independent hypothesis in any theory is a cognitive liability, and this obviously trivializes the concept as a criticism of any theory; it makes it far too strong. This means that an argument that an hypothesis is derivable-ad-hoc must be couched in senses b and/or c; while, an argument against such a claim may be expressed in any of the three senses. Derivability may mean a and/or b and/or c. Derivable-ad-hoc only may claim that the hypothesis at hand is not derivable in senses b and/or c. In the end it will be argued, however, that points about derivability-ad-hoc do not have pejorative consequences.

Now I understand the following quotation from Zahar's 1973I paper as the rhetorical statement of an inductive argument at least tacitly held by Lorentz. Prior to this passage Zahar shows that Lorentz had derived the Lorentz-Fitzgerald Contraction (LFC) from the molecular forces hypothesis (MFH), and hence that the LFC was not postulated solely on the basis of saving the ether theory from disconfirmation by the results of the Michelson-Morley experiment. Here is the passage:

(A) The MFH is therefore non [ad hoc][13] within

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[13] Zahar uses "ad hoc_3" here; ad hoc_3 is Zahar's "heuristic" sense of ad hoc. For a justification of my use of it in the context of the "derivability" account see the discussion to follow in this section. Also, see the discussion of the "heuristic" account in section 5 below.

> the ether programme, whose heuristic requires that physical phenomena be explained in terms of contiguous actions through the medium. Molecular forces determining the shape of a given body are transmitted by the same medium as the electromagnetic field; since both types of force are states of the same substratum, why should they not behave and transform in the same way? (Zahar 1973I p116)

This is a case of derivation in sense b above; and it argues that the LFC is not ad hoc in the derivability sense. My view is that the heuristic of a physical theory is implicit in the theory, rather than that the theory and heuristic form separate parts of a research program.[14] My rationale for this is that a physical theory applied to a physical system that models it includes ontological commitments regarding the physical system, and that such commitments at least restrict, if they do not specify, the possible kinds of heuristic. And by 'heuristic,' I intend what Zahar intends, i.e. "...a set of suggestions and hints which govern the construction or modification of the auxiliary hypotheses." (Zahar 1973I p100) This view may seem to be merely terminological, but I have given a rationale, and it is faithful to Zahar's reconstruction of Lorentz's progress, and it does have this consequence: There is an intersection of the "heuristic" and "derivability" accounts. The example (A) above falls within this intersection. From that example and the example to follow (B) (also within the

[14] This differs from Zahar's view (1973I p99-101).

intersection), I want (i) to display the two-sided consequence for the Lorentz ether theory and the derivability account of the ad hoc; (ii) to suggest a pejorative account of the ad hoc (the ontological account); and, (iii) to suggest how we should respond when faced with choosing between abandoning the derivability-ad-hoc criticism of the Lorentz ether theory and Schroedinger Quantum Mechanics, or criticizing as derivable-ad-hoc the conservation of baryons and of strangeness.

We are still dealing with the Lorentz ether theory and Zahar tells us:

> (B) Lorentz, unlike Einstein, did not create the heuristic of his own programme. The heuristic of Lorentz's programme consisted in endowing the ether with such properties as would explain the behavior both of the electromagnetic field and of as many other physical phenomena as possible. In view of the overwhelming success of Newtonian dynamics it is hardly surprising that the ether was supposed to possess primarily mechanical properties. *The ether programme developed rapidly in certain respects, yet towards the end of the nineteenth century its positive heuristic was running out of steam.* A succession of mechanical models for the ether were proposed and abandoned. One serious difficulty was the presence in these models of longitudinal as well as transversal waves. Lorentz faced a daunting problem of a different sort: In order to explain certain electromagnetic phenomena he postulated an ether at rest. He considered a portion of the ether, calculated the resultant R [vector] of the Maxwellian stresses acting on its surface and found that the R [vector] is generally non-zero. Hence, if he was to assume that the ether was anything like an ordinary substance, he would have also to suppose that it was in constant motion. But this contradicted his original assumption of an ether at rest. He concluded 'that the ether is

undoubtedly widely different from all ordinary matter' and that 'we may make the assumption that this medium, which is the receptacle of electromagnetic energy and the vehicle for many and perhaps for all the forces acting on ponderable matter, is, by its very nature, never put in motion, that it has neither velocity nor acceleration, so that we have no reason to speak of its mass or of forces that are applied to it.' In other words Lorentz had reached a point where the behavior of the electromagnetic field dictated what properties the ether ought to have, no matter how implausible these properties might be: for example, the ether was to be both motionless and acted upon by non-zero net forces. The ether was nothing but the carrier of the field. *This involved a reversal of the heuristic of Lorentz's programme: Instead of learning something about the field from a general theory of the ether, he could only get at the ether post hoc by way of the field*. In the case of the MFH, for example, Lorentz *first* studied the transformational properties of the electromagnetic field; only *then* did he extend these properties to other molecular forces. Instead of positing *one medium* endowed with certain properties from which all forces inherit some *common* characteristic, we have an electromagnetic field acting as the archetype which determines the respects in which all forces are similar.[15] [Zahar's emphases]

From passages (A) and (B) and from the analysis provided thus far it is possible to support (i) the two-sided consequence for derivability-ad-hoc, (ii) the ontological account of the ad hoc, and (iii) the non-pejorative nature of the derivability account of the ad hoc as follows:

(i) The two-sided consequence for derivability-ad-hoc. Lorentz's decision as quoted in (B) is a derivation in

[15] Zahar 1973II p242-243. Zahar's quotations from Lorentz may be found in Lorentz 1909 p30.

senses a, b and/or c,[16] so it is not derivable-ad-hoc. But the two-sided consequence is that in both of our examples (i.e. in quotations (A) & (B) above) the derivation requirement has been met, and yet in this latest case (B), it has gone too far. For now we must either adopt an apparent contradiction, viz., that the ether is both in motion and motionless at the same places and times, or recognize the ether concept as vacuous, i.e. as having no physical properties of its own.[17] The latter is Lorentz's

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[16] I say, "a, b and/or c" here because I agree with Scriven concerning "converting" arguments from deductive to inductive and from inductive to deductive. (See Scriven 1976 p33-34.) I said only b in the earlier example because that seems to be the most plausible reconstruction from the rhetorical form. But in the present case reconstruction in any of the senses seems plausible given (i) possible alternative views about what is in the theory and what is in other relevant theoretical considerations, and (ii) the "Scriven-conversion-position." However, for those who may disagree with the "conversion-position," this generates no problem for the point at hand, since the sense of derivable-ad-hoc is b or c. And again, Lorentz's decision is not derivable-ad-hoc so long as it may be derived in sense a, or b, or c.

[17] Zahar suggests (1973II p243) that the ether theory might still not be "beyond redemption" since it might be "saved" by postulating some "non-mechanical properties" of the ether to account for both the "electromagnetic phenomena and for molecular interactions." However, (i) in the context of competing alternative theories such a postulation would need to avoid being ad hoc on both the testing and the ontological (introduced below) accounts -- an unlikely prospect; and, (ii) this would seem to be counter to the dynamical heuristic of the program that Zahar describes. This latter point just raises the issue of what to count as the heuristic. It might be thought that this should just remain "fuzzy." But if we take that route here, the result is that any claim about consistency or derivability concerning the heuristic can be eliminated by an adjustment

choice and it amounts to nothing more than the ontological commitment that the field must exist in something and we call that something the ether. We must ask here, what is the difference between this something and nothing?

(ii) Ontological ad hocness. These considerations suggest an account of the ad hoc which is pejorative and which is distinct from the falsificationist, inductivist, heuristic and derivability accounts. Such an hypothesis is ad hoc in that it is posited solely with the function of preserving the ontology of a theory. I call this the ontological account and define it as follows:

(I) It is an added auxiliary hypothesis which revises the theory such that it allows the retention of a direct or implicit ontological claim[18] of the retained theory despite a disconfirmation, or despite a conceptual problem derived from the retained theory or from the conjunction of the retained theory and other theoretical considerations relevant to the domain. (An inconsistency is a paradigm case of a conceptual problem.) And,

(II) This hypothesis is vacuous, not in the sense

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in the extension of "heuristic." So, if we say that a non-mechanical auxiliary violates the dynamical heuristic of the classical program, it cannot be replied that this "redemption" is alright because the ether is in the heuristic but the mechanical is out, unless we are told why. However, if we are told why, that is an argument about the extension of the heuristic.

[18] See footnote 7, above.

of having no testable consequences (though it may be vacuous in that sense), but in the sense that in the revised theory it plays no role, has no function except to eliminate the disconfirmation or the derived conceptual problem.

This kind of ad hocness is pejorative, if science is to remain coherent and empirical; whether this account provides a sufficient ground for rejecting a theory containing such an hypothesis is another question. The answer to that question, I believe, depends upon comparative merits of alternative theories. The ontological account may be viewed as an application to science of the general epistemological principle that it is unreasonable to accept a claim without some good reason to accept it. A claim's serving to preserve the ontology of a theory despite conceptual problems or disconfirmation is not a good reason to accept the claim, as the examples above support. We will return to the general epistemological principle in Chapter 4, but for now suffice it to say that the ontological account of the ad hoc applies the principle to physical claims of physical theories; it applies the principle to claims about the world of scientific, descriptive, empirical theories. On this analysis the LFC and Lorentz's conclusions regarding the ether are ontologically ad hoc hypotheses, and these are cognitive liabilities: Having reached the point that the ether is conceived as both in motion and motionless at the same places and times, the ether theory is ontologically-ad-hoc not in the sense of disconfirmation but conceptually in the sense of logical inconsistency. Having reached the point that the ether theory cannot be expressed so as to remove the troublesome longitudinal waves, the ether theory is again ontologically-ad-hoc not in the sense of disconfirmation but in the sense of what might be called physical inconsistency or physical impossibility. The ether is simply assumed not to have any longitudinal waves, though there is no physically consistent way to describe such an ether. Both of these are conceptual problems. In the sense that the LFC removes the possibility of disconfirmation of the ether theory provided by the Michelson-Morley experiment, the LFC is ontologically-ad-hoc as well. In each case it is the ontology of the ether theory which is being preserved.

At this point it is appropriate to ask whether the LFC is ad hoc on the testability account. Does the Michelson-Morley experiment exhaust the opportunities for falsification or confirmation/disconfirmation of the ether theory with LFC? The historical record indicates that other testing opportunities were available after the formulation of the LFC and so the LFC is not testable-ad-hoc. As Grunbaum has noted,

> It is evident that it is logically possible for a Kennedy-Thorndike type of experiment to *confirm*

the quantitative predictions of the Lorentz-Fitzgerald hypothesis as against those of the original aether theory independently of the Michelson-Morley experiment.

Furthermore, it is a matter of *empirical* fact that the Kennedy-Thorndike experiment of 1932 did not yield a shift in the interference fringes corresponding to the time difference (variation) deduced from the Lorentz-Fitzgerald hypothesis. In fact, just like the Michelson-Morley experiment, the Kennedy-Thorndike experiment has a negative outcome in the sense that there were no fringe shifts. Thus, one is entitled to claim that the Kennedy-Thorndike experiment failed to produce the kind of positive effect whose occurrence would have served to *confirm* the Lorentz-Fitzgerald hypothesis. But it would be an *error* to suppose that the *non*-obtaining of this particular kind of confirmation suffices to prove that the Lorentz-Fitzgerald hypothesis was *falsified* by the null result of the Kennedy-Thorndike experiment! For...the adjunction of the *further* auxiliary hypothesis of time dilation to the Lorentz-Fitzgerald hypothesis does enable the thus *doubly* amended aether theory to explain the null outcome of the Kennedy-Thorndike experiment while upholding the Lorentz-Fitzgerald hypothesis.... [T]he justification for rejecting the Lorentz-Fitzgerald hypothesis along with the doubly amended aether theory depends on having philosophical reasons for accepting Einstein's rival theory of special relativity to the exclusion of the doubly amended aether theory.[19]

Further, although Karl Popper had earlier held that,

An example of an unsatisfactory auxiliary hypothesis would be the contraction Hypothesis of Fitzgerald and Lorentz which had no falsifiable consequences but merely served to restore the agreement between theory and experiment--mainly the findings of Michelson and Morely,

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[19] Grunbaum 1964 p1411. For Lorentz's own account of the testable consequences of the doubly amended ether theory (with an account of the MFH and especially its application to the electron) see his 1904 in Perrett & Jeffery 1952 p28-34.

Zahar points out that Popper later accepted Grunbaum's argument to the contrary.[20]

In addition, Grunbaum continues as follows:

> Moreover, purely mathematically the doubly amended variant of the aether theory permits the deduction of the Lorentz transformation equations no less than does Einstein's special theory of relativity. And this aether-theoretic deducibility of the Lorentz transformations now permits us to see that even the *conjunction* of the Lorentz-Fitzgerald hypothesis with the assumption of the time dilation is not [testable] ad hoc. That the latter conjunction of auxiliary hypotheses is indeed testable in a kind of experiment which is independent of *both* the Michelson-Morley and Kennedy-Thorndike types is shown by the example of the so-called 'quadratic' optical Doppler effect as follows: Being *mathematically* identical with the space and time transformations of the special theory of relativity, the Lorentz transformations of the doubly amended aether theory entail an optical Doppler effect which is *quantitatively different* from the one that is deducible from the original aether theory. Hence, the rejection of the doubly amended aether theory cannot be justified by claiming that the conjunction of its two auxiliary hypotheses is [testable] ad hoc.... (Grunbaum 1964 p1413-1414)

This account follows Lorentz's own (1904) suggestions regarding testable consequences. Testability was never at issue.[21]

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[20] See Zahar 1973I p98-99&104-105. Popper's earlier position is in Popper's 1935 section 20; Popper's later position is in Popper's 1969 p51.

[21] For Einstein's acknowledgement of this point on testability and his own predictions of the same results based on the Special Theory of Relativity see his [1916, 1952] 1961 p49-54; on this same point see also the famous 1905 paper in Perrett & Jeffery 1952 p55-65. And again Lorentz's own support of this point is in his 1904 in Perrett & Jeffery 1952 p28-34.

The ether theory with LFC posits a "length contraction" of $\ell \cdot \sqrt{1-(v^2/c^2)}$ [where ℓ is the length of a measuring rod at rest relative to the ether, c is the constant velocity of light relative to the ether, and v is the velocity of the rod along length ℓ relative to the ether]. The ether theory with LFC and "time dilation" adds the equation $t/\sqrt{1-(v^2/c^2)}$ [where t is the time between ticks of a clock at rest relative to the ether and v is the velocity of that clock or a clock of identical construction relative to the ether]. Thus the ether theory with LFC and time dilation predicts a negative result to the Kennedy-Thorndike experiment and a negative result to any generalized round trip light experiment. (Sklar 1977 p244-251) Yet these very same equations explain the same results within Einstein's Special Theory of Relativity (STR).

Some of the philosophical reasons that favor the STR over the doubly amended ether theory (DAE) may be described as follows. The DAE explains the null results of any round trip light experiment on the <u>dynamical</u> basis of physical forces acting through an ether medium and the constancy of the speed of light in that medium. The STR explains these null results on the <u>kinematic</u> basis that the null results are a direct consequence of relative, uniform, rectilinear motions of reference frames and the constancy of the speed of light in any such reference frame. So, the ether and the physical forces of the DAE (and absolute space and absolute time as well) are seen to be ontologically superfluous.[22] The STR is favored ontologically because it "does not multiply entities (forces and mediums) beyond necessity."[23] It is worth noting also that, under the definition given above of the ontological account of the ad hoc, the conjunction of the LFC and time dilation is ontologically-ad-hoc in the DAE.

We are now in a position to inquire whether there is some other testable difference between the DAE and the STR. It turns out that there is a testable difference, and this difference will help to shed light on the notion of function in a theory employed in the definition of the ontological

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[22] Einstein 1905 in Perrett & Jeffery 1952 p38. It is worth noting that here Einstein says, "The introduction of a 'luminiferous ether' will prove to be superfluous inasmuch as the view here to be developed will not require an 'absolutely stationary space' provided with special properties, nor assign a velocity-vector to a point of the empty space in which electromagnetic processes take place." [My emphasis.] I here emphasize the dynamically superfluous nature of the ether. The kinematically superfluous nature of absolute space and time is also contained in the quotation and may be expressed thus: The STR "does not multiply entities (absolute space, absolute time) beyond necessity." See also Einstein [1916, 1952] 1961 p50-54.

[23] Although this expression of a principle of parsimony is often referred to as "Ockham's razor," Ernest Moody, writing in The Encyclopedia of Philosophy says that William of Ockham "seems not to have used the formulation 'Entities are not to be multiplied without necessity.'" Moody attributes to William of Ockham the formulations "Plurality is not to be assumed without necessity" and "What can be done with fewer [assumptions] is done in vain with more." (The Encyclopedia of Philosophy, Vol.8, 1967 p307.)

account of the ad hoc. Lorentz, in presenting the (1904) DAE does not apply the LFC and time dilation to the force equation of mechanics; but, Einstein in presenting the (1905) STR does make this application. This application leads directly to Einstein's famous mass-energy equivalence, and that equivalence provides a testable difference between the STR and the DAE. However, there is nothing in Lorentz's (1904) DAE that prevents this application as well. Suppose this application to be made. We now have a tri-amended-ether-theory (TAE) which is empirically equivalent to the STR, i.e. it predicts all and only the same observations as the STR. Interestingly, in the TAE the LFC and time dilation are no longer ontologically-ad-hoc. For these hypotheses now function in the TAE to provide an application to mechanics that predicts ontological[24] and observable phenomena not predicted by Newtonian Mechanics, and of course, these predictions are exactly the same as those of the STR. Hence, in the TAE the LFC and time dilation no longer serve the sole function of preserving the ontology of the ether theory, and so they are not ontologically-ad-hoc in the TAE. Unfortunately for the TAE, this result does not affect the status of the other ontologically-ad-hoc problems for the TAE (the "longitudinal waves" and the "ether in motion and not in motion" problems), nor does this result

[24] See again footnote 7.

alter the ontological superfluity of the ether and of the supposed forces acting through it; nor does this result alter the ontological superfluity of absolute space and absolute time. But this result does serve to illustrate the notion of function in a theory employed in the definition of ontological-ad-hoc, and it does serve to illustrate how an hypothesis might recover from being ontologically-ad-hoc. This result also serves to illustrate the ontological nature[25] of many of the claims of physical theory; both the TAE and the STR make implicit or direct ontological claims. Finally, it is only when the STR is shown to be a special case of Einstein's general field theory, the General Theory of Relativity (GTR), that the GTR makes predictions (advance of the perihelion of the planet Mercury, deflection of light by a gravitational field, red shift of spectral lines of an element at the surface of a star compared with those of that same element at the surface of the earth)[26] that are testable beyond (and contrary to) the TAE (conjoined with a TAE-modified Newtonian Mechanics). Here it is worth noting that the ontological account of the ad hoc identifies

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[25] Again, see footnote 7.

[26] Einstein 1916 in Perrett & Jeffery 1952 p160-164. See also, Einstein [1916, 1952] 1961 p123-132. For an earlier exposition on the bending of light by a gravitational field (with slightly different quantitative predictions than those of the final GTR) see Einstein 1911 in Perrett & Jeffery 1952 p99-108.

cognitive liabilities for the various versions of ether theory even in the absence of a competing theory like the STR. It is also worth noting that the ontological account of the _ad hoc_ focuses criticism on the ontological status of the ether and the posited forces acting through it, and that these very areas of conceptual difficulty are the ones that the STR so successfully exploits by elimination.

Furthermore, the testability account of the _ad hoc_ does not identify the cognitive liability of the hypothesis that the ether is both motionless and in motion, because the testable consequences of that hypothesis are the same as those of the ether theory in the alternative forms noted above. Finally, with regard to the inability to express the ether hypothesis without longitudinal waves, the ether hypothesis is not testable-_ad_-_hoc_, because the ether is simply assumed to be without longitudinal waves and the testable consequences of that assumption are, once again, the same as those of the ether theory in its alternative forms. So, it is not the testability account of the _ad hoc_ which identifies the cognitive liabilities of the Lorentz ether theory, but instead, it is the ontological account which does so (even in the absence of a competing theory).

(iii) _The non-pejorative nature of the derivability account of the ad hoc_. As for the derivability account of the _ad hoc_, it appears not to identify a cognitive liability. For it has the two-sided consequences noted above. And further, if it were the case that the derivability account identified a cognitive liability, a reason to reject a theory, a reason to think that the theory is mistaken, then, for example, it would be a cognitive liability of Newton's mechanics that Newton's second and third laws cannot be derived from Newton's first law. So, I suggest that it is the ontological account of the ad hoc which gives sense to Minkowski's intuition concerning the LFC that

> the contraction is not to be looked upon as a consequence of the resistances in the ether, or anything of that kind but simply as a gift from above -- as an accompanying circumstance of the circumstance of motion. (Minkowski 1907 in Perrett & Jeffery 1952 p81)

The derivability account does not capture this pejorative sense. And I simply do not see any deficiency identified by the derivability account. My own conjecture is that the intuition behind the derivability account of the ad hoc is that we should consider overall simplicity, causal efficacy, and predictive power of alternative theories when deciding among them, or in searching for new theories. But we can well make such judgements without the derivability account of ad hocness. (When physicists discuss comparative merits of Schroedinger versus Dirac quantum physics, the making of such judgements is, I believe, among the points of their discussions.) Now this analysis applies to those cases within the intersection of the derivability and heuristic accounts. And we have been working within that intersection throughout.

Finally, it may be useful to discuss more fully a likely intuition behind the derivability account of the *ad hoc* that may be expressed as follows: Suppose we have two theories of, say, quantum mechanics, Q_1 and Q_2. In Q_1 conservation of baryons and of strangeness are independent hypotheses. In Q_1 if one asks, "Why do these conservation principles hold?" the answer is, either "That is just the way Nature is" or, "That, nobody knows." In Q_2, conservation of baryons and of strangeness are derived from hypothesis E. In Q_2, if one asks, "Why do these conservation principles hold?" the answer is, "They are explained on the basis of E in Q_2." Isn't Q_2 a better theory? The answer, it seems to me, is that *ceteris paribus*, Q_2 is a better theory. But now what about E? If we now ask, "Why does E hold?" the answer is either, "That is just the way Nature is" or, "That, nobody knows." Is this point about E a cognitive liability of Q_2 -- a reason to reject Q_2, a reason to think Q_2 is mistaken? The answer, it seems to me, is No, and hence so also as well for Q_1 regarding conservation of baryons and of strangeness.

There is a story that Richard Feynman often retold about his father that seems appropriate here:

> He had taught me to notice things and one day ...I was playing with a...wagon.... It had a ball in it...and I pulled the wagon and I noticed something about the way the ball moved. So I went to my father and I said, "Say Pop, I noticed something. When I pull the wagon the ball rolls to the back of the wagon... and when I'm pulling along and suddenly stop, the ball rolls to the front of the wagon." And I said, "Why is that?" And he said, "That, nobody knows. The general idea is that things that are moving try to keep on moving, and things that are standing still tend to stand still unless you push on them.... And this tendency is called inertia, but nobody knows why it's true."--Now that's a deep understanding. He doesn't give me a name. He knew the difference between knowing the name of something and knowing something.
>
> He went on to say, that if you look close, you'll find that the ball does not rush to the back of the wagon, but it's the back of the wagon that you're pulling towards/against the ball, that the ball stands still or as a matter of fact from the friction starts to move forward really, and doesn't move back.
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> So, I ran back to the wagon and set the ball up again and pulled the wagon... and looking sideways and seeing indeed he was right, the ball never moved backwards ...when I pulled the wagon forward. It moved backward relative to the wagon, but relative to the sidewalk it moved forward a little bit. It's just that the wagon caught up with it.[27]

The point of this story to which I want to draw attention here is that physical theories generally contain independent hypotheses-- e.g. inertia, various conservation principles, Planck's quanta-- and it is not a cognitive liability of physical theories that they have such independent

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[27] Feynman told this story in an interview for the PBS television show, NOVA broadcast in 1982. The episode is titled, "The Pleasure of Finding Things Out, An Interview With Richard Feynman." See also Gleick 1992 p28-29.

hypotheses. This is so, even though _assuming_ alternative theories like Q_1 and Q_2, as imagined above, _ceteris paribus_, Q_2 is a better theory; it is an improvement in overall simplicity, causal efficacy, and predictive power. But, once again, such judgements can be made without the derivability account of _ad hocness_. Note as well that such a case as the transition from Q_1 to Q_2 involves the _retention_ of the derivable-_ad_-_hoc_ hypotheses (conservation of baryons and of strangeness) of Q_1; whereas, a transition from a theory T_1 to another theory T_2, where T_1 contains an ontological-_ad_-_hoc_ or testable-_ad_-_hoc_ hypothesis, involves the _rejection_ of the ontological or testable-_ad_-_hoc_ hypothesis. Pejoratively _ad hoc_ hypotheses are rejected, not retained. The imagined principle E, the principle of inertia, Planck's quantum hypothesis, the Pauli exclusion principle, _etc_. are independent hypotheses, but not pejoratively _ad hoc_ hypotheses; they very well may be candidates for further investigation and possible explanation, but they are not for these reasons mistaken nor are they candidates for rejection; and, they are not themselves, on account of their independence, reasons to reject the theories containing them, nor are they reasons to think that such theories are mistaken. For a formerly independent hypothesis to be further explained is not for it to be found in error, nor is it for the theory containing it to be mistaken. Raising ad hocness here only complicates, it does not clarify.

5. The Heuristic Account of the Ad Hoc

Next, I want to suggest a different way in which the heuristic account of ad hocness does not identify a cognitive liability. The heuristic account holds an hypothesis to be ad hoc if it is inconsistent "with certain basic tenets of the research program of which the theory is a part." (Leibowitz 1979 p82) Zahar describes a theory as heuristic-ad-hoc by saying, "... the theory is said to be [ad hoc] if it is obtained from its predecessor through a modification of the auxiliary hypothesis which does not accord with the spirit of the heuristic of the programme." (Zahar 1973II p101) Now if an auxiliary is inconsistent with some "basic tenet" of a research program, then that tenet must be rejected or modified to avoid contradiction. Lorentz's decision described in passage (B) above is not ad hoc on the heuristic account, and yet Einstein's Special Theory of Relativity is ad hoc on the heuristic account. Consider also that the proposal of Galileo's antagonist was "in accord with the spirit of the heuristic of the existing program." Or again, consider that at the time Planck introduced the quantum hypothesis, it was inconsistent with the accepted heuristic which treated energy as a continuously distributed phenomenon. Hence, heuristic-ad-hoc may be a descriptive account, but it is not a pejorative account of the ad hoc. In other words scientific progress sometimes takes place when a heuristic is replaced.

Finally, it might be thought that if we do not take the heuristic or derivability accounts of ad hocness to be pejorative, then we open the door to all sorts of "obviously faulty" auxiliary hypotheses in science, i.e. as a part of scientific theories. For example, the appending of "creationist" auxiliaries to evolutionary or cosmological theory as part of theory i.e. as a legitimate part of science. But I do not think this door is opened in this way at all. For the ontological and testability accounts of the ad hoc, and criteria of simplicity, causal efficacy, and predictive power, in the context of competing alternative theories rule out such obviously faulty auxiliaries.

6. A Revised Taxonomy of the Ad Hoc

Given the arguments above the revised taxonomy of ad hocness that I suggest is as follows:

a. Pejorative accounts
    (1) Testability accounts
        (A) Falsificationist
        (B) Inductivist
    (2) Ontological account
b. Non-pejorative descriptive accounts
    (1) Heuristic account

(2) Derivability account

I want to close this chapter with a story once told by Alistair Cooke on the television show, "Masterpiece Theater." Winston Churchill once worried that the crowded conditions in the makeshift bomb shelters during "the blitz" of the Second World War would lead to an epidemic of diphtheria, influenza, and common colds. When such epidemics failed to appear, Churchill concluded that, "Nature has provided for this. The microbes that humans exhale in such circumstances must attack each other, so that mankind can simply walk away from the battle untouched." And then Churchill paused and added, "If this is not scientifically correct, it ought to be." Now we might even imagine the spirit of the heuristic a research program such as this. And perhaps Hume would not too much mind if we rejoiced that there are at least some uses of 'is' that cannot be derived from some uses of 'ought.'

Table 1-1 summarizes the applications of the accounts of the <u>ad hoc</u> to examples employed in this chapter. Also included in table 1-1 are examples from Grunbaum (1976) and from Hempel (1966) which have become standard cases in discussions of the <u>ad hoc</u>. Sometimes with and sometimes without the testability account, the attribution of the ontological account ("Yes" in table 1-1) lends support to what have come to be seen as correct evaluations; the non-pejorative accounts show no similarly reliable coherence. Although the testability account does identify a cognitive liability,[28] it appears to be too weak in those cases in which it does not counsel criticism ("No" in table 1) but in which the ontological account does counsel criticism. The general counsel offered here regarding theory choice is to look to reject a theory that is ontologically-ad-hoc and/or ontologically superfluous, and/or testable-ad-hoc. It is worth noting also that these are cognitive grounds for choice; as argued above, these grounds are properly connected with knowledge.

[28] In an interesting and typically closely reasoned paper Grunbaum (1976) gives an analysis of the ad hoc which combines what are called here the derivability and testability accounts, distinguishes three logically nested sub-accounts (known and tested consequences, known consequences, logical consequences), and concludes that this analysis "...excludes pejorativeness from the definiens of ad hocness." Without recounting details of Grunbaum's careful discussion, it should be sufficient here to note that although the present analysis does maintain a pejorative function for the testability account of the ad hoc, as table 1 indicates, genuine cases of testable-ad-hoc hypotheses are fairly rare, and this criticism identifies only the most obviously faulty kinds of hypotheses.

TABLE 1-1

Key: **T:** testability-ad-hoc
O: ontological-ad-hoc
D: derivability-ad-hoc
H: heuristic-ad-hoc

Yes: the example is ad hoc on this account
No: the example is not ad hoc on this account

Example	Accounts of the ad hoc			
	Pejorative		Non-pejorative	
	T	O	D	H
various hypotheses in defense of psychological egoism	Yes	Yes	Yes	No
moon-invisible-substance[29] hypothesis	No	Yes	Yes	No
moon-undetectable-substance[30] hypothesis	Yes	Yes	Yes	No
the LFC (except in the TAE)	No	Yes	No	No
conservation of baryons	No	No	Yes	No
conservation of strangeness	No	No	Yes	No
no longitudinal waves in the ether (in all versions of ether theory)	No	Yes	Yes	No
ether in motion and not in motion (in all versions of ether theory)	No	Yes	No	No
LFC + time dilation in the DAE	No	Yes	No	No
LFC + time dilation in the TAE	No	No	No	No
the Special Theory of Relativity	No	No	Yes	Yes
Planck's quantum hypothesis	No	No	Yes	Yes
the Pauli exclusion principle	No	No	Yes	No
the microbes-attack-each-other hypothesis	No	Yes	Yes	No

postulation of the existence of the planet Neptune in response to perturbations in the orbit of Uranus[31]	No	No	No	No
postulation of the existence of the planet Vulcan in response to perturbations in the orbit of Mercury[32]	No	No	No	No
postulation that Nature's horror vacui decreases with altitude[33]	Yes	Yes	Yes	No
the plenists' postulation of the "funiculus"[34]	Yes	Yes	Yes	No
the postulation of "negative weight" to phlogiston[35]	Yes	Yes	Yes	No

TABLE 1-1

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[29] Note that Galileo's antagonist does not advance a consequence of his theory based on an application of it to observed facts (see the Neptune and Vulcan cases below), but instead he revises his theory such that it becomes "immune" from disconfirmation of the appropriate kind; if confronted with observations of Mars or of Io, the revised theory is ready with an "answer" involving the added kind of theoretical entity, the alleged invisible or undetectable substance.

[30] See footnote immediately above.

[31] Grunbaum 1976 p355-358. Note that the postulation of the existence of the planet Neptune, like that of the planet Vulcan in the next example, cannot be ontologically-ad-hoc because there is no postulation of an added auxiliary hypothesis which revises the theory (Newton's laws). The postulation of the existence of these planets is a straightforward application of Newton's laws applied to observed facts without any revision of Newton's theory at all. Confirmation/disconfirmation then can be the proper next step.

[32] Grunbaum 1976 p355-358. (See also the footnote immediately above.)

[33] Hempel (1966 p28-29) describes this case as follows: Before Torricelli introduced his conception of the pressure of the sea of air, the action of suction pumps was explained by the idea that nature abhors a vacuum and that, therefore, water rushes up the pump barrel to fill the vacuum created by the rising piston. The same idea also served to explain several other phenomena. When Pascal wrote to Périer asking him to perform the Puy-de-Dôme experiment, he argued that the expected outcome would be a "decisive" refutation of that conception: "If it happens that the height of the quicksilver is less at the top than at the base of the mountain . . . it follows of necessity that the weight and pressure of the air is the sole cause of this suspension of the quicksilver, and not the abhorrence of a vacuum: for it is quite certain that there is much more air that presses on the foot of the mountain than there is on its summit, and one cannot well say that nature abhors a vacuum more at the foot of the mountain than at its summit." [footnote omitted] But the last remark actually indicates a way in which the conception of a horror vacui could be saved in the face of Périer's findings. Périer's results are decisive evidence against that conception only on the auxiliary assumption that the strength of the horror does not depend upon location. To reconcile Périer's apparently adverse evidence with the idea of a horror vacui it suffices to introduce instead the auxiliary hypothesis that nature's abhorrence of a vacuum decreases with increasing altitude. But while this assumption is not logically absurd or patently false, it is objectionable from the point of view of science. For it would be introduced ad hoc--i.e. 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. The hypothesis of the pressure of air, on the other hand, does lead to further implications. Pascal mentions, for example, that if a partly inflated balloon were carried up a mountain, it would be more inflated at the mountaintop. [The reference for the Pascal quotation is Spiers 1937 p101. This is from the Pascal letter to Périer of 15 November 1647.]

[34] Hempel's (1966 p29) description here is,
About the middle of the seventeenth century, a group of physicists, the plenists, held that a vacuum could not exist in nature; and in order to save this idea in the face of Torricelli's experiment, one of them offered the ad hoc hypothesis that the mercury in a barometer was being held in place by the "funiculus", an invisible thread by which it was suspended from the top of the inner surface of the glass tube.

[35] Again, Hempel's account (1966 p29-30) is as follows:
According to an initially very useful theory, developed early in the eighteenth century, the combustion of metals involves the escape of a substance called phlogiston. This conception was eventually abandoned in response to the experimental work of Lavoisier, who showed that the end product of the combustion process has greater weight than the original metal. But some tenacious adherents of the phlogiston theory tried to reconcile their conception with Lavoisier's finding by proposing the ad hoc hypothesis that phlogiston had negative weight, so that its escape would increase the weight of the residue.

CHAPTER 2

EMPIRICALLY EQUIVALENT DESCRIPTIONS AND CONGRUENCE

...the things above us: they admit of more than one cause of coming into being and more than one account of their nature which harmonizes with our sensations.

Epicurus
(Letter to Pythagoras 85-87)

...the story telling side of science is not just peripheral, and not just pedagogy, but the very point of it all. Science properly done is one of the humanities....

Daniel Dennett
(1981 p460)

A paradigm case of underdetermination of physical theory by observational evidence is discussed in this chapter. This case involves empirically equivalent theories of physics employing alternative physical geometries and matching alternative definitions of physical congruence. Underdetermination of theory by observational evidence may be described as a situation in which two or more alternative physical theories are empirically equivalent (they predict all and only the same observational and experimental outcomes) and these outcomes are well confirmed, but the alternative theories, though they are each internally consistent, contain claims which are contraries of each other. Poincaré (1905), Reichenbach (1958), Carnap (1966), Grunbaum (1973), Quine (1975), and Salmon (1975) have argued that this is the case regarding alternative formulations of the General Theory of Relativity (GTR) depending upon the physical geometry employed along with a suitable physical congruence definition, and that any choice among these alternative formulations can be based at most upon pragmatic considerations of elegance, simplicity of expression, ease of application, _etc_. (but not upon cognitive grounds, grounds properly connected with knowledge). I will refer to this as _the conventionalist position_.

Interestingly, Poincaré argued this conventionalist position (published in his 1905) before the GTR had been invented. He argued the conventionalist view of any future physics employing a nonEuclidean geometry before there existed any such theory; he was prescient, indeed, in this regard. He also thought that a Euclidean theory would always prevail on conventionalist grounds, and in this he differs from his fellow conventionalists. For the most part in what follows I will not attend to differences among the conventionalists, but instead will focus on their conventionalist agreement in making their position as strong as possible; the conventionalist position unites the conventionalists and it is the conventionalist position that I wish to address. Also, it has become standard to discuss this and other topics related to the GTR using mathematical manifold models. Truly superb accounts are provided by Friedman (1983), Earman (1989), and Norton (1992). But I will defer discussion of manifold models to chapter 3, and will discuss conventionalism here in what now may be the old-fashioned way to see how far this can take us. It can take us quite far, I believe. So, I will not neglect manifold models but only delay their discussion to chapter 3.

The task of the present chapter is to argue that there are cognitive grounds that favor employing the ordinary definition of physical congruence in physical theory rather than alternative definitions. The ordinary definition of physical congruence is simply this: At rest relative to each other, measuring rods (and equivalent physical devices) which are physically equal when in close proximity to each other are equal at a distance unless altered by physical forces. Given this coordinating definition (Reichenbach 1958) connecting theory with the physical world it is possible for physics to empirically confirm a geometry of space, and hence to confirm the GTR.

It may at first seem implausible even to consider alternatives to the ordinary definition of physical congruence, but section 1 will summarize arguments making alternatives quite plausible; this is the conventionalist position regarding physical geometry and physical congruence. Section 1 also summarizes some of the history of the question of the geometry of space. Section 2 argues that theories employing alternatives to the ordinary definition of physical congruence are pejoratively ad hoc. Section 3 takes up a conventionalist position designed to avoid pejorative ad hocery[1], and argues that this position fails. Section 4 considers further objections and responses. Finally, section 5 addresses the issues of the imaginability of concepts and coherent descriptions, and summarizes conclusions.

1. Observational Underdetermination. The conventionality of physical congruence has been supported by Reichenbach, Grunbaum, Salmon and others. From this conventionality it has been argued that alternative descriptions of the universe that differ only in their definition of physical congruence and matching physical geometry are equivalent descriptions. Salmon has described equivalent descriptions as follows:

> Two physical theories which do not differ in empirical content are equivalent descriptions: they do not differ from one another in truth-value, probability, confirmation, or any other cognitive respect. The differences between equivalent descriptions relate to such matters as descriptive simplicity, economy, elegance, and ease of application. (Salmon 1969 p62)

Following Reichenbach (1958), Salmon presents equivalent descriptions for an imaginary two-dimensional-world-with-a-hump. (Figure 2-1 below) We have a choice

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[1] Thanks to Keith Lehrer for suggesting this term.

between equivalent descriptions of this world: We can choose a definition of physical congruence, and then the physical geometry of this world is confirmed by empirical observations. Or, we can choose a descriptive geometry, and then the definition of physical congruence is observationally confirmed. Historically, Poincaré (1905) had held that when faced with such a choice, Euclidean geometry is to be preferred (because it is simpler than alternative geometries), and hence that physical congruence must be defined such that it preserves physical Euclidean geometry. In the world of figure 1 such a choice has the consequence that physical measuring rods which are equal when in close proximity, are not equal at a distance, hence denying the ordinary definition of physical congruence. And a conventionalist will maintain that an imagined two-dimensional conventionalist living in this imaginary world, who holds that measuring rods expand and contract in a way that preserves the correctness of physical Euclidean geometry as a description of this world (description B), has provided an empirically equivalent description of this world.

Now it is quite true that such a description does not differ empirically from the alternative: further observations could not disprove it. But it will be argued here that there are cognitive grounds to prefer the ordinary definition of physical congruence in physical theory and these are not merely pragmatic considerations of elegance, simplicity of description, ease of application, etc. If the argument holds it will show that there are grounds to prefer the ordinary physical definition of congruence which defines physical measuring rods, equal in close proximity, to be equal at a distance (allowing for ordinary distention and constriction by physical forces). This, of course, is the ordinary concept of physical congruence.

It is important to note that it will not be argued that a particular geometry has a privileged position, but rather that a particular definition of physical congruence has such a position. Further, the ordinary physical definition of congruence (coordinative definition, Reichenbach 1958) together with empirical observations can be used to confirm a geometry of the cosmos; or, geometries for various regions of the cosmos. Thus, the GTR which employs a nonEuclidean variable g-metric tensor geometry (described in chapter 3 below) does not have an equally justified equivalent description in terms of a different definition of physical congruence and Euclidean geometry.

We will begin by briefly sketching the background against which the two-dimensional-world-with-a-hump is presented. The invention of nonEuclidean geometries and the proof of three claims concerning geometry has led to the question, "Which geometry correctly describes the cosmos?" The three claims are as follows: (i) The Euclidean geometry of one parallel, Riemanian geometry of no parallels, and the many parallel geometry of Bolyai-Lobachewski each can be formalized in an axiomatic fashion and generalized in three or more dimensions. (ii) Curvature can be defined internally (tensors--see chapter 3 below), i.e. without reference to a higher dimension space. (iii) Though there is no unconditional proof that any of the geometries is consistent, there is a proof that if one is inconsistent, then all are inconsistent; and thus, the three geometries are on a logical par. (Reichenbach 1958 p32) The properties of the alternative geometries are set forth below (table 2-1) in a table originally presented by Salmon. (Salmon 1975 p14)

TABLE 2-1

	Riemannean	Euclidean	Bolyai-Lobachewskean
Parallels	Zero	One	Many
Surface	Sphere	Plane	Pseudosphere (Or saddle as an approximation)
Curvature	Positive	Zero	Negative

Angular sum for triangles	$>180^0$ depends on size of triangle	$=180^0$ independent of size of triangle	$<180^0$ depends on size of triangle
Ratio of circumference to diameter of circle	$<\pi$ depends on size of circle	$=\pi$ independent of size of circle	$>\pi$ depends on size of circle

TABLE 2-1

As the table shows there are several differences among these geometries and one of particular interest is that the angular sum of a triangle is distinct for each geometry. More specifically, the angular sum in the nonEuclidean geometries diverges from 180^0 as the size of the triangle becomes larger. The mathematician, Carl Friederick Gauss was apparently the first to realize that this fact might provide a method to answer the question concerning which geometry describes physical space: We need merely to measure the angular sum of a large triangle in space. To this end Gauss surveyed a triangle with vertices at the tops of three mountains in the Swiss Alps. Gauss obtained the unsurprising result that there was no significant difference between the angular sum of this triangle and 180^0 within the accuracy limits of his instruments. But this result is far from decisive, since the size of Gauss's triangle is exceedingly small by comparison to the size of the cosmos. (Salmon 1975 p15)

The historical positions of Poincaré and of Kant are as follows. Poincaré held that even if we were to measure a large triangle in space and to discover an angular sum different from 180^0, this would not be conclusive evidence that space is nonEuclidean. Salmon characterizes this position as follows:

> "After all, it could be that strange forces are affecting our measuring rods--perturbations which make them change their size or shape as they are moved from place to place." . . . the result does not prove conclusively that the sum of the angles of the triangle is less than 180^0; it might be taken to prove instead that the figure in question is simply not a triangle--and that we must adjust our views about light rays and measuring rods accordingly . . . <u>any</u> apparent deviations from Euclidean geometry could always be explained away much as we have just indicated . . . This means that we can always, if we wish, adjust our experimental results to fit Euclidean geometry; we <u>can</u>, if we wish, preserve Euclidean geometry at all costs. (Salmon 1975 p16)

Poincaré believed that we should do so on grounds of elegance and simplicity. Kant held that we must do so, not because of a logical superiority of Euclidean geometry (this would be the misconception that nonEuclidean geometries are inconsistent), but because it is impossible to conceive of physical spatial relations in other than Euclidean terms. As Salmon relates this view:

> . . . we can picture a two-dimensional nonEuclidean surface, but that is because we can stand outside of it in our three-dimensional Euclidean space and observe the curvature of the surface as it is embedded in three-dimensional space. But we cannot picture the curvature of our whole three-dimensional space because we cannot imagine stepping off into four-dimensional space. (Salmon 1975 p17)

But Salmon notes that Kant had only considered external visualization, and that it is quite possible to visualize nonEuclidean space internally. To visualize a space internally is just to imagine the kinds of experiences one would have if one were living in such a space. (Reichenbach 1958 p32-34&37-43 Salmon 1975 p18) Imagining such experiences does not involve stepping into a higher dimensional realm; it may be psychologically difficult, but it is conceivable. Internal visualization is the conceptual analogue of internal curvature; both are conceived without reference to a higher dimension space. Claims i, ii, and iii place the Euclidean and nonEuclidean geometries on a logical par; internal visualization (which we can label claim iv) places them on a conceptual par. Our question remains: Which geometry describes physical space?

These points bring us to Reichenbach's (1958 p11) two-dimensional-world-with-a-hump. (Figure 2-1) Since Salmon's description is especially clear it is worth quoting.

Figure 2-1

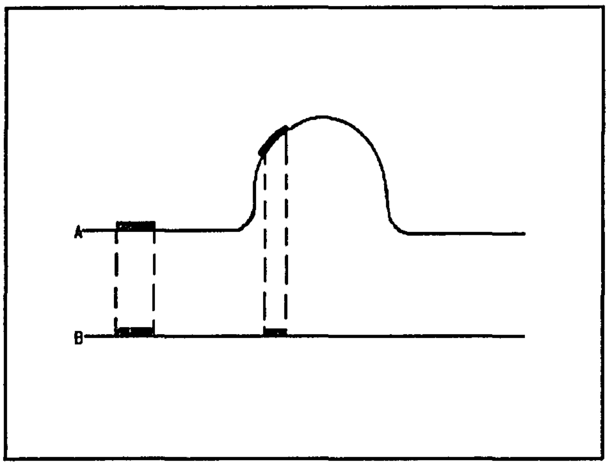

Figure 2-1

Imagine that world A, shown in cross-section in figure (1), is a two-dimensional world consisting of a flat plane with a hump in the middle. Again, imagine that this world is inhabited by two-dimensional creatures who move about and make measurements in an attempt to survey it geometrically. In the peripheral regions of their space, they would find that it has Euclidean characteristics--the ratio of circumference to diameter of a circle is always π, the angles of the triangle always add up to 180^0, and so on. In the central part, where the hump is located, they would find that their space has the geometrical properties of a sphere. In the region where the hump joins smoothly with the flat plane, they would find characteristics rather like those of the saddle surface. Moving about in their space and making such measurements, they could find out that their space has precisely the kind of curvature just described, though they would be unable to form a three-dimensional...image like ours.

Imagine, at the same time, beings who live in a world B, located directly below world A, with measuring rods that behave in a peculiar fashion. Whenever these rods are moved from one place to another, they behave exactly as if they were vertical projections of measuring rods in world A .

> above. Since <u>all</u> material bodies are assumed to expand and contract in the same manner, these beings would be quite unaware of such changes in their measuring rods. Obviously, the creatures in world B would get exactly the same results from their measurements as the inhabitants of world A. Now, this shows quite clearly that there are two interpretations available. A two-dimensional [conventionalist] living in world B might say, "Wait a minute--it is not necessarily true that our world is a flat surface with a hump in it. Maybe our world is one in which the measuring rods behave in a very odd manner, contracting and expanding in such a way that this world appears to have a hump when in fact it is perfectly flat." None of his countrymen in world B could refute him, as we saw in our discussion of Gauss's experiment. At the same time, precisely the same argument would be open to any inhabitant of world A as well, for the inhabitants of both worlds have precisely the same experiences. We see that, although these two worlds look very different from our God-like external vantagepoint, from the standpoint of beings confined within these two worlds there is absolutely no way of distinguishing one from the other. Indeed, these are identical worlds! We have merely offered two equivalent ways of describing the same spatial facts. Henceforth we shall refer, not to two worlds, but rather to two descriptions, A and B, of the world of figure (1). (Salmon 1975 p21-22)

Thus we are faced with the following problem. We imagine ourselves as inhabitants of this two-dimensional world and we want to know which geometry describes our world. We know that the three geometries are on a logical and conceptual par. We take measurements and discover that the three geometries describe different regions of our world. The conventionalist argues that our definition of physical congruence (the ordinary one) is a matter of convention, not an observational fact, and that by adopting a different convention one can give an empirically equivalent description of this world. He further maintains that equivalent descriptions cannot be refuted, though they may be rejected on pragmatic grounds of simplicity of description, elegance, ease of application, etc. The conventionalist further points out that nothing prevents the generalization of the relevant circumstances of these imagined two-dimensional-creatures to our own circumstances. Claims i, ii, iii, and iv serve to support such a generalization. So, the conventionalist concludes that the question of the geometry physical space is a pragmatic question not a (cognitive) question about our knowledge of the physical world, and this conclusion is illustrated by the heuristic device of the two-dimensional-world-with-a-hump. The conventionalist then further concludes that given a well confirmed physical theory like the GTR that includes a nonEuclidean physical geometry, that there is an empirically equivalent theory using Euclidean geometry. Now standard GTR employs a nonEuclidean geometry of variable curvature, so the conventionalist concludes that there is an indefinitely large class of other alternative empirically equivalent theories, each with its own particular combination of geometry plus matching congruence convention. Choice among these alternatives is pragmatic choice, according to the conventionalist; so, we have (a problem of) underdetermination of empirically equivalent theories on a truly grand scale.

Furthermore, Grunbaum (1967) offers an argument to further support the conventionalist position (called the denial of an intrinsic metric of space) which can be summarized fairly simply. Begin with Zeno's paradox of plurality as applied to a spatial line: Any line segment is infinitely divisible. So, any line consists of an infinite number of parts. But then Zeno asks what is the size (length) of these parts? If it is finite then the length of every line must be infinite because an infinite sum of finite lengths is an infinite length; but, if it is not some finite length then it is zero length, so every line must then have zero length because the sum of zero lengths of parts, no matter how many, is still zero. Either alternative is absurd. Now for our purposes here it is not necessary to review Cantor's mathematical analysis as employed to resolve this paradox (Grunbaum 1967, Salmon 1970), so suffice it to say that Grunbaum argues that the paradox is resolved by holding that length is assigned to any given spatial line (spatial interval) not on the basis of any internal structure (intrinsic metric), but rather by stipulation, by convention from the outside as an essential defining step before any physical measurements can be made. So physical "length assignment" (congruence definition) must be a matter of conventional stipulation prior to physical measurement; and, any attempted resolution of the problem of underdetermination of the geometry of physical space that involves the denial of the conventionality of physical congruence definition will fail, because it will immediately fall victim to the dilemma of absurdities posed by Zeno's paradox of plurality.

So the conventionalist position regarding the geometry of physical space is that it is a matter of pragmatic choice among empirically equivalent alternatives. In sum, this position is supported by claims i, ii, iii and iv; by Reichenbach's argument; and, by Grunbaum's argument. And again, this is rather radical underdetermination involving, as it does, an indefinitely large class of alternatives to the GTR and to any other physics as well.

2. Ad Hocery. We now turn to argument against the conventionalist position. Each of the empirically equivalent alternatives to the GTR posed by the conventionalist position involves the postulation of universal forces that physically alter measuring devices but that do so in an empirically undetectable way. Reichenbach distinguishes universal forces from differential forces, the latter being empirically detectable forces. Nagel (1961) argues against the postulation of universal forces as follows:

> . . . universal forces have the curious feature that their presence can be recognized <u>only</u> on the basis of geometrical considerations. The assumption of such forces thus has the appearance of an <u>ad hoc</u> hypothesis, adopted solely for the sake of salvaging Euclid. Indeed, the 'deformations' in the bodies which must be attributed to universal forces in order to save Euclid have a markedly geometrical rather than physical character. The deformations persist even if all differentiating forces are eliminated; and they are construed to be 'alterations' in the 'natural' shapes and spatial dimensions of bodies, only because the criterion of rigidity that is now tacitly employed is the possession by the body of just those geometrical properties prescribed by Euclid. (Nagel 1961 p264)

Nagel goes on to point out that even if we allow universal forces in order to retain a Euclidean description, the resulting system of physical theory is likely to be more complex than alternatives which incorporate no universal forces and a nonEuclidean geometry. And such, arguably, would be the case for any such reformulation of the GTR. (Nagel 1961 p265) These grounds regarding complexity seem to be sufficient grounds for rejecting a physical theory that requires universal forces, but these grounds are pragmatic grounds that the conventionalists already accept. So let us return to Nagel's charge of <u>ad hocery</u>.

Given the arguments of chapter 1 it is not difficult to see what sort of <u>ad hocery</u> is present in the Reichenbachian kind of argument. Setting aside the nonpejorative accounts, we first ask whether the postulation of universal forces is testable-<u>ad</u>-<u>hoc</u>. Universal forces fit the definition of testable-_ad_-_hoc_, for they are added auxiliary hypotheses that have no testable consequences other than their prediction in the context of the revised (_e.g_. Euclidean geometry retaining) theory of the kind of result that motivated their formulation in the first place. This shows that even among empirically equivalent theories it is possible to find some theories that are testable-_ad_-_hoc_, while others are not; standard GTR is not testable-_ad_-_hoc_, for it has no added auxiliary hypothesis like those posed by each of the alternatives suggested by the conventionalists. Now since inductivists are likely to hold the testability account of the _ad hoc_, and since most of the conventionalists are inductivists (surely Reichenbach, Grunbaum and Salmon are inductivists), I leave this situation for conventionalist inductivists to resolve.

More important on my view is whether the postulation of universal forces is ontologically-_ad_-_hoc_. Recalling that we define the _ontology_ of a physical theory as the individuals, properties, and relations in the physical system which is a model of the theory; and further recalling that we define a _direct ontological claim_ as a claim about the ontology of a theory, and an _implicit ontological claim_ as one that presupposes a direct ontological claim, we can see that the postulation of universal forces is ontologically-_ad_-_hoc_: Universal force hypotheses are added auxiliary hypotheses which revise the (respective) universal force theories such that they allow the retention of direct or implicit ontological claims (claims regarding geometrical properties and relations in the physical system that models the theory) of the retained theory despite a disconfirmation (recall that Reichenbach's advocate responds to the results of measurement); and, the universal force hypotheses are vacuous in the sense that in the revised theory they play no role, they have no function except to eliminate the disconfirmation.

Now at this point the conventionalist might claim that standard GTR is "equally pejoratively *ad hoc*" as is each of the empirically equivalent alternatives, asserting that in the case of standard GTR the testable-*ad*-*hoc* and ontological-*ad*-*hoc* hypothesis is the ordinary definition of congruence. But such a claim cannot be maintained, because in standard GTR there is *no added auxiliary hypothesis* and there is *no added auxiliary hypothesis which revises the theory*. (This will become even more obvious when we examine manifold models in chapter 3 below.) The Reichenbachian advocate responds to the results of ordinary measurements *by adding some universal force hypothesis to* the theory plus the results of ordinary measuring employing ordinary congruence. Standard GTR adds no such further auxiliary hypothesis. So, standard GTR cannot be "equally pejoratively ad hoc" as this possible conventionalist claim might assert. Of course, none of the conventionalists have made such a claim, but it does seem worth examining, even though in fact, those conventionalists who have noticed the problem of ad hocery (Carnap and Salmon) have not raised it.

Given then that universal force hypotheses are pejoratively ad hoc, the rational choice is to reject the indefinitely large class of empirically equivalent alternatives and to accept standard GTR on these cognitive grounds. We can further note that none of this runs afoul of Grunbaum's argument regarding Zeno's paradox of plurality. The physical congruence definition is stipulated as (an assumed) part of the physical theory (of the GTR, as well as all other physical theories), and is established conventionally prior to making observations; it does not involve any intrinsic metric of space. It is simply the ordinary definition of congruence: At rest relative to each other, measuring rods (and equivalent physical devices) which are physically equal when in close proximity to each other are equal at a distance unless altered by physical forces. Since universal forces are rejected as pejoratively ad hoc, we can safely employ standard physical measuring devices and confirm the GTR without further concern for the exceptionally large class of empirically equivalent alternatives. We do this while maintaining the conventionalist inductivist point that the physical definition of congruence is a coordinating definition that must be established by convention prior to measurement and then employed to properly link physical theory to the physical world that models it when the physical theory is confirmed. We do this as well while maintaining the conventionalist inductivist point about the empirical equivalence of the exceptionally large class of alternatives to standard GTR.

Is this the end of the story? Alas, not so. For of those who have advocated the conventionalist position it is Carnap and Salmon who have most clearly seen the threat of _ad hocery_, and who have taken measures to avoid it. We turn to a consideration of those measures in the next section.

3. _Metaphor_, _Conventions_, _Practices_. In his 1975 Salmon notes,

> The distinction between differential and universal forces is due to Reichenbach. This terminology has led to the criticism that such forces are introduced _ad hoc_. However, the term "force" in this context is metaphorical. Literally speaking, there are no such "forces" at all, there are only different definitions of congruence....Carnap... (1966) suggests that "differential effects" and "universal effects" are less misleading terms. The term "effect" is equally metaphorical in this context. (Salmon 1975 p132 n14) [my edit]

Interpreted in this way the conventionalist position is only offering alternative stipulative definitions (a great many of them) of physical congruence (to match a great many physical geometries). But the obvious question to ask about this offering is, On what grounds? The Reichenbachian argument gains physical plausibility in offering physical alterations of physical measuring devices produced by universal forces; even the illustrative picture offered (Figure 2-1) is one that portrays images of physically altered measuring rods (description B) and the accompanying narrative speaks of strange perturbing forces. So it is difficult to see how the metaphorical interpretation is supposed to work. If the use of the term 'force' in the conventionalist argument is just metaphorical, the argument loses a good deal of its initial physical plausibility. Furthermore, by suggesting that universal forces are only metaphorical the conventionalist position apparently becomes committed to an intrinsic metric of space: For now the only thing that produces the alleged contractions and distensions is simply being at different places. Just being at different places simply does produce the alleged "universal effects" of distension and constriction (in just the sort of way that preserves a physical geometry of choice), and what else can this be on this account other than simply an intrinsic property of different locations, an intrinsic property of space, a metric property of space? Indeed, paradox threatens, and I can see no way out on this view. I leave these problems of plausibility and of paradox to the metaphorical-conventionalists.

In addition, I do not know whether Salmon continues to hold this metaphorical-conventionalist view, but it does not appear to be compatible in any obvious way with his 1984 nor with his 1989. Some of the central positions argued in these works may be summarized (all too briefly) as follows: Causal processes propagate the structures of the physical world; causal interactions produce the causal structures and produce changes in causal structures; causal laws describe (govern) causal processes and causal interactions; causal processes and interactions are the mechanisms of the world. To explain is to answer why things are and why things happen. To answer why things are and why things happen is to show how they are produced by causal mechanisms.

Now metaphorical forces do not explain anything; they do not cause anything; and, they do not serve to support the conventionalist position (at least on Salmon's general philosophy of science). In a slightly different context Salmon (1984 p237-238) agrees with Cartwright (1983) that fictitious causal mechanisms do not have explanatory import-- "The tooth fairy does not explain anything" (Salmon 1984 p238) --and even a <u>useful</u> fiction can't explain. Perhaps we can ask metaphorical-conventionalists to explain their position and the support for it.

Perhaps the metaphorical-conventionalist view can be worked-out, though I doubt it. Nevertheless, <u>in the remainder of this section I wish to suspend discussion of the metaphorical-conventionalist position and of universal forces</u>, and to discuss the issue of the stipulation of alternative definitions of physical congruence on its own. We will interpret the conventionalist position as simply offering an indefinitely large class of alternative stipulative definitions of physical congruence properly matched with alternative physical geometries such that the resulting indefinitely large class of matching pairs are each empirically equivalent to each of the others. In particular, one matched pair would be a modified GTR with a physical Euclidean geometry and a suitably altered non-standard variable physical congruence definition; this matched pair is empirically equivalent to standard GTR (employing the ordinary definition of physical congruence).

<u>Discussion</u>. Note that the conventionalist position holds that we can stipulate a physical congruence definition and then observationally discover the physical geometry of our world, <u>or</u> alternatively, that we can stipulate the physical geometry and then observationally discover the physical congruence definition of our world. Now the question I wish to raise is, How could we discover a physical definition of congruence that describes our world in the way that the conventionalist claims? We have already

noted Grunbaum's argument that congruence must be stipulated prior to measurement as a matter of convention. And then we asked, Which geometry describes our world? But can this process be reversed as the conventionalist position asserts? The definition of congruence is a matter of stipulation and agreement. So, in these rather unusual circumstances, we wonder which convention shall we use? To ask this, is to ask for a reason, but in order to give a reason we must have some end in view. What is the purpose of defining physical congruence? Is it possible that we should define physical congruence for a purpose other than measuring? The conventionalist proposes this, but is it possible that we could <u>first</u> define physical congruence in a way that would preserve, say, Euclidean physical geometry, and <u>then measure</u> with rods so defined? This cannot just involve imagining the kinds of experiences one would have if.... It involves conceiving of <u>measuring</u> as a practice in which the results of measurement are acceptable only insofar as those results are compatible with the functions of Euclidean geometry. But no one ever does <u>measuring</u> with such an end in view. And further, how can such a definition of physical congruence be established <u>specifically</u> before making any measurements? And how can any measurements be made without first establishing a specific conventional definition of physical congruence? Such an imagined Euclidean-geometry-preserving-coordinative-definition does not merely link physical objects to mathematical functions; instead, it requires that physical objects have properties prescribed by a restricted set of mathematical functions. And this simply is not anything like ordinary measuring.

Perhaps we can imagine "measuring" as so described, but we should not call such an imagined practice measuring. Our ordinary practice of physical measuring uses the ordinary definition of physical congruence. Within that practice there are congruent and incongruent measuring rods. But this imagined practice subsumes both what is incongruent and what is congruent. Ordinary measuring distinguishes these. Someone might distinguish some new use for 'congruent' and 'incongruent.' But we need not examine such uses to note the loss of these terms' ordinary use: That use is part of the practice of physical measuring. To call the imagined practice "measuring" creates conceptual confusion. It is a simple confusion of two (or more) quite different practices, one allegedly imagined, the other actual and ordinary.

A Reductio. There is a straightforward argument against the conventionalist position. The conventionalist requires the results of ordinary physical measurements in order to make the conventionalist claim. Note that the conventionalist makes his claim on the basis of facts stated in terms of the ordinary definition of physical congruence, in terms of ordinary physical measurements. Without _these_ facts he cannot make his claim; without the ordinary definition of physical congruence _these_ facts cannot be expressed: They cannot be expressed in terms of a conventionalist alternative definition unless they are first expressed in terms of the ordinary definition. Congruence is a matter of convention, so _before_ measuring starts, congruence must be defined. Try to conceive the statement of a _specific_ Euclidean-geometry-preserving-definition-of-physical-congruence _prior_ to any physical measurement. A conventionalist cannot state a physical congruence definition without using facts gathered using the ordinary physical congruence definition. Each of the alternative definitions is a contrary of each of the others. Measurement cannot begin with the indefinitely large class of conventionalist definitions; it must begin with the ordinary definition. So, in short, the conventionalist argument is a _reductio ad absurdum_ of itself. It assumes as part of its premises, a contrary of part of its conclusion. Hence, even if we suspend criticism of the metaphorical-conventionalist position and of universal forces, the conventionalist position fails.

4. _Further Objection, Further Response_. As a further objection to the arguments thus far it might be claimed that the ordinary definition of congruence cited here is not part

of the practice of measuring, and this might be combined with the "equally pejoratively ad hoc" objection noted above. It might be claimed that ordinary measuring goes on without any definition of congruence, other than perhaps a minimal ostensive one implicit in the use of measuring sticks. This view might claim that ordinary measuring goes along without the ordinary definition of congruence and without any of the contrary alternatives as well, and that the presence or absence of universal forces (metaphorical or not) are just by-the-by with regard to ordinary measuring. The objection can be more fully expressed along the following lines,

> Of course we need to start with some sort of definition of local congruence, we will call them "the same length" if when we lay them together their ends coincide, or something of that sort. But that is harmless enough since here we are just explaining our terms, not presupposing that things behave in any particular way. Although, as per Wittgenstein, such definitions may depend for their usefulness on things so behaving, that is quite another matter. Then to go on, however, to say that things will retain the same length when moved apart so long as not exposed to differential forces is not similarly something we need to do, nor in our ordinary practices do do. Nor is this latter, unlike the former, neutral or innocent, merely formal rather than material and substantive. Unlike the purely local definition, this latter does not merely explain how we will use our terms, but makes claims about how the world itself works. It claims that once we have found things equal in length, then, absent differential forces, they will remain equal when one is moved to another location. Such a strong claim is clearly not something we somehow assume with our simple local definition, nor something which could somehow be ostensively demonstrated by

our simply matching the ends of our objects and saying "equal". (Remember, moreover, what Wittgenstein has shown us about the limitations of ostensive definitions.) It is, indeed, thus perhaps better called an assertion or assumption than a definition.

That this is so is shown by the simple fact that with this congruence assumption, plus observations, we can derive geometrical conclusions not derivable from those observations plus our neutral local congruence definition alone. And this in turn shows that we do indeed have also open to us the alternative course of instead taking simply our definition of local congruence, together with a geometry, and leaving the question of the variation of length with location to be determined empirically by the observations. It is indeed only the triad of such a congruence assumption, a geometry, and observations that can yield an inconsistency, no pair of them alone can do so. Thus the inconsistencies can be removed, with equal cognitive justification or lack thereof, <u>either</u> by changing the geometry as required to conform to the other two <u>or</u> by changing the congruence assumption to conform to the other two. Neither course is, at least so far as the above arguments go, any more, or less, <u>ad hoc</u> than the other. Any objection that can be raised to the one, or at least that has been so far raised, is equally applicable to the other.

<u>We start out with two assumptions</u>: <u>that the universe has a Euclidian geometry and that measuring rods do not change their lengths absent differential forces</u>. We then encounter observations that, together with the conjunction of these two, lead to contradictions. "Let's change the rigid rod assumption," I suggest. "No!" you retort. "That would be ontologically-<u>ad</u>-<u>hoc</u> since you would be doing it only to preserve the Euclidian geometry assumption. We must therefore abandon the latter instead." "But <u>that</u>," I am force mildly to protest, "would be equally ontologically-<u>ad</u>-<u>hoc</u> since you are doing it only to preserve the rigid rod assumption."

Your attitude here strikes me as not entirely unlike that of student I once had. In order to drive home the Wittgensteinian point about the limited, local, and context bound character of our uses of words like "same" and "equal" I used

> Wittgenstein's reductio example (1958 §350): It's as if I were to say: "You surely know what 'It is 5 o'clock here' means; so you also know what 'It's 5 o'clock on the sun' means. It means simply that it is just the same time there as it is here when it is 5 o'clock." I was, of course, like Wittgenstein himself, assuming that it would be perfectly obvious to everyone in this day and age that knowing what "It's 5 o'clock here" means would not confer knowledge of what "It's 5 o'clock on the sun" might mean. With Wittgenstein, I assumed that one doesn't really know what such familiar sounding words mean, that an understanding of local simultaneity does not confer an understanding of simultaneity at a distance. This may well have been Einstein's deepest and most difficult discovery, and one undoubtedly instrumental to Wittgenstein's own later generalized application. Thus I leaned back with a triumphant smile. At this point, however, one of the students raised his hand and announced ringingly, "Of course it's 5 o'clock on the sun when it's 5 o'clock here. It's the same time everywhere!" I was, I must admit, completely dumbfounded. I am, I must admit, equally dumbfounded by your equally ringing, and, thus far at least, equally unjustified, insistence that, "It's the same length everywhere!"[2]

There is, it seems to me, an adequate mode of response to such objections. We do not merely define ordinary physical congruence for measuring by saying, something like, "We will call them 'the same length' if when we lay them together, their ends coincide." A good deal more goes into defining physical congruence for ordinary measuring, though it is quite right that much of this is implicit. For example, we partly implicitly and partly ostensively hold that rubber bands are not suitable for what we have said in

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[2] Thanks to Joseph Cowan for suggesting this line of objection.

saying the proposed "definition of congruence" cited in the objection. And this sort of example shows we are taking care to avoid distortions introduced by physical forces. We make corrections as well for temperature and barometric pressure, *etc*. in very precise measurements. Also, for example, we partly implicitly and partly ostensively hold that the defined congruent measuring devices are congruent at relative rest with each other. Ordinary definition of congruence proceeds by laying down measuring rods, not by continuously moving them past each other. Such points help to display the context of the ordinary definition of physical congruence in the practice of ordinary physical measuring. So, although it is *very seldom said completely*, it is our ordinary definition of physical congruence that: At rest relative to each other, measuring rods (and equivalent physical devices) which are physically equal when in close proximity to each other are equal at a distance unless altered by physical forces. But geometries are much more complex and come much later, and they surely are not constitutive of the practice of measuring in the way that the ordinary definition of physical congruence is so constitutive.

When the conventionalist "suggests *changing* the rigid rod assumption" (*i.e*. *changing* the ordinary definition of congruence that we do our measuring with) he introduces also an additional universal force hypothesis, and that hypothesis is testable-ad-hoc and ontological-ad-hoc and has also the problems for its metaphorical version noted above. Further, the attributions of testable-ad-hoc and ontological-ad-hoc to universal force hypotheses are not based upon a phrase like "you would be doing it only to preserve the Euclidean geometry assumption," but instead are based upon the fully specified definitions of testable-ad-hoc and ontological-ad-hoc worked out in chapter 1 and applied in section 2 of the present chapter. But no such additional hypothesis is employed by standard GTR. (Again, this point is made even more obvious with manifold models to be discussed in the next chapter.) Hence, standard GTR cannot be "equally ontologically-ad-hoc", nor can it be "equally testable-ad-hoc." Now since "differential forces" only make sense if there are such things as "universal forces", I eschew the term "differential forces" in this response. There simply is no reason to admit the alleged universal forces (metaphorical or not), just as there was no reason to admit the alleged undetectable substance proposed by Galileo's antagonist as discussed in chapter 1; and, there are very good reasons, as argued above, to reject both sorts of alleged undetectables and to conclude that theories employing them are mistaken.

Even if we were to assume beginning with a kind of "minimal" absence-of-the-usually-tacit-points in the ordinary definition of congruence, like that suggested in the objection, the result is not the sort of cognitive par suggested in the objection, but rather a quite different one. That this is so is shown as follows: The empirically equivalent alternatives to standard GTR each require the addition of a specific universal force hypothesis (metaphorical or not), while standard GTR needs no such further added hypothesis. Even if we were to begin by "operating" with the suggested sort of "minimal" (absent-tacit-points) congruence definition, we would arrive at standard GTR; it requires the addition of a further hypothesis, a particular universal force hypothesis (metaphorical or not), to generate any of the empirically equivalent alternatives. It will not do to object here, "But standard GTR employs the additional rigid rod assumption," because each of the empirically equivalent alternatives must employ not only a non-rigid-rod-assumption (must assume rods to be non-rigid in an undetectable way), but also must employ an additional specific (quantitative) universal force hypothesis (metaphorical or not), and this is so even if interpreted as something like, "the undetectable distensions and constrictions are simply due to changes in location." So, even if we proceed initially in the way suggested in the objection, it is plain that standard GTR is on a par with each of the empirically equivalent alternatives as regards having to make some "rod-assumption." But unlike the GTR, each of the empirically equivalent alternatives must specify some further added universal force hypothesis (metaphorical or not), and it is at this point that the arguments against universal forces (in their alternative forms) can be brought to bear. And note again in particular, the rather formidable arguments that can be brought to bear against such hypotheses offered in a form like, "the undetectable distensions and constrictions are simply due to changes in location."

Further, we do not start out with <u>the two assumptions</u> indicated in the objection. We start out knowing that there are three contrary general systems of geometry, and knowing also that there are far more mathematical functions and systems of functions than ever can be modeled by the world, the possible applications among the general systems of geometrical functions being just one of many such possibilities for the application of mathematics to physical things. But we do not start out <u>measuring</u>, assuming anything at all about geometry. Much later when we set out to find "the geometry of the world" (the geometrical functions that properly apply to the world) we used <u>measurements</u>, and we needed <u>not</u> additional hypotheses to arrive at standard GTR.

Moreover, our conclusions are rather unlike those of the student cited, because according to the GTR it is quite right, to say, "Objects are the same relative rest length everywhere unless altered by physical forces." As Einstein (1952 pvi) remarks (and as this will be more fully discussed in the next chapter), "Physical objects are not <u>in space</u>, but these objects are <u>spatially extended</u>. In this way the concept "empty space" loses its meaning." There is no intrinsic metric of physical space and time, on pain of severe paradox to the contrary (Zeno's paradox of plurality as noted above); and, this point has been properly advocated by conventionalists. And further, an understanding of local simultaneity in the GTR does in fact confer an understanding of simultaneity at a distance (through the Lorentz Transformation, the *g*-metric tensor functions, and the field equation discussed in the next chapter). We might even use a thought experiment to determine quite precisely what time it is on the sun when it is five o'clock here, employing imagined physical clocks of identical construction. Such thought experiments are routinely used to demonstrate how the GTR works, and we might as well use a particular spot on earth and perhaps a particular sun spot for a demonstration. This particular sort of demonstration would be in fact very complicated indeed (as will become more obvious in the next chapter) but it could be done, quite precisely. It could even be used as a basis for a very technically difficult experimental test of the GTR employing observed red shifts and decay periods of certain particles (physical clocks of identical construction). So, in standard GTR it is indeed the case that we understand distant simultaneity through an understanding of local simultaneity. We might even idly amuse ourselves by finding the precise time on our local clock corresponding to the clock-reading on the sun of five o'clock--that is, we might idly do so in a thought experiment. Hence, with the GTR we can indeed know what it means to say, "It's five o'clock on the sun," though aside from demonstrating how the GTR works, this would be a mere idle amusement.

For the reasons just stated, I conclude that the ordinary definition of physical congruence is an ordinary part of the ordinary practice of measuring, and that the "equally pejoratively ad hoc" objection fails even if we start out with the minimal congruence definition suggested in the objection. The foregoing looks like adequate argument to me on both points, but if someone were to wish to hold a "minimal" definition of physical congruence, then I simply wish to claim my series of assertions against his, and to declare mine more plausible. No? Very well then, we can declare an impasse of intuitions about ordinary physical congruence, though I think this only obscures the truth of the matter. And I note in passing that in every case of my own experience (many of them over nearly 20 years), when I first introduce the ordinary definition of physical congruence to any audience there is universal acceptance of it as beyond any reasonable doubt as part of what we are doing when we measure; and, audiences are likewise universally amazed and perplexed by the genuinely interesting and initially plausible conventionalist arguments. This last, of course, is no argument, only instead is it evidence about broadly shared intuitions regarding the ordinary definition of physical congruence.

> "So you are saying that human agreement decides what is true and what is false?"--It is what human beings *say* that is true and false; and they agree in the *language* that they use. That is not agreement in opinions but in form of life. (Wittgenstein 1958 §241)

Finally, two minor points. First, should someone wish to stand on intuitions contrary to the ordinary definition of physical congruence offered here, this stand is unlikely to be acceptable to any of the traditional advocates of the conventionalist position. The conventionalist position is that choice among empirically equivalent alternative physical theories can be based at most upon pragmatic grounds of elegance, simplicity, ease of application, etc. For conventionalists, cognitive grounds are strictly empirical grounds. But now the minimal congruence view is essentially connected with particular intuitions. Appeal to intuitions is not the sort of thing that any of the actual, historical conventionalists would be likely to accept. Secondly, none of the arguments here is intended to bear on what might be a proper interpretation of the philosophy of Wittgenstein, though I do think them compatible with at least a possible interpretation; but a discussion of such matters is far beyond the scope of this study.

To summarize the discussion of this section, I conclude that there is adequate counterargument against the line of objection raised, and hence that this line of objection fails; that this line of objection fails even if we begin with the "minimal" definition of physical congruence suggested in the objection; and, that if the matter of physical congruence definition is taken as an impasse of intuitions, then the ordinary definition of physical congruence offered here is more plausible and more widely shared than the suggested alternative.

5. Very General Natural Facts. Should anyone think that the arguments thus far somehow show that the ordinary concept of congruence is "absolutely correct", that it could not be abandoned under any circumstances (perhaps, that it is a synthetic necessary or synthetic a priori proposition), then he need only imagine certain very general matters of fact to be different than they are, and then he will see how we might have different concepts and what that would

involve. (Wittgenstein 1958 p56&230) To illustrate, imagine that all measuring rods are unreliable, spontaneously expanding and contracting. I mean this in a way not explainable by regular physical changes (e.g. temperature, etc.), and I mean this as a general matter of experience. So that, for instance, when a carpenter measured an arch and cut a door to the size of the measurements that the door might sometimes (or always) not fit. Under such circumstances we should say that measuring had lost its point. (Wittgenstein 1958 §242) Measuring would not be an effective physical procedure under such circumstances. Measurement, as we know it, would be useless. But note that such is not the case for the conventionalist position (e.g. as in figure 2-1). Carpenters would have no such trouble there, even if they measured the arch and then relocated the building before installing the door: The changes in description B are so-called "universal changes." The arguments herein have been designed to show that if the ordinary physical definition of congruence is abandoned, then so too is the practice of physical measuring; and also, that a conventionalist needs measurements in order to make his argument. But in such circumstances as just imagined the practice of making measurements would be abandoned, and abandoned along with it would be the physical definition of congruence which is constitutive of it. However, as things are (both in the actual world, and for the imagined world of figure 2-1), the practice of making physical measurements continues; the ordinary definition of physical congruence is part of that practice; and therefore, description A is cognitively preferable to description B; and therefore also, standard GTR is cognitively preferable to the indefinitely large class of empirically equivalent alternatives posed by the conventionalist.

The just imagined circumstances were stated in order to throw light on the concept of physical congruence. Physical congruence is indeed a matter of stipulation and agreement. But it is an agreement that is necessary to the practice of making physical measurements. Such agreement is stipulated, but not stipulated arbitrarily; instead, it properly conforms with (seldom noticed) very general natural facts. This is what Wittgenstein has called agreement in "form of life." (1958 §241) As such, the ordinary definition of physical congruence is a paradigm of the practice of physical measuring. It is the standard by which physical <u>measurements</u> can be made. Can circumstances be imagined in which the paradigm should be abandoned? Oh yes! But these are not such circumstances as the conventionalist imagines. And where the paradigm is abandoned so too must be the practice, the form of life.

The conventionalist tells a story that is quite plausible in terms of our ordinary linguistic intuitions. It is not our intuitions which need to be consulted when we are presented with such a picture. We need instead to understand how this picture employs concepts, such as physical congruence, and whether this employment is coherent. (The employment may be coherent given a different, imaginable context.) In doing this we have described the status of the concept of physical congruence and of the alternative geometries in relation to physical theory of physical things.

In sum, this chapter argues that universal forces falter on pejorative _ad hocery_; that a metaphorical interpretation does not help; that even if we suspend criticism of the metaphorical interpretation and of universal forces, the conventionalist argument is a _reductio_ of itself; and, that the minimal-congruence-and-equally-pejoratively-_ad_-_hoc_ objection (section 4) fails. Finally, this chapter attempts a description of relations among conventions, practices and very general natural facts. We have come quite far, I believe, with our somewhat old-fashioned approach to the question of "the geometry of physical space." With a change of fashion in view we move on to the next chapter.

Postscript to Chapter 2

Although the following remarks are not needed for the arguments of this chapter, they may be of interest and they do display a kind of unity of argument in this work. The fact that both very general natural facts and the very general natural concept of the ordinary physical definition of congruence (by contrast to a geometry) are pretheoretical (in the sense that they are prior to any theory of science) does not prevent them from being implicit or direct ontological claims of a theory of physics. They are in fact direct or implicit ontological claims of every known theory of physics, though again it is quite possible that there could be alternatives like those imagined above, but not like those the conventionalists imagined. In brief, very general natural facts and concepts are contingent ontological claims; they are not *a priori* and they are not necessary claims. These points are straightforward consequences of some of the arguments of this chapter.

We do rebuild our ship at sea (Neurath 1933) working along on a fairly stable deck on a sea that does stay mostly below us, never suddenly moving above in its entirety all at once into the sky. On radically different seas we would (re)build, if we existed at all, a radically different ship or something not much resembling what we would call a ship. Of course, these last remarks are only metaphor but not the sort of metaphor of the conventionalist position.

CHAPTER 3

RECENT ANALYSES

> *Feynman said to Dyson, and Dyson agreed, that Einstein's great work had sprung from physical intuition and that when Einstein stopped creating it was because "he stopped thinking in concrete physical images and became a manipulator of equations."* (Gleick 1992 p244)

> *I wished to show that space-time is not necessarily something to which one can ascribe a separate existence, independent of the actual objects of physical reality. Physical objects are not in space, but these objects are spatially extended. In this way the concept "empty space" loses its meaning.*
>
> A. Einstein,
> June 9th, 1952
> (1952 pvi)

Apparently Einstein stopped to think in physical terms in 1952. As noted in chapter 2, some truly spectacular accounts of spacetime manifold models of physical theory (Friedman 1983, Earman 1989, Norton 1992) have appeared fairly recently. In order to address relevant points of these models to the question of the geometry of physical space it will be useful to give (1) a very brief account of relativity theory, and (2) a very brief account of manifold models. We then will be in a position to discuss (3) Friedman's (1983) response to the conventionalist position (I will call this the parsimony/unification response), and to follow that with a discussion of (4) Glymour's (1980) response (which I will call the confirmation analysis response). I will attempt to show that both Friedman's and

Glymour's responses are subject to criticism, and fail against a properly resourceful conventionalist. But should the reader find these criticisms themselves lacking, then I hope to find comfort in having provided in this work another cogent response to conventionalism (as presented in chapters 1 and 2).

1. Relativity Theory. By far the best and most accessible account of Relativity Theory is Einstein's own, Relativity, the Special and General Theory (1952). The following extremely brief description is intended to say enough to serve the purposes of this chapter.

Special Relativity. The Special Theory of Relativity (STR) is based in an analysis of relative uniform rectilinear motion, though it makes important predictions about acceleration and about mechanics. Before Einstein's work the motion of bodies and the action of forces had been based upon Newton's laws of motion, which involve constancy of mass and the simple method of obtaining relative velocities by vector addition of the velocities of the observer and the object observed. It was assumed that light was propagated through a stationary medium, the ether (considered to be at rest relative to absolute space), at a fixed velocity **c** relative to the ether. This meant that the velocity of light measured on earth should change as the earth moved through the ether. But carefully designed experiments failed to find any such difference. The best known such experiment, the Michelson-Morley experiment, indicated that the velocity of light was the same when measured in the direction of the earth's rotation and also when measured perpendicular to this direction. The observer always observed light to have the velocity, **c**. We examined in chapter 1 the various attempts to patch-up ether theory and found that these are ontologically-ad-hoc.

In his 1905, Einstein simply assumed that the velocity of light is the constant, **c**, for all inertial observers, no matter what their inertial motion, and hence that there is no standard of absolute rest. As Einstein notes in his 1905 (in Perrett and Jeffery 1952 p37) this assumption is fully supported by all observational evidence, and it leads to certain results for observations made on a system that is moving at a constant rectilinear velocity relative to the observer. An object of length, ℓ when at rest relative to an observer will be observed to have a length $\ell\cdot\sqrt{1-(v^2/c^2)}$ when moving at velocity **v** relative to the observer. The observed shortening is mathematically equivalent to the LFC of Lorentz's (1904) DAE and our TAE (chapter 1). A similar change occurs in the mass of objects: An object having a mass **m** when at rest relative to an observer has a mass $m\cdot\sqrt{1-(v^2/c^2)}$, when moving at a velocity **v** relative to the observer. This increase in mass is only significant at very high velocities near **c**. It produces the observed limiting velocity of particles in particle accelerators; if it were not the case, the energies employed in particle accelerators would produce particle velocities much greater than **c**. The increase of mass with velocity leads to the conclusion that mass and energy are interconvertible: Mass can be reduced with the production of an equivalent amount of energy and mass can be produced with an equivalent loss of energy. The two are related by Einstein's famous equation, $\mathbf{E = mc^2}$. Mass/energy interconvertability produces atomic power in nuclear reactors and in nuclear and thermonuclear explosions; it is also observed in spontaneous decay of radioactive elements. Another relativity effect is time dilation, which is again mathematically equivalent to the time dilation of Lorentz's (1904) DAE and our TAE. If an observer **A** measures the passage of time with a clock at rest relative to **A** and another observer **B** is moving at a uniform rectilinear velocity **v** relative to **A**, **A** will observe time as measured on **B**'s clock as given by $t/\sqrt{1-(v^2/c^2)}$; so, **A** observes time running more slowly in **B**'s reference frame by the time dilation factor. This effect is observed in particle accelerators and in observations of some particles in cosmic rays, which are observed to have longer than usual lifetimes because they are moving at relativistic (close to **c**) velocities. Hence, one of the consequences of special relativity theory is that there is no absolute value of time (simultaneity). Two observers moving relative to one another will assign different times to the occurrence of an event; and, observations of length and time are inextricably connected in the manner specified by the length contraction and time dilation equations. This inextricable connection provides a basis for the use of the term, space-time.

Throughout its development the STR is guided by the Special Principle of Relativity which may be stated thus: "All inertial reference systems are equivalent, in that the laws of physics do not single out any particular privileged class of rest systems independent of the distribution of matter (mass-energy)." (Healey 1987 p600)

General Relativity. The General Theory of Relativity (GTR) deals with gravitation and with accelerated relative motion. The central idea of the GTR is to generalize the Special Principle of Relativity to a principle holding for all motion. The General Principle of Relativity may be expressed as: "All reference systems are equivalent, in that the laws of physics do not single out any particular privileged class of reference systems independent of the distribution of matter (mass-energy)." (Healey 1987 p600) An observer who is travelling in a circular path experiences an acceleration in the direction of the center of the path and is subjected to an outward, centrifugal, force. This force is proportional to the observer's mass in the same way that the weight of an object resulting from gravitational attraction is proportional to the object's mass. Einstein notes that if the observer were in a sealed vehicle and were not aware of the constant circular motion (or alternatively, of a constant linear acceleration), he might ascribe the force to a gravitational attraction by a body outside the vehicle. In other words, the effects of gravity due to a body's gravitational field, are mathematically and kinesthetically equivalent to a force produced by a constant circular or constant linearly accelerated motion. This idea is used in the GTR to describe gravitational attraction and the force of acceleration and centrifugal force as properties of interaction with physical gravitational fields (of physical bodies) which constitute space-time. The gravitational force experienced by a body (mass-energy) in relation to another body (mass-energy) is described mathematically in the GTR by assigning nonEuclidean geometric (g-metric) functions to physical gravitational fields that constitute space-time. As Einstein puts it,

> We are now in a position to see how far the transition to the general theory of relativity modifies the concept of space. In accordance with classical [Newtonian] mechanics and according to the special theory of relativity, space (space-time) has an existence independent of matter or field.... On the basis of the general theory of relativity, on the other hand, space as opposed to "what fills space", which is dependent on the co-ordinates, has no separate existence. Thus a pure

> gravitational field might have been described in terms of the g_{ik} (as functions of the co-ordinates), by solution of the gravitational equations. If we imagine the gravitational field, i.e. the functions g_{ik}, to be removed, there does not remain a space of the type [of the STR], but absolutely *nothing*, and also no "topological space". For the functions g_{ik} describe not only the field, but at the same time also the topological and metrical structural properties of the manifold. A space of the type [of the STR], judged from the standpoint of the general theory of relativity, is not a space without field, but a special case of the g_{ik} field.... There is no such thing as an empty space, i.e. a space without field. Space-time does not claim existence on its own, but only as a structural quality of the field.
>
> Thus Descartes was not so far from the truth when he believed he must exclude the existence of an empty space. The notion indeed appears absurd, as long as physical reality is seen exclusively in ponderable bodies. It requires the idea of the field as the representative of reality, in combination with the general principle of relativity, to show the true kernel of Descartes' idea; there exists no space "empty of field". (Einstein 1952 p154-156) [my edit; my emphasis in underline]

So, space-time is constituted by the physical, varying, gravitational field produced by all the matter (mass-energy) of the cosmos; and, that field is described in the GTR by the g-metric tensor (the g_{ik}) together with a field equation that specifies how much g-metric tensor (representing a gravitational field) is produced by any given amount of matter (mass-energy).

One way to see the significance of mass-energy equivalence and the physical gravitational field can be found in the following remarks by Steven Weinberg.

> Gravitational fields are generated not only by particle masses, but by all forms of energy. The earth is going around the sun a little faster than it otherwise would if the sun were not hot, because the energy in the sun's heat adds a little to the source of its gravitation. (Weinberg 1977 p135-136)

And,

> When Einstein in 1915 worked out the consequences of his new theory [the GTR], he found that it immediately explained the excess precession of 43 seconds per century in the orbit of Mercury. (One of the effects that contributes to this extra precession in Einstein's theory is the extra gravitational field produced by the energy in the gravitational field itself. In Newton's theory gravitation is produced by mass alone, not energy, and there is no such extra gravitational field.) Einstein recalled later that he was beside himself with delight for several days after this success. (Weinberg 1992 p91-92)

To give a fuller sense of the physical significance of relativity theory it will be useful to consider the following further passages from Weinberg:

> Einstein in developing general relativity had pursued a line of thought that could be followed by the subsequent generations of physicists who would set out to learn the theory and that would exert over them the same seductive qualities that had attracted Einstein in the first place. We can trace the story back to 1905, Einstein's *annus mirabilus*. In that year, while also working out the quantum theory of light and a theory of the motion of small particles in fluids, Einstein developed a new view of space and time, now called the special theory of relativity. This theory fit in well with the accepted theory of electricity and magnetism, Maxwell's electrodynamics. An observer moving with constant speed would observe space and time intervals and electric magnetic fields to be modified by the observer's motion in just such a way that Maxwell's equations would still be valid despite the motion (not surprising, because special relativity was developed

specifically to satisfy this requirement). But special relativity did not fit at all well with the Newtonian theory of gravitation. For one thing, in Newton's theory the gravitational force between the sun and a planet depends on the distance between their positions *at the same time*, but in special relativity there is no absolute meaning to simultaneity--depending on their state of motion, different observers will disagree as to whether one event occurs before or after or at the same time as another event.

There were several ways that Newton's theory could have been patched up so that it would be in accord with special relativity, and Einstein tried at least one of them before he came to general relativity. The clue that in 1907 started him on the path to general relativity was a familiar and distinctive property of gravitation: the force of gravity is proportional to the mass [mass-energy] of the body on which it acts. Einstein reflected that this is just like the so-called inertial forces that act on us when we move with a non-uniform speed or direction [acceleration]. It is an inertial force that pushes passengers back in their seats when an airplane accelerates down the runway. The centrifugal force that keeps the earth from falling into the sun is also an inertial force. All these inertial forces are, like gravitational forces, proportional to the mass [mass-energy] of the body on which they act. We on earth do not feel either the gravitational field of the sun or the centrifugal force caused by the earth's motion around the sun because the two forces balance each other, but this balance would be spoiled if one force was proportional to the mass [mass-energy] of the objects on which it acts and the other was not; some objects might then fall off the earth into the sun and others could be thrown off the earth into interstellar space. <u>In general the fact that gravitational and inertial forces are both proportional to the mass [mass-energy] of the body on which they act but depend on no other property of the body makes it possible at any point in any gravitational field to identify a "freely falling frame of reference" in which neither gravitational nor inertial forces are felt because they are in perfect balance for all bodies</u>. When we do feel gravitational or inertial forces it is because we are not in a freely falling frame. For example, on the earth's surface freely falling bodies accelerate toward the center of the earth at 32 feet per second per second, and we feel a gravitational force unless we happen to be accelerating downward at the same rate. Einstein made a logical jump and guessed that gravitational and inertial forces were at bottom the same thing. He called this <u>the principle of equivalence of gravitation and inertia</u>, or the equivalence principle for short. <u>According to this principle, any gravitational field is completely described by telling which frame of reference is freely falling at each point in space and time</u>.

Einstein spent almost a decade after 1907 searching for an appropriate mathematical framework for these ideas. Finally he found just what he needed in a profound <u>analogy</u> between the role of <u>gravitation in physics</u> and that of <u>curvature in geometry</u>. The fact that the force of gravity can be made to disappear for a brief time over a small region around any point in a gravitational field by adopting a suitable freely falling frame of reference is just like the property of curved surfaces, that we can make a map that despite the curvature of the surface correctly indicates distances and directions in the immediate neighborhood of any point we like. If the surface is curved, no one map will correctly indicate distances and directions everywhere; any map of a large region is a compromise, distorting distances and directions in one way or another. The familiar Mercator projection used in maps of the earth gives a good idea of distances and directions near the equator, but produces horrible distortions near the poles, with Greenland swelling to many times its actual size. <u>In the same way, it is one sign of being in a gravitational field that there is no *one* freely falling frame of reference in which gravitational and inertial effects cancel everywhere</u>.... In its final form, the general theory of relativity was just a reinterpretation of the existing mathematics of curved spaces in terms of gravitation, together with a *field equation* that specified the curvature produced by any given amount of matter and energy. Remarkably, for the small densities and low velocities of the solar system, general relativity gave just the same results as Newton's theory of gravitation, with the two theories distinguished only by tiny

> effects like the precession of orbits and the deflection of lights [deflection of starlight passing close to the sun]. (Weinberg 1992 p98-101) [my edit; my emphases in underline]

And again from Weinberg,

> Newtonian physics did explain virtually all the observed motions of the solar system, but at the cost of introducing a set of somewhat arbitrary assumptions. For example, consider the law that says that the gravitational force produced by any body decreases like the inverse square of the distance from the body. In Newton's theory there is nothing about an inverse-square law that is particularly compelling. Newton developed the idea of an inverse-square law in order to explain known facts about the solar system, like Kepler's relation between the size of planetary orbits and the time it takes planets to go around the sun. Apart from these observational facts, in Newton's theory one could have replaced the inverse-square law with an inverse-cube law or an inverse 2.01-power law without the slightest change in the conceptual framework of the theory. It would be changing a minor detail in the theory. Einstein's theory was far less arbitrary, far more rigid. For slowly moving bodies in weak gravitational fields, for which one can legitimately speak of an ordinary gravitational force, general relativity *requires* that the force must fall off according to an inverse-square law. It is not possible in general relativity to adjust the theory to get anything but an inverse square law without doing violence to the underlying assumptions of the theory.
>
> Also, as Einstein particularly emphasized in his writing, the fact that the force of gravity on a small object is proportional to the object's mass [mass-energy] but depends on no other property of the object appears rather arbitrary in Newton's theory. The gravitational force might in Newton's theory have depended for instance on the size or shape or chemical composition of the body without upsetting the underlying conceptual basis of the theory. In Einstein's theory the force that gravity exerts on any object *must* be both proportional to the object's mass and independent of any other of its properties {Strictly speaking, this is only for slowly moving small objects

> [whose energy is insignificant]. For a rapidly moving object the force of gravity also depends on the object's momentum [significant energy]. This is why the gravitational field of the sun is able to deflect light rays, which have momentum but no mass.} [So 'mass-energy' is the more perspicuous terminology and the general statement of this point is, <u>the force of gravity on an object is proportional to the object's mass-energy but depends on no other property of the object</u>. I]f this were not true, then gravitational and inertial forces would balance in different ways for different bodies, and it would not be possible to talk of a freely falling frame of reference in which no body feels the effects of gravitation. This would rule out the interpretation of gravitation as a geometric effect of the curvature of space-time [physically constituted by the cosmic varying gravitational field]. So, again, Einstein's theory had a rigidity that Newton's theory lacked, and for this reason Einstein could feel that he had explained the ordinary motions of the solar system in a way that Newton had not. (Weinberg 1992 p105-106) [my edit; Weinberg's footnote within braces {}]

And finally from Weinberg,

> In general relativity the underlying principle of symmetry [General Principle of Relativity] states that *all* frames of reference are equivalent: the laws of nature look the same not only to observers moving at any constant speed but to all observers, whatever the acceleration or rotation of their laboratories. Suppose we move our physical apparatus from the quiet of a university laboratory, and do our experiments on a steadily rotating merry-go-round. Instead of measuring directions relative to north, we would measure them with respect to the horses fixed to the rotating platform. At first sight the laws of nature will appear quite different. Observers on a rotating merry-go-round observe a centrifugal force that seems to pull loose objects to the outside of the merry-go-round. If they are born and grow up on the merry-go-round and do not know that they are on a rotating platform, they describe nature in terms of laws of mechanics that incorporate this centrifugal force, laws that appear quite different from those discovered by

the rest of us.

The fact that the laws of nature seem to distinguish between stationary and rotating frames of reference bothered Isaac Newton and continued to trouble physicists in the following centuries. In the 1880s the Viennese physicist and philosopher Ernst Mach pointed the way toward a possible reinterpretation. Mach emphasized that there was something else besides centrifugal force that distinguishes the rotating merry-go-round and more conventional laboratories. From the point of view of an astronomer on the merry-go-round, the sun, stars, galaxies--indeed, the bulk of the matter of the universe--seems to be revolving around the zenith. You or I say that this is because the merry-go-round is rotating, but an astronomer who grew up on the merry-go-round and naturally uses *it* as his frame of reference would insist that it is the rest of the universe that is spinning around him. Mach asked whether there was any way that this great apparent circulation of matter could be held responsible for centrifugal force. If so, then the laws of nature discovered on the merry-go-round might actually be the same as those found in more conventional laboratories; the apparent difference would simply arise from the different environment seen by observers in their different laboratories.

Mach's hint was picked up by Einstein and made concrete in his general theory of relativity. <u>In general relativity there is indeed an influence exerted by the distant stars that creates the phenomenon of centrifugal force in a spinning merry-go-round: it is the force of gravity</u>. Of course nothing like this happens in Newton's theory of gravitation, which deals only with a simple attraction between all masses. General relativity is more complicated; <u>the circulation of the matter of the universe around the zenith seen by observers on the merry-go-round produces a field somewhat like the magnetic field produced by the circulation of electricity in the coils of an electromagnet. It is this "gravitomagnetic" field that in the merry-go-round frame of reference produces the effects that in more conventional frames of reference are attributed to centrifugal force</u>. The equations of general relativity, unlike those of Newtonian mechanics, are precisely the same in the merry-go-round laboratory and conventional laboratories; the difference between

> what is observed in these laboratories is entirely due to their different environment--a universe that revolves around the zenith or one that does not. But, if gravitation did not exist, this reinterpretation of centrifugal force would be impossible, and the centrifugal force felt on a merry-go-round would allow us to distinguish between the merry-go-round and more conventional laboratories and would thus rule out any possible equivalence between laboratories that are rotating and those that are not. *Thus the symmetry among different frames of reference* [the General Principle of Relativity] *requires the existence of gravitation*. [And the GTR is very well confirmed.] (Weinberg 1992 p142-144) [my edit; my emphases in underline]

With this last passage of Weinberg's in mind it will help to display the sort of thinking in concrete physical terms that unifies the Special and General Theory of Relativity to consider the opening words of Einstein's famous 1905:

> It is known that Maxwell's electrodynamics--as usually understood at the present time--when applied to moving bodies, leads to asymmetries which do not appear to be inherent in the phenomena. Take, for example, the reciprocal electrodynamic action of a magnet and a conductor. The observable phenomenon here depends only on the relative motion of the conductor and the magnet, whereas the customary view draws a sharp distinction between the two cases in which either the one or the other of these bodies is in motion. For if the magnet is in motion and the conductor at rest, there arises in the neighbourhood of the magnet an electric field with a certain definite energy, producing a current at the places where parts of the conductor are situated. But if the magnet is stationary and the conductor in motion, [the customary view holds that] no electric field arises in the neighbourhood of the magnet. In the conductor, however, we find an electromotive force, to which in itself there is no corresponding energy, but which gives rise--assuming equality of relative motion in the two

> cases discussed--to electric currents of the same path and intensity as those produced by the electric forces in the former case. (Einstein 1905 in Perrett & Jeffery 1952 p37) [my edit]

The parallel thinking in concrete physical terms for the STR (relativistic electrodynamics) and the GTR (relativistic gravitation) is apparent. In both cases an observed force arises with <u>relative</u> motion. In both cases a physical explanation is provided by a field. For the magnet and coil, the field is the electro-magnetic field, <u>i.e</u>. the magnetic field is there all along and the electro-magnetic field arises with relative motion and produces the observed force. For the turntable and remainder-of-the-cosmos, the field is the "gravito-magnetic" field, <u>i.e</u>. the gravitational field is there all along and the "gravito-magnetic" field arises with the relative motion and produces the observed force.

As far back as Newton's time it was held that Newton's laws of motion applied <u>exactly</u> only in a pure empty inertial physical space, even though physicists had no such physical space in which to perform experiments. The motion of a Foucault pendulum, for example, would not take place, according to Newton's laws, in an inertial space and it is for this reason that this motion is sometimes said to reveal the rotation of the earth. (<u>e.g</u>. Norton 1992 p182) But Newton's laws apply <u>approximately</u> near the surface of the earth and in most cases the slight deviations are impossible to detect. Formerly, it was thought that Newton's laws exactly apply far off in physical interstellar space, or at least that they would exactly apply there to idealized point-masses (test bodies). The same sort of applicability was considered to be the case for the STR. But the GTR holds that there is no such pure empty physical space. So, when it is said that the STR is a special case of the GTR, this can be understood to mean that the STR describes a special case of the *g*-metric gravitational field of the GTR.

It is said that the GTR holds that the geometry of physical space-time is nonEuclidean, and though this is true enough, it would be more perspicuous to say something like the following about the GTR: Space-time is constituted by the physical varying cosmic gravitational field that is a property of all the matter (mass-energy) of the cosmos. The GTR describes the (total) field by mathematically describing (all) the free-fall paths of the field. That description is provided by the nonEuclidean *g*-metric tensor together with the field equation that tells how much gravitational field strength (intensity) (quantitative value of *g*-metric tensor) results from any given distribution of matter (mass-energy). Since the distribution is constantly changing, so also is the field. It is in this physical sense that the geometry of physical space-time is a varying nonEuclidean geometry. The application of geometric functions to physical theory is in this sense no different than the applications of, say, the functions of differential calculus. It is useful, I believe, to have these points (the physical as contrasted with mathematical relations) in relativity theory before us as we turn to manifold models.

2. Manifold Models. I will attempt to limit the presentation of manifold models to the extent that it serves the purposes of this chapter. The idea of a **manifold model** of a physical theory is to represent physical intervals and physical points in a four dimensional (three dimensions for the usual spatial dimensions and one dimension for time) continuum of the real numbers. To do this a four dimensional continuum of the real numbers is modified in certain ways in order to represent physical spatial and temporal intervals and points. This modified continuum of the real numbers in four dimensions, the **manifold**, is designated as M.

The word 'dimension' here plays a dual role. **Dimension** has a physical and empirical sense in our ordinary observations of front-to-back, side-to-side, up-to-down, and before-to-after. In a manifold model to speak of a 'dimension' is simply to refer to one linear continuum of real numbers; with four of these, the manifold model can represent physical events, the motion of objects, etc. To help maintain this distinction regarding the term 'dimension' I will adopt the somewhat peculiar convention of spelling the term for a 'manifold dimension' as **d@mension**, while keeping the usual spelling for a physical dimension. Also, it may be worth noting that in a manifold model, strictly speaking, nothing moves. That is to say, states of motion or rest are represented by sets of real numbers in four d@mensions. So, in this sense manifold models present a static representation of the dynamic phenomena of physical motions.

Newtonian Mechanics (NM), Special Relativity (STR) and General Relativity (GTR) are physical theories that can be represented in manifold models. The only known way of stating the GTR requires the use of a concept of manifold models (the *g*-metric tensor). NM and STR may be stated without explicitly employing a concept of manifold models. A motivation for representing alternative physical theories in manifold models is to present differences between them, while employing the same (manifold model) mode of representation.

The physical concept of an **instant** may be taken to be that of a physical point designated by a physical object or event in three physical spatial dimensions at a physical time designated by a physical clock; an instant also may be designated in relation to objects or events. An instant may be represented in a manifold model by a set of four real numbers (three numbers, one for each of the usual spatial dimensions, and one number for time).

TABLE 3-1

Pronunciation Guide

The symbols employed here are pronounced as follows. (Free-standing English letters on the right are pronounced as in standard English.)

$\mathbf{M}$	Mu
$\mathbb{R}_4$	R four (also Hollow R four)
$\mathbb{R}'_4$	R four prime
$\mathbb{R}''_4$	R four double prime
γ	gamma (lower case Greek letter)
$d\mathbf{T}$	d Tau (**T** is the upper case Greek letter, tau)
h	h
∇	Nabla (also Gradient)
η	ā'tă (η is the lower case Greek letter, eta)
g	g

TABLE 3-1

In order to reach the (more or less) standard notation, **M**, for the manifold we begin with $\mathbb{R}_4$, the continuum of the real numbers in four d@mensions. With two modifications to $\mathbb{R}_4$, we will reach the manifold, **M**, which can then be used in a manifold model to represent physical theories of the physical world.

In contrast to objects and events, the set of instants is **homogeneous** <u>i.e</u>. every instant is exactly like every other. The set of instants is **isotropic** <u>i.e</u>. future and past directions are exactly the same; and, side-to-side, up/down, back/forth are exactly same. In a manifold model sets of four real numbers represent an instant. This representation is called a **coordinate system**. The denseness of $\mathbb{R}_4$ represents the apparent denseness of space and time with no space or time "atoms"; space and time are modelled as **continuous**. But since $\mathbb{R}_4$ is **anisotropic** i.e. numbers greater and lesser than zero (positive and negative numbers) are not exactly the same, we allow for the representation of any "reflection about zero" to be considered the same; call $\mathbb{R}_4$ with this modification $\mathbb{R}_4'$. $\mathbb{R}_4'$, however, is **inhomogeneic** (e.g. the number 9 is different in quantity from the number 2.4; e.g. the numbers 1,3,5,7,11... are primes, while 2,4,6... are not) but instants are homogeneic; so, to eliminate the **inhomogeneity** of $\mathbb{R}_4'$ we allow any **translation along** (sliding along) the scale of real numbers to be considered the same, and thus we reach $\mathbb{R}_4''$. These two stipulations are called allowing any **reflection** and any **translation**, and with these two stipulations we arrive at $\mathbb{R}_4''$, the manifold of the real numbers properly modified to represent physical instants. $\mathbb{R}_4''$ is **M** in (more or less) standard notation, so we will drop $\mathbb{R}_4''$ and continue with **M.**

Manifolds with reflection and translation are called **standard coordinate systems** of a theory; they are also called the **covariance group** of a theory; this is also stated by saying that the theory is **covariant** under reflections and translations or that the theory is subject to **covariant transformation.**

An invariant of a transformation or a **symmetry of a transformation** is something that remains unchanged under the transformation. In a manifold model the factual or physically significant quantities of the model of a theory of space and time are the invariants of the model's covariance group; the set of the invariants of the model's covariance group is called the **symmetry group** of the model. All other quantities simply can be chosen arbitrarily. (Norton 1992 p198). The absolute value of coordinate differences in standard coordinate systems is an invariant of these systems and therefore is considered a physically significant representation. So, a standard manifold model represents physical distances and physical time-intervals as the absolute values of standard coordinate differences; standard coordinate systems, on the other hand, may be chosen arbitrarily.

A **generally covariant formulation** allows all the transformations of the standard formulation and also allows any transformation involving "stretching or squeezing" that preserves the "smoothness and uniqueness" of the "instants" (i.e. the real numbers that represent the instants). This additional kind of translation is called a **scale factor** and provides further **geometric structure**, it provides **metric**

structure. The "flat" scale factor of the standard coordinate systems discussed above is one (=1); this is the **Euclidean scale factor,** or **Euclidean metric tensor,** γ. A scale factor is a **covariant quantity,** i.e. once its value is given for one coordinate system the factor can be used to give its value in any other coordinate system; it is an invariant of a transformation, a symmetry of a transformation. Any covariant quantity may be called a **geometric object.** Geometric objects represent physically significant phenomena in a manifold model. A **scale factor dT** along with all of its possible coordinate transformations is called a **covector** or **one-form** or a **metric tensor** or may be called a **temporal metric** (**dT**) where it represents time (where the covector applies to the d@mension of the manifold that represents time).

The **generally covariant formulation** of a theory has models that consist of ordered pairs, < M, **dT,...**>. This is M, the modified manifold of the real numbers in covariant form (i.e. allowing for translations and reflections) along with a **scale factor** or **metric structure** or **metric tensor,** which picks out some "stretching or squeezing or flatness" status for the manifold, M. Any physical space and time theory can be modeled in generally covariant form as **<manifold, geometric object,...>** i.e. the manifold, M, plus one or more geometric objects. The **manifold,** M, represents the **topology** of the physical time and space represented. (The standard topology is the "smooth" M topology, but non-standard topologies can be stated such as ones with "tears" or "holes" in the continuum.)

The term **spacetime** is sometimes used for the manifold, M, but **spacetime** more often refers to a manifold plus one or more geometric object(s). In this way we can refer to Newtonian spacetime or Minkowski (STR) spacetime, etc. These are the manifold model uses of the term spacetime.

There is also a physical sense of space-time that may be designated by the spelling convention **space-time**. Physically, as summarized above, relativity theory shows that objective physical observations of length, duration, and before and after depend upon relative motion of reference frames. So, physical space and time are inextricably linked in the manner specified by the equations for length contraction and time dilation and by the *g*-metric tensor functions and field equation of the GTR, and as we noted above physical **space-time** is constituted by the cosmic (*g*-metric) gravitational field. This is the physically-inextricably-linked-sense of **space-time** of the physical world that a **spacetime** can represent in a manifold model. **Space-time** is a physical concept of Relativity Theory; **spacetime** is a manifold model concept that may be used to represent the physical claims of a variety of physical theories.

A manifold model of Newtonian Mechanics (NM) is represented by < **M**, **dT**, ***h***, ∇>, and may be summarized as follows: **M** is the modified four d@mensional manifold of the real numbers outlined above. The theory combines linear (absolute) time with Euclidean (absolute) geometry. So, time is represented in the model by the **dT** factor, **dT**=1. Three dimensional physical space is represented in the model by the (flat) Euclidean spatial metric (metric tensor) γ (γ=1), and γ is represented through all linear absolute time in the model by ***h***. ***h*** is the symbol representing the flat Euclidean three-d@mensional spatial metric, γ in the model, through all of linear absolute time represented in the model. At each point of linear absolute time represented in the model there is the entire three-d@mensional manifold of Euclidean space represented in the model and each of these representations is called a **hypersurface of simultaneity.** ∇ represents the **straight line structure**, or **affine structure** of Newtonian spacetime in the model (through all hypersurfaces of Newtonian simultaneity); ∇ picks out the set of **curves** (linear continua of real numbers) in **M** that represent the paths of inertial motion in the physical world according to Newtonian Mechanics (according to the Galilean Transformation, to be discussed below). The paths of inertial motion are represented by the straight line curves in M designated by ∇.

There is in Newtonian Mechanics a Newtonian Principle of Relativity, even though Newtonian Mechanics postulates an absolute space and an ether at rest relative to this absolute space. This **Newtonian Principle of Relativity** is that the laws of nature are the same for every reference frame in inertial (uniform, rectilinear) motion relative to absolute space. This is a **symmetry** or an **invariance principle** in Newtonian Mechanics. This means that from the point of view of the application of the laws of nature any inertial reference frame can be treated as the absolute reference frame. In the Newtonian spacetime manifold model this amounts to the symmetry of the laws of nature as we transform from any represented inertial frame to any other represented inertial frame; this is called **inertial transformation** (both physically and in the model). So, for the application of the laws of nature, the designation of any particular inertial frame as in absolute rest (both physically and in the model) is purely a matter of arbitrary choice. Hence, as expressed <u>in the language of manifold models</u> the **Newtonian Principle of Relativity** is: An inertial transformation is a symmetry of a Newtonian spacetime; it leaves Newtonian spacetime unchanged. (Norton 1992 p211)

Finally, the Newtonian spacetime manifold model properly represents the physical claims regarding inertial transformations of Newtonian Mechanics. In Newtonian Mechanics an inertial transformation from one physical inertial frame (t,x,y,z) to another physical inertial frame (t',x',y',z'), moving in the x direction relative to the first frame, is given by the **Galilean Transformation**, viz. $t'=t$, $x'=x-vt$, $y'=y$, $z'=z$, where t represents absolute time; x,y, and z represent the three spatial dimensions; and, v represents the velocity of the second frame (t',x',y',z') relative to the first frame (t,x,y,z) in the x direction. The Galilean Transformation is the transformation for the simple addition of velocities. It is the Galilean transformation that is violated by the observed constant speed of light, **c**, in different reference frames in such experiments as the famous Michelson-Morely experiment.

A manifold model of Special Relativity (STR) is represented by $< \mathbf{M}, \eta >$. This is known as **Minkowski Spacetime** (in honor of its inventor, 1907) and may be summarized as follows: The STR is a theory of inertial (uniform, rectilinear) motion employing the **Lorentz Transformation** to describe an inertial transformation (as above) from one physical reference frame (t,x,y,z) to another (t',x',y',z') thus: $t'=(1/\sqrt{1-v^2/c^2})(t-vx/c^2)$, $x'=(1/\sqrt{1-v^2/c^2})(x-vt)$, $y'=y$, $z'=z$, where the symbols are as above in the previous section except that t and t' are time as measured by clocks of the respective frames, and **c** is the constant speed of light. In the model, η is the geometric object, the **metric tensor** that uniquely captures the Lorentz Transformation for the model and η replaces h of Newtonian spacetime. So, the model properly represents the STR, and since the STR does not employ absolute space nor absolute time, there is no need in the model for counterparts to **dT** and **∇**, because the temporal and straight line (representing inertial paths) structures required by the Lorentz Transformation are already captured by η in the model. η picks out the **Lorentz group**, the set of all **curves** (sequences of real numbers designated by a scale factor, metric tensor) representing all physical inertial paths required by the Lorentz Transformation. The Lorentz group is both the model's covariance group and its symmetry group, i.e. in the model every transformation of reflection or translation produces the very same set of invariants.

The **Principle of Relativity for the STR** is: "All inertial reference systems are equivalent, in that the laws of physics do not single out any particular privileged class of rest systems independent of the distribution of matter (mass-energy)." (Healey 1987 p600) This is a symmetry principle and it may be expressed in the language of the manifold model of the STR as: An inertial transformation is a symmetry of a Minkowski spacetime; it leaves Minkowski spacetime unchanged.

A manifold model of General Relativity (GTR) is represented by $< M, g>$. M is as above. g is the g-metric tensor as described above in the account of the GTR. In the model g picks out the free-fall curve structure (sequences of real numbers designated by a scale factor, metric tensor) for represented objects (mass-energy) in a gravitational field given a represented distribution of matter (mass-energy). η is then seen as the special case of g where no representation of gravitation/acceleration is involved.

The **Principle of Relativity for the GTR** is: "All reference systems are equivalent, in that the laws of physics do not single out any particular privileged class of reference systems independent of the distribution of matter (mass-energy)." (Healey 1987 p600) There is not any straightforward way to express this symmetry principle in the language of a manifold model of the GTR, because the distinct distribution of matter (mass-energy) around each particular physical object (mass-energy) produces a varying physical gravitational field (and gravitational fields themselves, being energy, produce more gravitational field), and for the model to represent this it must present a properly varying g-metric-tensor-set-of-curves, around the representation of each physical object (mass-energy) in the model. For example it would be false to say something like, "A free-fall transformation is a symmetry of a g-metric spacetime; it leaves g-metric spacetime unchanged." This would be false because g-metric spacetime is changed by a free-fall transformation; a distribution of matter (mass-energy) affects the overall gravitational field of the cosmos represented by the g-metric-tensor-designated-set-of-curves in the model. The field equation relating a given distribution of matter (mass-energy) to a particular state of the cosmic gravitational field comes into play to produce distinct representational models for each possible distribution. Again, spacetime manifold model representations are static representations of dynamic physical phenomena. Since any distinct distribution of matter (mass-energy) produces a distinct cosmic gravitational field, there are as many distinct models as there are distinct distributions. This is the rather odd way in which the symmetry principle of General Relativity <u>is</u> represented in the exceptionally large set of <u>distinct</u> covariant models $< \mathbf{M}, g>$. <u>In the language of manifold models this may be expressed as follows</u>: In the transition from η to g we eliminate the last part of the identity noted above for a manifold model of the STR, <u>viz</u>. η picks out the Lorentz group, which is the model's covariance group, which is the model's symmetry group. In a GTR model g picks out the model's **general covariance group** which is <u>not</u> identical to a symmetry group of the model.

It takes a given distribution of matter (mass-energy) along with the GTR field equation to designate a *g*-set-of-curves for a manifold model. The *g*-metric tensor is included in the laws of nature along with the field equation; with this inclusion of the *g*-metric tensor in the laws of nature the General Principle of Relativity holds, and this is why in the manifold model the covariance group cannot be a symmetry group of the model. So, there must be an indefinitely large class of distinct models < **M**, *g*> of the GTR. An indefinitely large class of indistinguishable models (i.e. covariance group is identical to symmetry group) for NM and STR have been with us all along, but there is no particular point in discussing them, for there is no physical difference called for by their respective theories for their respective models to represent. Finally, none of these points detracts from the point that the General Principle of Relativity is a symmetry principle of the physical world described by the GTR. The laws of nature according to the GTR are invariant as we transform from one frame of reference to another, regardless of the state of motion of these reference frames. So it is appropriate to emphasize here once more Healey's [1987 p600] insightful formulation of the General Principle of Relativity: "All reference systems are equivalent, in that the laws of physics do not single out any particular privileged class of reference systems independent of the distribution of matter (mass-energy)."

This concludes our summary of manifold models. The reader will no doubt note that this summary is not accompanied by the usual spacetime diagrams with time represented on the verticle axis and distance represented in one d@mension on the horizontal axis. Diagrams are beautifully provided in Friedman 1983, Earman 1989, and Norton 1992, but diagrams are not essential in order to discuss the points at issue here. I must admit, as well, that the difficulties involved in the text processing of such diagrams served as a motivation to me to write the summary without them (the codes for symbols alone are fast filling the present disk), but the reason for the absence of diagrams is that they are not needed for the present discussion. One further rationale for their omission may be that this omission tends to eliminate the rather natural but mistaken transition to viewing such diagrams as pictures of physical space and time; as we have noted earlier "nothing moves" in these diagrams; they are heuristic devices which help in the "visualization" of manifold models of physical theories of the physical world.

3. <u>The Parsimony/Unification Response</u>. In a truly extraordinary intellectual accomplishment Friedman (1983)

presents a very rich series of manifold models for physical theories including not only those summarized above, but also models for both classical and relativistic electrodynamics and several other models as well. In this work Friedman also presents arguments regarding standard and nonstandard simultaneity, spacetime realism, and other matters related to relativity theory that are beyond the scope of the present work; and, Friedman develops the following argument against the conventionalist position: The basic argument is one by analogy to realism regarding molecules. Friedman argues that realism about molecules in both kinetic gas theory and in atomic structure/chemical bonding theory creates "boosts" of confirmation for the conjoined (unified) theories that are not possible if molecules are not taken to be real. Normally, Friedman argues, parsimony rules out taking any particular theoretical structure as real unless that particular structure provides for a unification which in turn allows boosts in confirmation of the unified theories. The boosts in confirmation consist in the fact that, once the particular theoretical structure is taken as the same real thing claimed by both theories, then each of the theories is confirmed not only by its own confirmations but also by the confirmations of the other theory. Molecules, Friedman argues, are just such a structure, and the analysis of the molecules-case provides a basis for making determinations about the reality of other theoretical structures posed by physical theories. Basically then, parsimony rules-out a theoretical structure as real unless unification (of the kind just described) rules it in.

Employing some of the notation developed in the previous section, Friedman's argument against the conventionalist position can be stated thus: If we replace the standard GTR metric tensor g with another metric tensor **h** plus a universal force term **U**, such that **U+h**=g, we note immediately that the **U+h** structure is extra theoretical structure with no unifying power, so the principle of parsimony rules-out the **U+h** structure in favor of the g structure. So, the g structure is favored over any one of the indefinitely large class of **U+h** structures and the conventionalist position is defeated.

Now <u>a conventionalist may argue against this view</u> by arguing against the analogy basis. For, the molecules of kinetic gas theory are taken to be perfect spheres (like ping-pong balls), while the molecules of molecular structure/chemical bonding theory are decidedly irregular in shape (lumpy and often with extended protrusions), so there is at least a <u>prima facie</u> case to be made that molecules are not <u>the same</u> theoretical structure posed in the two theories. In an alternative, the conventionalist can argue that the **U+h** structure does indeed unify in just the sort of way that Friedman has advocated. For, experience can provide massive confirmation of Euclidean physical geometry, and all previous physical theory explains observed physical effects with forces. So, with a **U+h** structure we get the sort of boosts in confirmation that the parsimony/unification response advocates. And the conventionalist can argue that if the parsimony/unification argument supports anything, then it supports the rejection of the *g* structure in favor of the Euclidean **U+h** structure.

But more likely, the conventionalist would use the arguments just stated only to indicate the malleability of the parsimony/unification kind of argument. The conventionalist can then go on to hold that this malleability is just the sort of thing that one expects from pragmatic considerations (cf. Hanson 1958 p54), and that the parsimony/unification argument is perfectly compatible with the conventionalist position, that to justify a choice among empirically equivalent alternative theories we must turn to pragmatic grounds of elegance, descriptive simplicity, ease of application, etc. The conventionalist can argue that unity is a matter of elegance, and that parsimony is a matter of descriptive simplicity; so, the parsimony/unification response is just a restatement of the conventionalist position. I conclude that the parsimony/unification argument fails against the conventionalist position, and turn to consideration of the confirmation analysis response in the next section.

But before moving on and quite independent of arguments regarding the conventionalist position presented here, it may be appropriate to digress for a moment to point out that there are good grounds to be suspicious of an analysis that purports to generate acceptable ontology out of parsimony and unification regarding theoretical structures. As Richard Feynman once remarked,

> The way I think of what we're doing is that we're exploring. We're trying to find out as much as we can about the world. People say to me, Are you looking for the ultimate laws of physics? No, I'm not. I'm just looking to find out more about the world. And if it turns out that there is a simple ultimate law that explains everything, so be it; that would be very nice to discover. If it turns out that it's like an onion with millions of layers and we're just sick and tired of looking at the layers, then that's the way it is. But whatever way it comes out, it's Nature that's there and she's going to come out the way she is. And therefore, when we go to investigate her, we shouldn't pre-decide what it is we're trying to do, except to find out more about it. If you say, but your problem is, <u>Why</u> do you find out more about it; if you thought that you were trying to find out more about it because you were going to get an answer to some deep philosophical question, you may be wrong. It may be that you can't get an answer to that particular question by finding out more about the character of Nature. But I don't look at it that way. My interest in science is to simply find out about the world, and the more I find out, the better I feel. I like to find out. (NOVA 1982)

Parsimony and unification over theoretical structures are unlikely to turn out to be very reliable guides to ontology. (*cf*. Reid 1788 p470 Lehrer 1989 p289) Nature is the way she is independent of human interests in simplicity or in theoretical grandeur. In fact, one way to understand the role of molecules in kinetic gas theory and in atomic structure/chemical bonding theory is that the term 'molecule' is not the same theoretical concept in both theories. This is to say that the two theories deal with different physical properties (properties at different scales of investigation) of the same physical thing, the molecule. But the two theories (being theories about the properties of molecules at different scales of investigation) deal with quite different theoretical concepts. By analogy, one might say that the same physical thing, the planet Venus, is described as quite a different theoretical concept by a radio astronomer and by a planetary explorer employing a robot landing vehicle. Yet the two theoretical concepts are quite compatible as referring to the same physical thing; similarly, with molecules. Now parsimony and unification applied to these quite different theoretical concepts are not likely to produce any arguably defensible results about ontology. Of course, these last few paragraphs take us too far from the conventionalist position for any further discussion, so we instead turn to consideration of the confirmation analysis response.

4. *The Confirmation Analysis Response*. As we noted in

chapter 2 the general problem of underdetermination of a theory by observational evidence may be described as a situation in which two theories predict the same set of observations (all and only the same observations), and these observations are confirmed, but the two theories, although they are each internally consistent, contain claims which are contraries of each other. As we have seen Poincaré, Reichenbach, Grunbaum, Carnap, and Salmon have argued that this is the case with regard to alternative formulations of the GTR depending upon the geometry (Euclidean/nonEuclidean) and matching physical congruence definition used in these alternative formulations. Glymour approaches this issue as follows. From Sklar (1974), Glymour lists five possible positions regarding this underdetermination: (1) Maintain skeptical doubt about the possibility of knowing geometrical truths about the world; (2) hold that geometrical truths (about the world) are matters of convention; (3) argue that there are a priori grounds for choice in cases of empirical equivalence; (4) claim that empirically equivalent theories say the same thing; (5) deny there is any coherent sense of "empirical equivalence." Glymour poses and defends a sixth position. (As we have seen Friedman argues a seventh position, and chapters 1 and 2 of the present work offer an eighth approach.) Glymour's position is to argue that one theory is better tested by the body of evidence than the others, and that, although underdetermination <u>may</u> arise in some cases, it does not arise in the GTR/geometries case. Glymour outlines a number of senses in which one theory may be better tested than another:

- <u>a</u> standard confirmation and disconfirmation;
- <u>b</u> one theory contains more untested hypotheses than another, the one with fewer (more or less independently) untested hypotheses is better;
- <u>c</u> evidence may be more varied for one theory than another, the theory with more varied evidence is better;
- <u>d</u> one theory may explain phenomena in a more uniform way than another, the theory that explains in a more uniform way is better;
- <u>e</u> some hypotheses are more central to a theory than others, confirmation of central hypotheses is better than confirmation of less central (less important) hypotheses, so the theory that has more confirmation of more central hypotheses is better.

Glymour next describes a hypothetical case of teaching high school physics, Newtonian physics. Suppose an especially bright student, we'll call him Clever Hans, proposes an alternative to Newtonian Mechanics. Hans's theory postulates two theoretical entities, gorse and morse, such that Force = gorse + morse. Hans's new theory is empirically equivalent to Newtonian Mechanics. What should we say to Clever Hans? Glymour suggests that we should point out that there is no evidence to support the gorse + morse hypothesis and so criterion <u>b</u> above rules-out Hans's theory.

Glymour next describes Reichenbach's argument as follows: The GTR may be expressed differently by replacing the metric tensor, *g*, with a different metric tensor, **h**, plus a universal force **U**, such that **U+h=*g***. Glymour's response is as in the Clever Hans case. There is no evidence for the **U+h** hypothesis, so it is ruled out by criterion b.

Glymour next anticipates a Reichenbachian reply: The story so far leaves out important "assumptions" (Reichenbach calls these "coordinating definitions") connecting **U+h**, or *g* with material systems. But Glymour replies that these connecting assumptions in the **U+h** case involve an "enormous number" of claims in order to permit the specification of the metric, the affine structure, and the universal force; and, that since, for example, the equation of motion must include **U**, it can't be tested as independently as can standard GTR employing *g*. So, on these grounds criteria b, c, d and e favor standard GTR. (Glymour actually mentions only b and c here but it is easy to see how a case might be made employing d and e.)

Glymour anticipates a further response that argues as follows: The *g* theory uses the same kinds of coordinating definitions, "connecting assumptions" as the **U+h** theory. But Glymour replies that although this point is correct, the **U+h** theory requires "many more" connecting assumptions and allows any one to be tested <u>only</u> by using many others. So, again on ground <u>b</u> the *g* theory is preferable.

<u>How might a conventionalist defend the conventionalist position against the confirmation analysis response</u>? <u>First a few fairly minor points</u>: Glymour says that he is not taking one of Sklar's options, but criteria <u>b</u> thru <u>e</u> look suspiciously <u>a priori</u>, and that is Sklar's option 3. The conventionalist already accepts confirmation/disconfirmation (Glymour's criterion <u>a</u>), so a conventionalist can argue that <u>b</u> thru <u>e</u> are just unacceptable appeals to <u>a priori</u> criteria, not supported by empirical evidence. The conventionalist can further argue that if we are counting-up enormous numbers of claims, then each of the variable nonEuclidean geometries contains a great many claims (many more than "flat" Euclidean geometry); and so, the many-more-claims-argument does not actually hold, since each of the members of the indefinitely large class of alternative GTR formulations turns out to be about the same on this score. The conventionalist also simply may criticize criterion <u>b</u>, since no hypothesis can be meaningfully tested outside its theory; and, for example, relative to the data <u>then</u> available it would have been correct, based on criterion <u>b</u>, for Newton to have <u>rejected</u> the GTR, had some prescient philosopher been clever enough to think of it.

Moreover, the conventionalist can argue that if (more or less) independent evidence (_b_), more varied evidence (_c_), and more uniform explanation (_d_) are to be valued, then a flat-Euclidean-geometry-plus-nonordinary-congruence-version of the GTR compares quite favorably against standard GTR. For consider that if carefully examined with regard to the application of geometrical functions, all ordinary experience conforms with Euclidean physical geometry; all other physical theory presupposes or employs flat Euclidean physical geometry; and, all other physical theory accounts for physical effects with forces. So, the conventionalist can argue that a flat-Euclidean-nonstandard-GTR has more independent evidence, has more varied evidence, and provides more uniform explanation than does standard GTR. Hence, the conventionalist can argue that a case can be made in several ways based upon these criteria, so the criteria are far from decisive. Finally, the conventionalist can argue that criteria _b_ thru _e_ are somewhat vague and (as just argued) indeterminate; and, the conventionalist can argue that this is particularly so with regard to _e_: Discerning which hypotheses are more central is likely to be tricky at best, and is likely to depend upon the interests and purposes of the investigator. (_cf_. Hanson 1958 p54)

But perhaps the major reply that the conventionalist can raise to the confirmation analysis response is simply not to object at all. For the conventionalist can point out that he readily accepts confirmation/disconfirmation; and then the conventionalist can argue that criteria _b_ thru _e_ only elaborate the conventionalist position, because _b_ thru _e_ simply elaborate pragmatic and confirmational grounds for choice: Variety of evidence (_c_) just further elaborates confirmation/disconfirmation (_a_); criteria _b_ and _d_ elaborate descriptive simplicity; criteria _d_ and _e_ elaborate descriptive elegance; and, criteria _b_, _c_, and _d_ elaborate ease of application. In fact, the conventionalist can argue that the confirmation analysis response is saved from being merely a set of _a priori_ stipulations by being interpreted as elaborations of confirmation/disconfirmation plus pragmatic grounds of descriptive simplicity, descriptive elegance, ease of application, _etc_; and, that this does not detract from, but only properly adds to the value and insight of the criteria of the confirmation analysis response. So, the conventionalist can conclude that the confirmation analysis response is only a more fully developed version of the conventionalist position: grounds for theory choice among empirically equivalent alternative theories remain pragmatic grounds.

I conclude that the conventionalist has adequate defenses against, and can even co-opt, the confirmation analysis response, just as the conventionalist has adequate defenses against and/or can co-opt the parsimony/unification response.

Finally, it is worth noting that the manifold model formulation, **U+h=**g, helps to display the fact, noted above in chapter 2, that standard GTR adds no auxiliary hypothesis and adds no auxiliary hypothesis that revises the theory, while each of the indefinitely large class of empirically equivalent theories (employing some particular **U+h**) does add such an auxiliary hypothesis.

CHAPTER 4

CONCLUDING REMARKS

The great thing about science is that you get such a grand return in speculation from such a small investment of fact.

Mark Twain
(Day 1966 p212)

...if there is no solace in the fruits of our research, there is at least some consolation in the research itself. Men and women are not content to comfort themselves with tales of gods and giants, or to confine their thoughts to the daily affairs of life; they also build telescopes and satellites and accelerators, and sit at their desks for endless hours working out the meaning of the data they gather. The effort to understand the universe is one of the very few things that lifts human life a little above the level of farce, and gives it some of the grace of tragedy.

Steven Weinberg
(1977 p144)

In this chapter (1) we return to the general epistemological principle employed in chapter 1, and then turn to (2) some general consequences of this study for the philosophy of science.

1. An Epistemological Interlude.

A General Account of the Principle N. In chapter 1 I appealed to a general epistemological principle, viz.

N: It is unreasonable to accept a claim without some good reason to accept it.

Now I take N to be a principle that is acceptable on any possible defensible epistemology. (cf. Russell 1928 Hanson 1971) The obvious place to look for a counterexample to this view would be in a foundationalist epistemology. Some foundationalist epistemology may be defensible, and if so, since foundationalism posits basic beliefs not justified on the basis of something else, then a defensible foundationalist epistemology would seem to constitute a counterexample to this view of N. But this sort of objection does not seem to me to hold. For the usual maneuver of the foundationalist is to argue that basic belief(s) are evident (or certain). But then, such a view is compatible with N, because the foundationalist is claiming that the good reason to accept the basic belief(s) is that they are evident. Besides, if we do not accept N what are we to make of all those foundationalists' arguments regarding which beliefs are the basic beliefs and how the basic beliefs are evident? So, I conclude that this sort of objection to N, based upon an interpretation of foundationalism, does not succeed.

Approaching the matter in another way, what is it that is mistaken and irrational about prejudice? To hold a prejudice is to prejudge. In so far as a prejudgment involves accepting some claim, what it is that is irrational and mistaken about a prejudgment is the acceptance of a claim without some good reason to accept it. So, we arrive at a justification for N, the principle of nonprejudice, by a straightforward analysis of prejudice. In fact, it seems to me that any denial of N is simply unphilosophical. If anything is a distinguishing (and distinguished!) characteristic of philosophers and philosophy it is adherence to the principle N.

In a genuinely philosophical vein it might be appropriate to ask whether N falters on self-reference. For how does it apply to itself? The proper response, it seems to me, is to rehearse the points made above. Perhaps N is "constitutive of the practice of philosophy," but if so (and I do think so) this point by itself does not prevent us from raising the points just raised in its favor (just as we raised in chapter 2 points in favor of the ordinary concept of congruence which is constitutive of the practice of measuring). And just as it was held in chapter 2 that it may be possible to imagine circumstances (very general natural facts) in which the ordinary concept of congruence would be abandoned, so also we might imagine such circumstances for the abandonment of N. (A dark day for philosophy and for humanity, indeed, for this would amount to the abandonment of reason itself or at least a considerable part of it.) These then, are as much as I am able to provide as a general defense of N, with perhaps the addition of asking the reader to consider whether or not it is in fact the case that N simply does underlie rationality.

In the remainder of this section I wish to explore a role that N can play in defense of what I take to be the best supported epistemological theory now available, and then to explore another role that N can play within a theory of social rationality. The epistemological theory I have in mind is Lehrer's (1990) theory. Now a presentation and discussion of Lehrer's theory is well beyond the scope of this study, and besides that is already available in Lehrer's (1990). So I will simply present enough theory to address the _isolation objection_ to a coherence theory of knowledge, and then show how N fits into the defense of Lehrer's coherence theory against this objection. We will then turn to social rationality and a role that N can play there.

The Isolation Objection. Typical isolation objection examples are the usual "demon" or "brain-in-vat" skeptical hypotheses. Another more mundane example would be a memory belief about something in fact dreamed, but which coheres very well with all of one's other beliefs--one's acceptance system--and which in fact did not happen, but which one recalls as having happened. Hence, the objection is that a coherence theory will accept such a belief as knowledge, but such a belief isn't knowledge: It isn't true and it isn't justified.

Any fallibilist epistemology can begin a response to an isolation objection by arguing that if the objection requires that every belief "approved" by a theory of knowledge must turn out to be true, then this is requiring certainty. But such a requirement is too strong. What is required of an adequate theory of knowledge is that it be truth-conducive. A coherentist can then claim that in the memory-belief sort of case, checking coherence with other beliefs (including beliefs about the circumstances under which the putative memory belief arose) is the best that can be done in the interest of seeking truths and avoiding errors. Hence, the coherence theory can be properly connected with truth.

With this beginning coherentist responses to the isolation objection have been developed in two basic ways: "Inference to the best explanation" argument (Sellars 1963, Bonjour 1985), or the "ultra-justification" reply (Lehrer 1990). An inference to the best explanation response to skepticism involves pointing out that skeptical hypotheses are extremely implausible, and that the best explanation for the fact, say, that I believe I see pen and paper before me is that pen and paper are in fact before me, and that I am justified in this belief on the basis of its coherence with the rest of my beliefs (including my belief that perception properly connects me to the world), and in particular, my coherence system's ability to distinguish dreams, perceptions, memories, and so on. Of course, a skeptic is likely to reply that all of this is just as it would be in the demon sort of case; all of this including the inference-to-best-explanation-argument. Here the coherentist can respond that the coherence theory requires that the approved belief actually be the best explanation. But the skeptic is likely to reply that, so far, the theory has not provided a basis for making a distinction sufficient to show that the approved belief actually is the best explanation, given such circumstances as described in the demon kinds of cases.

Lehrer's "ultra justification game" reply to the isolation objection is to allow the skeptic even greater power than usual. The idea is that the skeptic is allowed omniscience regarding all of the claimant's beliefs. (The "claimant" is the putative knower.) So, for example the claimant says, "I see pen and paper before me. (It is more reasonable for me to accept that I am properly perceptually connected to the world than not....)" If the claimant is so connected, then skepticism is defeated. But, if the claimant is not so connected, then the skeptic responds, "Replace 'I am so connected' with 'I am not so connected....'" So, in the ultra justification game, if the claimant is so connected, then the claim, "I see pen and paper before me" is knowledge. If the claimant is not so connected, then the claim is not knowledge, and the skeptic wins the round of the game. In either case Lehrer's coherence theory properly connects knowledge with truth.

Lehrer points out that the ultra-justification-game account is a heuristic device that illustrates the response of his coherence theory to the isolation objection. The basic point is that in Lehrer's theory whether one has knowledge in such a case depends upon whether one is in fact properly perceptually connected; it does not depend upon whether or not it is possible to invent skeptical alternatives. So, Lehrer's theory provides the needed connection with truth, and meets the isolation objection.

A somewhat fuller expression of Lehrer's view is as follows: Lehrer notes that if one accepts, say, a perceptual belief, then one is committed to accepting that one is trustworthy in accepting it; and, accepting that is accepting that one is trustworthy in accepting what one does accept in the interest of seeking truth and avoiding error. As Lehrer argues,

> The claim that I am trustworthy in any particular matter under any special set of circumstances may be justified on the basis of the other things that I accept; I accept that I have had success in reaching the truth about similar matters in similar circumstances in the past and that the present circumstances do not differ in any relevant way from past circumstances when I was correct. There is, however, more to the issue. I may accept that my faculties, perception, memory, reasoning, and so forth are trustworthy guides to truth in circumstances of the sort that I find myself in when I accept some claim of those faculties. I must accept, however, that I am trustworthy as well: that when I accept something, that is a good enough reason for thinking it to be

true, so that it is at least more reasonable for me to accept it than to accept its denial.

Thus, there is one special principle of an acceptance system, to wit, that one is trustworthy in matters of obtaining truth and avoiding error. This amounts to the following principle formulated in the first person:

> T. Whatever I accept with the objective of accepting something just in case it is true, I accept in a trustworthy manner.

If someone else accepts that I am trustworthy in this way, then my accepting something will be a reason for her to accept it. Similarly, if I accept that I am trustworthy in this way, then my accepting something will be a reason for me to accept it. Another person might be confronted with some other considerations that cast enough doubt on whether what I accept is true, even granting my trustworthiness, so that my accepting something, though providing a reason for her accepting it, does not justify her in accepting what I do. My accepting something when I am confronted with similar considerations casting doubt on whether what I accept is true, would not justify me in accepting it when I do either....

The consequence of adding principle (T) to my acceptance system is that whatever I accept is more reasonable for me to accept than its denial. It has the effect of permitting me to detach the content of what I accept from my acceptance of the content. My acceptance system tells me that I accept that *p*, accept that *q*, and so forth. Suppose I wish to justify accepting that *p* on the basis of my acceptance system telling me that I accept that *p*. How am I to detach the conclusion that *p* from my acceptance system? The information that I accept that *p*, which is included in my acceptance system, does not justify detaching *p* from my acceptance of it in order to obtain truth and avoid error. I need the additional information that my accepting that *p* is a trustworthy guide to these ends. Principle (T) supplies that information and, therefore, functions as a principle of detachment. It is the rule that enables me to detach the conclusion that *p* from my acceptance of *p*.

The manner in which we trust what we accept indicates that we do accept that we are trustworthy. The mark of our regarding a person as trustworthy is that we trust them, and this applies to ourselves as well. The acceptance of (T) is, perhaps, the result of our nature and universal among people, but this is by no means certain. Some more restricted principle may supplant it in a reflective person, forcing her to arrive at the conclusion that she is trustworthy in some domains but not others. She might accept that she is not trustworthy in some domains, for, despite her best efforts not to accept things without adequate evidence, she often commits a kind of doxastic *akrasia* and accepts some things without adequate reason. For example she might be attracted to particularly elegant mathematical principles and, as a result, accept some principles as theorems because of their elegance, without adequate consideration of the proofs offered for them.

If, however, a person does accept (T) in full generality, then her acceptance of (T) itself will have the result that it is more reasonable to accept (T) than its denial. For, of course, the principle applies to itself. It yields the results that if she accepts (T) with the objective of accepting it just in case it is true, then she does so in a trustworthy way. Thus, principle (T) not only provides for the detachment of other things we accept from our acceptance of them, it provides for the detachment of itself as well....

....To borrow an analogy from Reid, just as light, in revealing the illuminated object, at the same time reveals itself, so the principle, in rendering the acceptance of other things more reasonable than not, at the same time renders the acceptance of itself more reasonable than not. [Reid 1895 p617]

This does not entail, as the foundationalist might wish, that we are personally justified in accepting the principle, only that it is more reasonable to accept it than to accept its denial. Some competitor of it might not be beaten. One such competitor is the fallibilistic claim that we are sometimes in error in what we accept. To meet such a competitor, we need more information about the sort of circumstances in which we err and those in which we do not. Thus, even in the case of principle (T), we require some background information in order to be personally justified in

> accepting the principle. If, however, the foundationalists are incorrect in arguing that there are basic beliefs that justify themselves, they are right in thinking that there are some beliefs that may contribute along with other beliefs to their own justification. (Lehrer 1990 p122-124) [my edit]

Now Lehrer notes that various skeptical arguments (demon cases, brains-in-vats) raise clever possibilities, but whether I am justified in accepting, say, that I see pen and paper before me, does not depend upon the possibility or lack of possibility of describing consistent, coherent alternatives, but rather depends upon whether or not T happens to be true for the case at hand. So, Lehrer argues that his coherentist theory does not succumb to the isolation argument, because raising consistent, coherent alternatives does not defeat T.

Whether Lehrer's reply to the isolation objection is successful can be challenged by the skeptic. Put heuristically, the skeptic might simply refuse to play the game in the way described. She may say that since her position is to doubt that people have knowledge, she doubts that she or anyone can play an omniscient role--even hypothetically or heuristically. Her move in the game, she might say, is not to replace assertion with denial, but rather to replace it with doubt and with coherent alternatives. Her challenge, then, to Lehrer's claimant is for the claimant to show that his acceptances are more reasonable than it is to doubt those acceptances.

It is here that the principle N can come into play, for the claimant may now reply to the skeptic that it is unreasonable to accept a claim without some good reason to accept it (our principle N). The claimant has good coherentist reasons for his acceptances as outlined above. The claimant, furthermore, can accommodate the skeptic's grounds for doubt in accepting that the skeptic's grounds do show that it is possible that the claimant's acceptances are mistaken (again as argued above). And, this claim about the possibility of error is true for every knowledge claim (once more, as argued above). But the claimant may now apply N to the skeptic's challenge to show that his acceptances are more reasonable than it is to doubt those acceptances. Applying N, the claimant simply points out that the skeptical arguments (demon cases, brains-in-vats) do not support the claim that his acceptances are in doubt, because the skeptical arguments do not provide a good reason to accept that T is not true for a (suitable) case at hand. So, the claimant has no good reason (on the skeptical arguments) to doubt T and the claimant has some good coherentist reasons (as described above) to accept T. So, applying N, the claimant's acceptances are more reasonable than doubting those acceptances (remembering again that among those acceptances is the acceptance that the claimant possibly may be mistaken in any knowledge claim). This is how N can function in a defense of Lehrer's theory against the isolation objection. N was there all along, I believe; the present discussion only serves to make N explicit.

Social Rationality. There are many circumstances in which there is considerable interest in arriving at rational outcomes socially. Examples include juries, boards of directors, hiring, selection, or promotion committees, government councils, staffs, and agencies, and scholarly and scientific groups. There are also what appear to be straightforward advantages to a social approach to answering questions and solving problems. These include combining information, resources, and cognitive skills as well as such advantages as division of labor and specialization. There are also what might be called both catalytic and critical advantages to social cooperation: People are often "sparked" in their thinking by interaction with others, and people often find it difficult to think of criticisms for their own ideas, but easier to think of criticisms for the ideas of others.

Despite considerable interest in rational consensus, it is often difficult or impossible to achieve. This is where Lehrer and Wagner's Rational Consensus in Science and Society (1981) enters the picture. [In what follows I refer to Lehrer and Wagner as "L & W" and I refer to Rational

Consensus in Science and Society as "RCSS".] In those cases in which a group fails to reach consensus, where all known information is shared, and where no new information is likely to result from continued communication, RCSS offers a consensus-producing strategy. L & W note that generally it is rational to reach a decision based upon more information rather than less, and in such cases more information is available in the form of members' opinions regarding other members' competence as judges of the issue at hand. If one's opinion is that another member is no more reliable on the issue than chance, then one weights that member's judgement on the issue at zero. Otherwise, one gives that member's judgement on the issue some positive weight. L & W then show that, once certain conditions are met, a consensus is mathematically assured through a succession of weighted averages (arithmetic means) or through a mathematical equivalent of such a succession. L & W then argue that if irrational assignment of weights (based on, say, prejudice or ignorance) is perceived by each member to have been eliminated from the process and if members assign positive weights in certain ways, then members are rationally committed to the consensus outcome. Ultimately, this conclusion rests on the premise that one who enters into a rational process is rationally committed to the outcome.

The "elementary" and "extended" models of L & W's RCSS are as follows. In <u>the elementary model</u> the following assumptions are made: (i) Every member of the group assigns some positive weight to the judgements on the issue of each other member of the group. This is called "positive respect." (ii) The weights assigned remain constant at each stage in the succession of weighted averages. This is called "constancy." (iii) At each stage in the succession each member uses the previous arithmetic-mean-outcome to "improve" her own probability assessment on the issue (moves closer to the mean). Under these conditions the consensus outcome is mathematically determined as a convergent sequence by the weights assigned in the first round, and hence the outcome simply can be computed. There is no need to actually "go thru" the process past the first round. Furthermore, condition (i) can be replaced by a weaker condition called, "chain of positive respect," and still yield the same result. "Chain of positive respect" can be quite weak, for in an extreme case each member could assign zero weight to each other member save one, so long as there is some sequence of members such that each assigns positive weight to the next and the sequence covers all members.

<u>The "extended" model</u> adopts further refinements. Still with a mathematically determined outcome, the extended model (<u>a</u>) drops the "constancy" requirement, (<u>b</u>) allows for a succession of exchanges of information <u>re</u> judges of judges,

judges of judges of judges, and so on, and (_c_) shows that convergence toward a limit (a "fixed point vector") for the aggregation, _without_ infinitely iterated aggregation, occurs. The rationale for (_a_) is that one's judgement about another's competence on the issue well may differ from one's judgement about another's competence in evaluating judges or judges of judges, and so on. The rationale for (_b_) is that it allows exchange of information relevant to the distinction noted regarding (_a_). The mathematical proof of (_c_) is given in _RCSS_ and need not be discussed here, but interestingly, "chain of positive respect" is mathematically adequate for this outcome (_c_), once the aggregation process gets to the last stage at which no new information is available, so long as after that stage members give less variance or no variance (rather than more variance) to individual weights compared to the highest and lowest weight they previously assigned to each individual. Once this level is reached the weights assigned at this level simply can be used to calculate the outcome. A rationale for members to assign less or no variance to weights after that stage is that members are getting no new information, and hence, have no basis for greater variance in weights assigned. L & W point out that less or no variance in weights after all information is in seems a plausible assumption without actually mentioning a specific rationale.

Finally, L & W show that on either model whatever level of agreement is present on the issue (at the first stage) is not reduced by aggregation of the social information; yet, where consensus is not reached on the issue, the consensus produced by the aggregation of social information is rational consensus in the sense that it is based upon more information, the "packet" of information that each member represents.

All of this (L & W's 1981, Lehrer's 1990, and N) fits within a broad epistemological view which may be described as follows: (A) Standards of evidence are standards of what it is rational to accept. Such standards generally are socially communicated, though often only implicitly. The applications of say, some methods of mathematics or of an empirical science do result in wide ranges of agreement, but these depend upon the acceptance of those methods themselves. L & W argue that acceptance of methods is social acceptance and so this social acceptance of methods is the connection to standards of evidence. *I.e.* social acceptances of methods are standards of evidence for rational acceptance. (B) An individual interested in what is true is not committed to the social conception of what is true. Arguably, acceptance of methods is social acceptance, so what it is rational to accept is in this sense a social conception. But accepted methods may simply be wrong, *i.e.*

they simply may not be reliable guides to what is in fact the case. One can be rational and yet be quite mistaken. Both Ptolemy and Lorentz, for example, were rational and in fact quite brilliant; but, each was wrong in certain key ways. Lorentz had the good fortune to become better informed.

Now the reader likely noted the employment of the principle N in the extended model. Once aggregation reaches the stage at which no new information is available, members have no grounds for changing the weights assigned at that stage. To do so would be irrational because it would violate N. It would be to accept a claim that a change in weight is warranted without some good reason to accept that claim. The mathematical model allows the computation of a rational consensus even if members irrationally alter their assignments of weights after this stage, so long as such irrational assignment of weights does not vary beyond previous high and low weights assigned to each individual, but this is a mathematical virtue of the model and goes no distance to excuse such irrationality. Plainly then, we have found a role for N in social rationality just as we found a role for N in the defense of coherentist epistemology against the isolation objection. I take these roles to be among the merits of N.

Finally, all of this epistemology can be summarized in a sentence. It amounts to the argued compatibility and mutual support of a coherence theory of knowledge, a consensus theory of social rationality, and a correspondence theory of truth.

2. Some General Consequences for the Philosophy of Science. If the arguments of this work are cogent, then certain general views regarding theory transitions and choices among empirically equivalent alternative theories, which have been popular and influential in the twentieth century, cannot be correct. For Poincaré, Reichenbach, Grunbaum, Quine, Carnap, and (perhaps still) Salmon, choices among empirically equivalent alternative physical theories can be made only on pragmatic grounds of elegance, descriptive simplicity, ease of application, etc. For Ayer (1946) such choices are meaningless; for Popper (1935 1969) unless some falsifiable difference can be found, they are either meaningless or merely pragmatic. For the historian, Thomas Kuhn (1962 1970) theory transitions are something akin to religious conversion; and, for Quine (1953 1975) they are, only pragmatically rational. For van Fraassen (1980) alternative physical theories can be evaluated cognitively only on their empirical adequacy (conformance to phenomena) and on their empirical strength (range of predictions); but empirically equivalent theories are automatically entirely equal on these criteria, and hence for van Fraassen, any

choice among them can be based at best only upon pragmatic grounds or perhaps upon grounds of taste or interest. For these and still other philosophers of science such choices and transitions are only very weakly justified, if justified at all, but not justified on any cognitive grounds, grounds properly and reasonably connected with legitimate knowledge of physical things. But chapters 1 and 2 argue that there are cognitive grounds for choice and cognitive grounds for transitions, and that these grounds are, in the senses indicated (including the postscript to chapter 2), ontological grounds. Hence, if these arguments hold, then the weakly-justified and/or the not-justified-at-all views must be mistaken. We have found, I believe, cognitive grounds for choice among empirically equivalent theories and cognitive grounds for theory transitions. They were with us all along.

The position advocated here is compatible with Lehrer's (1990) epistemology and with the general positions in the philosophy of science shown in Byerly's 1979 and 1990 and in Salmon's 1984 and 1989. It is compatible as well, I believe, with the mature philosophy of Wittgenstein (1958). For this broad general conjunction of coherent views, I would like to suggest a name: physical empiricism. I do hope that Hume would appreciate this name, for I do see this broad conjunction as a proper development of Hume's (1739

1748) philosophy, and as a proper response to questions about knowledge and science that Hume's philosophy properly leaves us with. Lehrer (1975 1989) likely would point out that much of this broad conjunction is already present in Reid (1764 1788), and I surely would not dispute this point with Lehrer. Of course, a discussion of this broad general view (beyond the mostly sketchy points that have already been made in passing) is far beyond the scope of this study, but mentioning it may give the reader an impression of the general approach that has served as a guide, the guide of physical empiricism.

The general view in the philosophy of science argued in this work can be summarized fairly briefly: Physical theories make claims about physical things; they make claims about physical properties, individuals, and relations. There likely always will be available empirically equivalent alternative theories (similarly, in epistemology there likely always will be available skeptical alternative theories). For the grounds for choices among physical theories and for the grounds for transitions from one to another physical theory addressed in this work, we find that grounds are, where rational, ontological. We <u>can</u> decide which are right, and which are illusions.

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