HUME AND THE EXTERNAL WORLD

by

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Abstract

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In this dissertation I try to combine epistemology and philosophy of science with Hume exegesis. I defend a qualified version of the Representative Theory of Perception, arguing that even if we perceive external objects only in virtue of our being aware of the sense-data (or Humean impressions) they occasion, we are nevertheless justified in believing that there is an external world and that it has at least the structure that we ordinarily believe it to have. I also try to show that, once some prevalent misinterpretations of Hume's discussion of "what causes induce us to believe in the existence of body" are corrected, it will be seen that Hume described for us both the view about the external world which I am defending and the best evidence we have for that view. Of course, Hume himself denies that the view in question is confirmed by the evidence. But I claim that this denial is due primarily to his insistence that belief of a matter of fact not given in sensation or memory can be justified (if at all) only by causal reasoning as he has analyzed it. If hypothetico-deductive reasoning is admitted to be justificatory, then

the facts about our impressions that Hume points out in his discussion of our belief in "body" justify our beliefs about the existence and structure of the external world.

After indicating in Chapter 1 the main purposes of the dissertation, I proceed in the second chapter to distinguish three different views about the nature of the external world, and to show that Hume argues that one is unintelligible, or absurd, another is empirically false, and the third (which is what Hume calls "the philosophical view of body" and what I call "PV") is unsupported, i.e., we simply have no reason to believe it. Then in Chapter 3, I examine some difficult parts of Hume's discussion "Of Scepticism With Regard to the Senses" (e.g., his distinction between coherence and causal inferences), and I argue that the results of this examination provide us with a good reason to believe PV.

But PV involves the claim that objects persist without changing through periods of time. This claim seems to conflict with Hume's statement that, on his empiricist principles, we have no idea of such changeless persistence through time. Chapter 4 is a discussion of Hume's views about space and time, showing that there is no real conflict.

In Chapter 5 I follow out the consequences of a suggestion of Hume's to the effect that even if we could infer an external world, we could never have reason to accept the whole content of PV. The problem is that when we try to extract from PV all and only what we are justified in believing,

the result is unintelligible, at least on Hume's principles. This problem is solved in Chapter 6 by (1) construing PV as a "partially interpreted" theory, (2) using the Ramsey method to eliminate problematic terms in the theory, and (3) using our ordinary beliefs about the external world as a model, whereby we can picture the sort of thing asserted by the theory.

Finally, in an effort to do justice to Hume's claim that the only type of non-deductive inference is causal, I examine arguments by some twentieth century philosophers of science to the effect that the hypothetico-deductive method must be supplemented by analogical reasoning similar to Hume's causal reasoning. I conclude that such analogical reasoning is not necessary for justifying acceptance of a theory, but only for a certain kind of theoretical reduction.

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CHAPTER 1

INTRODUCTION

Of all the many discussions of perception and our knowledge of the external world in the philosophical literature, perhaps the most famous is Hume's "Of Scepticism with Regard to the Senses," Section 2 of Part IV of Book I of A Treatise of Human Nature. This discussion, in which Hume combines philosophy and associationist psychology in his own characteristic and enigmatic way, has profoundly influenced subsequent philosophers' efforts to deal with the same topics. Its skeptical tendencies have given rise in many philosophers to the attitude that Hume's view represents a challenge which must be answered, if not on its own grounds, then by shifting to some other grounds. Thus, for example, many philosophers have answered Hume by rejecting his fundamental principle that what is before the mind when (if ever) we perceive an external object is not an external object, but an impression.[1] Hume's psychological explanation of our beliefs about the external world has had somewhat less influence, but some philosophers have found inspiration in it, and have used parts of it

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[1]See, e.g., Thomas Reid, An Inquiry Into the Human Mind, ed. Timothy J. Duggan (Chicago: University of Chicago Press, 1970), and Maurice Mandelbaum, Philosophy, Science, and Sense Perception (Baltimore: Johns Hopkins Press, 1964), Chapter 3.

for their own purposes.[2]

Though Hume's discussion of perception and the external world has received a good deal of attention -- at least since 1940, when H.H. Price complained that it had not received the attention it deserved[3] -- I think there is much that is useful (and true) in it that has not been generally appreciated, due to certain prevalent mistakes and confusions about what Hume was saying. I intend to correct some of these mistakes, first of all in the interest of historical accuracy, but also with an eye toward getting some insight into the problems of perception and the external world. In particular, I hope to show, through an exegesis of Hume's discussion, how a certain sort of representative realism can be defended. Hume himself did not, of course, argue for such a representative, or causal, theory of perception, nor did he believe that any kind of realism with respect to the external world is rationally defensible. But I will argue that he presented us with the major ingredients of a defensible representative realism, and that he was wrong not to see them as such.

The importance of this project can be indicated as follows:

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[2]See, e.g., A.J. Ayer, The Foundations of Empirical Knowledge (London: MacMillan, 1940), Section 23; and compare Arthur O. Lovejoy, The Revolt Against Dualism, 2nd ed. (LaSalle: Open Court, 1960), P. 334, with Hume's Treatise, P. 195 of the Selby-Bigge edition (Oxford: Clarendon Press, 196

[3]H.H. Price, Hume's Theory of the External World (Oxford: Clarendon Press, 1940), pp. 1-2.

Many philosophers in both the distant and recent past have adopted the position that whatever awareness we have, in perception, of public, physical objects is mediated by awareness of something else, which is private to the perceiver. These private objects of immediate (or less mediate) awareness have been called "ideas" (by Descartes, Locke, and Berkeley), "impressions" (by Hume), and "sense-data" (by G.E. Moore and, following him, other twentieth-century philosophers), as well as "images," "representations," and "sensa." Let us call this position subjectivism.[4] The tradition of objecting to and rejecting subjectivism is about as old as subjectivism itself. But in recent years the rejectionists have become much more numerous and prominent than the proponents of subjectivism. The reasons for this include (1) the influence of J.L. Austin, whose painstaking critical analyses[5] of some of the main arguments for and tenets of subjectivism convinced many other philosophers of its falsity, and (2) the fact that materialism, which has become the dominant philosophy of mind (at least in the English-speaking philosophical world), cannot easily accommodate the realm of private objects of awareness the existence of which seems to be entailed by subjectivism. But one consideration that has provided the reason for many philosophers, over the centuries, to reject subjectivism, and that has prompted

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[4]See Mandelbaum, op. cit., P. 121.

[5]Sense and Sensibilia, reconstructed from Austin's manuscript notes by G.J. Warnock (Oxford: Oxford University Press, 1962).

other philosophers to look for and welcome other reasons to reject subjectivism, is that, it is thought, subjectivism leads to skepticism with regard to the external world -- i.e., the world of physical objects and (or including) other people. That is, it has been widely believed that subjectivism entails (with the help of other premises, whose plausibility is greater than that of subjectivism) that each person (or I) can have no good reason to believe that either physical objects or other people exist. Thus, e.g., Thomas Reid rejected subjectivism because he believed that Berkeley and Hume had shown that it entails skepticism[6]; more recently, D.M. Armstrong[7] and George Pitcher[8] have each cited its entailing skepticism as an essential part of their arguments for rejecting subjectivism; and I assume that its alleged entailment of skepticism is one reason for many philosophers' willingness to reject subjectivism on the basis merely of Austin's and others' critiques (often not too cogent at that[9]) of the arguments for subjectivism.

The two major attempts by subjectivists to avoid skepticism with regard to the external world are (a) phenomenalism and (b) the representative, or causal, theory of

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[6] Op. cit., especially pp. 15-18.

[7] Perception and the Physical World (New York: Humanities Press, 1961), P. 27 and pp. 29-30.

[8] A Theory of Perception (Princeton: Princeton University Press, 1971), pp. 62-63.

[9] See A.J. Ayer, "Has Austin Refuted the Sense-Datum Theory?" Synthese 17 (1967), reprinted in Ayer, Metaphysics and Common Sense (San Francisco: Freeman, Cooper & Co., 1970).

perception. Phenomenalism is the view that all we mean by 'external object' is a group of actual and possible sense-data and that whatever we ordinarily believe about external objects can be expressed by sentences about actual and possible sense-data. E.g., to say that I see a particular physical object is, according to phenomenalism, to say merely that such-and-such sense-data exist and such-and-such further sense-data would exist if such-and-such conditions were met (where the conditions are also just the existence of certain types of sense-data). Since the "external" world is just actual and possible sense-data, and since subjectivism allows that I know about actual and possible sense-data, subjectivism does not involve skepticism with regard to the external world. But (waiving other problems -- e.g., the well-known difficulties involved in specifying, purely in terms of actual and possible sense-data, the conditions relevant to the existence of certain possible sense-data) to me phenomenalism, in this sense, is just plain false: I certainly do not mean by 'external object' merely a group of actual and possible sense-data. My belief that, e.g., I see a desk is not simply the belief that I am having certain sense-data now and I would have certain other sense-data if ... ; and so the view that all my knowledge about the external world is about actual and possible sense-data is a form of skepticism vis-a-vis my beliefs about the external world. And I assume that at least the majority of my readers are like me in this

respect.[10]

The representative, or causal, theory of perception, insofar as it is a subjectivist attempt to avoid skepticism, is the claim that we can justifiably infer, from certain facts about our sense-data, most of what we ordinarily think we know about the external world. That is, the representative theory flatly denies that subjectivism leads to skepticism about the external world, claiming that, though our awareness of the external world is always mediated by our awareness of sense-data, we are nevertheless justified in believing most of what we ordinarily believe about the external world, based on evidence provided by our sense-data. Proponents of the representative theory have differed as to the nature of the evidence and of the inference; and several of the accounts of how the existence and nature of the external world could be inferred from facts about our sense-data have been specifically criticized. But some arguments -- notably some of Berkeley's and Hume's arguments[11] -- purport to show on general grounds that it is impossible to infer anything about the external world from any facts about our sense-data. These arguments

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[10]See the footnote to the penultimate paragraph of Section XII, Part II of Hume's Enquiry Concerning Human Understanding, in which he says that Berkeley's view that the physical world is nothing but "ideas" is really skeptical, "though otherwise intended." See also D.G.C. MacNabb, David Hume: His Theory of Knowledge and Morality, 2nd ed. (Oxford: Blackwell, 1966), pp. 122-123.

[11]Berkeley's arguments that belief in matter is either unjustified or absurd are scattered throughout his Treatise Concerning the Principles of Human Knowledge and Three

have enjoyed wide acceptance[12], with the result that the representative theory is widely discredited among philosophers.

But recently a few philosophers -- notably Frank Jackson[13] and Grover Maxwell[14] -- have defended representative realism, arguing that we can avoid the Berkeleian and Humean (as well as other) criticisms by suitably construing the kind of inference and the conception of the external world involved. Hume had argued that the only way to infer anything's existence is through causal reasoning; in causal reasoning we infer the existence of a thing of one kind A from the existence of a thing of another kind B, after having observed things of kind A "constantly conjoined"

Dialogues Between Hylas and Philonous, see especially sections 1-33 of the former. Hume's main argument that belief in the external world is unjustified is on p. 212 of the Selby-Bigge edition of the Treatise; he argues that certain forms of representative realism are unintelligible, or absurd, on pp. 67-68 and pp. 225-231.

[12]Hume's argument on P. 212 of the Treatise is given (without reference to Hume) in Ayer, Foundations of Empirical Knowledge, pp.220-221; in Armstrong, op. cit., P. 29; and in James W. Cornman, Perception, Common Sense, and Science (New Haven: Yale U.P., 1975), pp. 254-258. Armstrong also uses some of Berkeley's arguments on pp.30-31. Gilbert Ryle, in The Concept of Mind (N.Y.: Barnes & Noble, 1949),P. 235, calls representative realism "unavailing" but does not say why. Similarly Pitcher, op. cit., P. 63, says that the "enormous epistemological difficulties" for representative realism are "well-known" and "insuperable," and leaves it at that.

[13]Perception: A Representative Theory (Cambridge: Cambridge University Press, 1977).

[14]"Scientific Methodology and the Causal Theory of Perception," in Problems in the Philosophy of Science, ed. I. Lakatos and A. Musgrave (Amsterdam: North Holland, 1968); and "Theories, Perception, and Structural Realism," in The Nature and Function of Scientific Theories, ed. R. G. Colodny (Pittsburgh: University of Pittsburgh Press, 1970).

with things of kind B; but we have never observed (in the relevant sense of 'observe') external objects conjoined with impressions -- we observe (in this sense) only impressions; so we cannot infer the existence of external objects from our impressions. Jackson and Maxwell, among others, argue that we can infer the existence of objects not only through causal reasoning as analyzed by Hume, but also through hypothetico-deductive reasoning, They claim that our beliefs about the external world, construed as a hypothesis explaining our sense-data, are hypothetico-deductively justified. But Hume and Berkeley argued that we cannot so much as conceive of objects that are not impressions or ideas, so the external world hypothesis, if it posits such objects, is unintelligible. Maxwell has countered this argument by treating the external world hypothesis as a theory and expressing it by means of a "Ramsey sentence"[15], in which the only substantive terms that occur refer to sense-data and their properties and relations.

I find myself basically in agreement with the above approach. But Jackson and Maxwell, as well as earlier proponents of this approach, have only indicated in general terms the type of inference and the kind of conception of the external world they have in mind. I would like to proceed further along these lines and examine in greater detail what we could infer about the external world and how we could infer it. In so doing I hope to

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[15]See Frank P. Ramsey, The Foundations of Mathematics and Other Logical Essays, ed. R.B. Braithwaite, pp. 212-236, especially P. 231.

show that subjectivism does not entail skepticism with regard to the external world.

How does the exegesis of Hume's Treatise fit in with these aims? First of all, one of my exegetical claims about Hume will put into relief an important step in my proposed justification of our beliefs about the external world. I will argue that underlying Hume's discussion of "coherence" and the distinction between coherence and causality is the (perhaps vague) realization that the vast majority of our universal laws of nature depend, for their universality, on the external world hypothesis. The fact that the external world hypothesis allows us to subsume the irregular stream of our sense-data under an orderly system of universal laws is evidence for the truth of that hypothesis. Secondly, through an examination of Hume's version of the arguments from illusion, hallucination, and perspectival relativity, I will show why subjectivism is preferable to naive realism. Most commentators and philosophers write off Hume's version of these arguments as either totally confused or too concise to be of any use. I will argue that, on the contrary, Hume has given us a compelling reason to adopt subjectivism. Thirdly, there is a passage in the Treatise[16] which has drawn little attention, but which is, I believe, extremely important in that it drives the defender of representative realism in the direction of Maxwell's

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[16]Pp. 216-217 of the Selby-Bigge edition.

Ramsey-sentence conception of the external world.

But aside from the interrelatedness of my exegetical and epistemological claims, there are further reasons for treating the problems of our knowledge of the external world from within the context of Hume's philosophy. I believe that Hume's way of setting out subjectivism, with some superficial inconsistencies ironed out, is as good (i.e., as defensible and as close to the truth) as we could want. I also believe that Hume's discussion of the psychological "causes" of our "belief in body" leads naturally to my proposed justification of that belief (or a reasonable facsimile thereof). Moreover, Hume has formulated some of the most powerful objections to the epistemological view I wish to defend, so my dealing with those objections on Hume's own grounds will be worthwhile. Finally, Hume's standards of intelligibility and justification are extremely high -- i.e., if they are wrong (and nearly everyone would agree that they are), it is because they are too strict, often ruling out as unintelligible or unjustified what is really perfectly intelligible and justified. So if the intelligibility and reasonableness of belief in the external world can be defended from within (or just outside) Hume's framework, then it is unlikely that any better (because less strict) framework would pose any further problems.

In the next chapter I will sort out the various views about perception and the external world that Hume discusses,

and I will briefly survey what Hume says as to the justification and intelligibility of each. I will claim that Hume deals primarily with three different views about the external world, which I shall call (following Hume) (1) "the vulgar view," (2) "the philosophical view," and (3) the view of external objects as "specifically different" from perceptions. Hume argues that (1) is false, (2) is unjustified, and (3) is unintelligible.

In Chapter 3 I will examine some of the central concepts and arguments in Hume's discussion of the "causes" which "induce us to believe in the existence of body." This examination, besides correcting some prevalent misunderstandings both of the particular concepts and arguments examined and of the relationship among Hume's psychological accounts of our various natural beliefs, will bring to light the main evidence that confirms our belief in the external world. It will also indicate the respects in which the philosophical view of body is superior to the vulgar view.

Chapter 4 will deal with Hume's claim, argued for in I:II:3-5, that we cannot conceive of anything's existing unchanged throughout a period of time (longer than a single moment). Both the philosophical and the vulgar views of body entail that some things do exist unchanged for (non-momentary) periods of time. These views, therefore, would seem to be inconceivable according

to Hume. Removing this difficulty will require an examination of Hume's views about space and time.

In Chapter 5 I will argue that our hypothetico-deductive argument for the philosophical view is insufficient to justify that view in its entirety, and that when we try to extract all and only what is justified, we end up with something that Hume (at least) would say is unintelligible. The philosophical view will thus be faced with the problem that to the extent that it is justified, it is unintelligible, and to the extent that it is intelligible, it is unjustified.

This problem will be solved in Chapter 6, in which the philosophical view is construed as a theory positing unobservable entities the intrinsic nature of which is unknown. The intelligibility of this theory is established by (1) using the original, or naive, version of the philosophical view (not to be confused with naive realism, which is more like the vulgar view) as a model of the theory which allows us to picture what the theory asserts, and (2) setting out the theory as a formal system, in which the rules of formation and transformation are explicitly stated, so that we can avoid the error of mistaking aspects of the model for part of what is asserted by the theory. Finally, in an effort to take seriously Hume's claim that causal reasoning, or inductiion, is the only way to infer matters of fact, I will consider,

and rebut, arguments by some twentieth-century philosophers of science to the effect that the hypothetico-deductive method must be supplemented by analogical reasoning which would reduce it to something very much like Hume's causal reasoning. I will argue that such supplementation is unnecessary, and that therefore our theory of the external world is <u>justified</u> despite its lacking the relevant kind of analogical support.

CHAPTER 2

THREE VIEWS ABOUT THE EXTERNAL WORLD

Most of what Hume has to say about perception and our knowledge (or lack thereof) of the external world is in "Of Scepticism with Regard to the Senses," which is Section 2 of Part IV of Book I of A Treatise of Human Nature (henceforth I:IV:2, and similarly for other sections of the Treatise). However, there is a brief but important complementary discussion in I:II:6, "Of the Idea of Existence and of External Existence." And Hume takes up specifically the Lockean representative theory of perception in I:IV:4, "Of the Modern Philosophy." Hume's main point in these sections seems to be that though we all believe that the physical world, or "body," exists, we are not justified in so believing. I would like here to isolate the main arguments that Hume puts forward for the claim that our belief in the existence of the physical world is unjustified.

But first it may be asked exactly what this belief is that is claimed to be unjustified, though universally held. Startingwith such a question is generally good procedure in examining skeptical arguments; but it is especially helpful in the present case. For, though I think Hume is fairly explicit about it, nevertheless, for various reasons, it is easy to misinterpret some of what he says.

In the first paragraph of I:IV:2 and the beginning of the second paragraph, Hume tells us that he is concerned with the question, "What causes induce us to believe in the existence of body?" (P. 187)[1]. And he begins his "reasonings on this head" by suggesting that, in order to facilitate our inquiry,

> We ought to examine apart those two questions, which are commonly confounded together, viz. Why we attribute a continu'd existence to objects, even when they are not present to the senses; and why we suppose them to have an existence distinct from the mind and perception. Under this last head I comprehend their situation as well as their relations, their external position as well as the independence of their existence and operation. (P. 188).

Thus, apparently, the belief in the existence of body with which Hume is concerned in this section is the belief that there are objects which continue to exist when not perceived, and which are external to the mind and independent of the mind and of being perceived.

Before going into such things as exactly what Hume meant by 'external' and 'independence'[2], it should be noted that there is apparently something else that might be referred to as 'belief in the existence of body' besides the belief that there are objects that have continued and distinct existence. For Hume concludes the second paragraph of I:IV:2 saying,

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[1]Page numbers in parentheses refer to David Hume, A Treatise of Human Nature, edited by L.A. Selby-Bigge (Oxford: Clarendon Press, 1968).

[2]See below, Chapter 3, Part 4, pp. 90-96.

> These [i.e., the questions of why we attribute continued and distinct existence to objects] are the only questions that are intelligible on this subject. For as to the notion of external existence, when taken for something specifically different from our perceptions, we have already (Part II, Section 6) shown its absurdity. (P. 188).

Thus 'the belief in the existence of body', or at least 'the notion of external existence', may be taken to mean the belief that there is something "specifically different" from perceptions -- i.e., the belief that there exists something which is not a perception, i.e., neither an impression not an idea. Hume argued in I:II:6 that "'tis impossible for us so much as to conceive or form an idea of any thing specifically different from ideas and impressions," (P. 67); which explains why he considers questions about anyone's believing in the existence of any such thing as not "intelligible." Furthermore he claimed that "generally speaking, we do not suppose them [i.e., external objects] specifically different; but only attribute to them different relations, connexions, and durations. But of this more fully hereafter (Part IV, Section 2)." (P. 68). So since he thought that we do not ("generally speaking") suppose that external objects are specifically different from perceptions, but rather that they are perceptions (odd as that may sound) which have a continued and distinct existence, it follows that in discussing the origins of <u>our belief</u> in the existence of body, he need concern himself only with the latter supposition, and should ignore the former.

However, in I:IV:2 Hume discusses two different views

concerning the existence of body -- that of "the vulgar" and that of "the philosophers." (See, e.g., P. 193). According to the vulgar, the very perceptions that come into the mind are what continue to exist when not in the mind, and are independent of being in the mind; whereas according to the philosophers, these perceptions have neither a continued nor an independent existence, but there are objects which do have continued and independent existence, and which never themselves come into the mind but are represented by the perceptions that do. Now it may be thought that the philosophers' view is the same as the view that there are objects specifically different from perceptions.[3] And consequently it will be wondered why Hume considers this view at all, in light of what was said above (unless we say that by the "generally speaking" Hume meant to exclude philosophers) and, more importantly, why he does not merely repeat his earlier criticism of that view but instead gives a much weaker criticism (see below).

The answer seems to be that Hume was willing to give the philosophers the benefit of the doubt: They are not proposing something as absurd as the existence of objects specifically different from perceptions (i.e., they are not just fooling themselves in thinking that they are proposing a view at all); rather "they arbitrarily invent a new

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[3]See, e.g., Jonathan Bennett, Locke, Berkeley, Hume: Central Themes (Oxford: Clarendon Press, 1971), P. 349; and Terence Penelhum, Hume (N.Y.: St. Martin's Press, 1975), P.202n.5

set of perceptions" (P. 218, my emphasis) which are supposed to have the continuity and independence lacked by the perceptions in our minds. That is, in order for the philosophers' view to be at all intelligible, and therefore to merit our attention, what is said to exist must be perceptions.

Perhaps the main reason why a reader of the Treatise may be apt to think that the philosophers' view is just the view that body is specifically different from perceptions is that he feels that this is what the philosophers' view should be. For surely once we distinguish the fleeting impressions in our minds from the rocks and trees, desks and chairs etc. that lie behind them, we do not want to say that the latter are just more impressions, but ones that happen to last longer than the ones in the mind and to be independent of minds. As a matter of fact, isn't it also a misrepresentation of the view of the vulgar (a misrepresentation initiated, no doubt, by Berkeley's strange claims about the affinity between his immaterialism and common sense) to say that they attribute continued and distinct existence to perceptions? Isn't it rather the case that the vulgar first become apprised of the existence of perceptions by philosophers, and that they realize that the shoes and trees etc. that they formerly thought were the things that come before the mind are specifically different from perceptions?

Perhaps the feeling that gives rise to the above sentiments can be overcome if we realize that, at least

according to Hume, the terms 'impression', 'perception', and even 'idea' do not necessarily connote something "mental" as opposed to "physical." There are no "mental substances" in which impressions "inhere," or "physical substances" in which impressions cannot "inhere." I do not think we would be too far from Hume's meaning if we substituted 'quality', or 'sensible quality', for 'impression'.[4] Then the vulgar view would be that the qualities which come before our minds continue to exist when not before the mind, and exist independently of minds. The philosophical view would be that there are qualities (e.g., the size and shape, if not the color of my desk) that persist through time, and exist independently of my perceiving any such qualities and of minds in general; and there are other qualities, dependent for their existence on my perceiving them (e.g., on the state of my nervous system, on my spatial position relative to that of the qualities whose representations are in my mind, etc.), and ceasing to exist when I stop perceiving them. Finally, the view that there are objects specifically different from impressions and ideas is the view that there are things which are not identifiable with any quality or group of qualities.

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[4]See below, Chapter 3, Part IV, pp. 82-84.

Whether or not I have correctly diagnosed and treated our qualms about the way I construe Hume's account of the various views of physical objects, I think it is fairly clear, from what Hume says, that this construal is correct.[5] Thus there are three possible beliefs (or rather two possible and one impossible) that might each be called "belief in the existence of body." One is the vulgar belief that the very impressions in our minds continue to exist apart from, and independently of, our minds. Another is the philosophical belief that there are perceptions that persist through time and are distinct from any perceptions in our minds. And the third is the notion or supposition (not a belief, for it cannot even be conceived) that there is something specifically different from impressions and ideas.

Hume has a different objection to each.

His main objection to the vulgar view is that it is proven false by certain "experiments" which show that "our sensible perceptions are not possest of any distinct or independent existence." (P. 211). And since "there is an intimate connexion betweixt those two principles, of

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[5]To repeat the main evidence for distinguishing the philosophical view from the "specifically different" view: on the top half of P. 218, Hume explicitly says that the philosophers posit perceptions, and adds that it must be perceptions if their view is to be intelligible; also, Hume's argument against the philosophical view in I:IV:2 is very different from his argument against the "specifically different" view in I:II:6.

a continued and of a distinct or independent existence, and ... we no sooner establish the one than the other follows, as a necessary consequence," (P. 210; see also P. 188), it follows that our perceptions do not have a continued existence either.

Hume's account of the experiments and how they show that our perceptions do not have independent existence, which is basically a version of the familiar "argument from illusion," is very brief -- i.e., one paragraph on pp. 210-211. (Apparently the argument was already so familiar in Hume's day that he felt no need to elaborate.) Suffice it to say here that there are many problems with the argument from illusion which Hume's formulation is far too brief to deal with; and that his claim that continued existence entails independent existence requires some elucidation and/or argument.[6]

Against the view that there are objects specifically different from perceptions Hume argues as follows:

> Now since nothing is ever present to the mind but perceptions, and since all ideas are derived from something antecedently present to the mind; it follows that 'tis impossible for us so much as to conceive or form an idea of any thing specifically different from ideas and impressions. (P. 67).

Thus the inconeivability of anything which is not a perception follows directly from Hume's brand of empiricism -- i.e., that we have an idea of x (if and) only if we

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[6]But see below, Chapter 3, Part IV.

have had an impression of x (or in case the idea of x is complex, i.e., composed of ideas of a_1, a_2,..., a_n, we have an idea of x only if we have had impressions of a_1, a_2,..., a_n) -- plus the view which is "universally allowed by philosophers, and is besides pretty obvious of itself, that nothing is ever really present with the mind but its perceptions or impressions and ideas." (P. 67). So according to Hume, whoever says that there are objects specifically different from perceptions has no idea -- nor do we -- of what he is saying.

As for the philosophical view, exactly what Hume's objections to it are is not at first sight clear. For on the one hand, he says "that it contains all the difficulties of the vulgar view, with some others that are peculiar to itself," (P. 211), and again that "'tis liable to the same difficulties as the popular system; and is over-and-above loaded with this absurdity, that it at once denies and establishes the vulgar supposition." (P. 218). This seems to indicate that Hume believed that the philosophical view is subject to the same (or similar) experimental refutation as the vulgar view, and in addition involves a logical contradiction. (And this is letting it off easy -- i.e., assuming that the "difficulties ... peculiar to itself" mentioned in the first quotation above are just the "absurdity" mentioned in the second.) On the other hand, all the arguments that Hume actually brings against the view seem to be only to the effect that we have no good

reason to believe it, not that it is false or contradictory, (See pp. 211-218, especially pp. 211-213.) Besides, the philosophical view is proposed, according to Hume, for the sole purpose of avoiding the experimental refutation to which the vulgar view is subject; and Hume nowhere tries to show that it fails to achieve that purpose, or that it is subject to any similar refutation.[7] And as for the supposed logical contradiction, Hume does not point out any. Granted, it attributes continued and distinct existence to some impressions and denies it to others (see P. 218); but that's no contradiction.

Now, aside from the purported experimental refutation, the only difficulty with the vulgar view, according to Hume, is that what leads people to adopt it is not any reasoned argument, but rather a series of confusions; the imaginative "fiction" of the continued, distinct existence of our impressions is believed only because it renders our confused opinions compatible with obvious experiential facts. (See pp. 188-210; especially 194-210). Hume argues (pp. 211-218; especially 211-213) that the only reason for anyone's adopting the philosophical view is that it allows one to maintain the basic tenets of the

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[7]It might be argued that Hume's "experiments" show that not only the perceptions in our minds, but also the extra-mental perceptions posited by philosophers cannot have continued and distinct existence. This objection to the philosophical view will be obviated by our abstract construal of the view, on which the posited extra-mental entities need not be perceptions. See below, Chapters 5&6.

vulgar view, while avoiding the experimental refutation, and that it does so by yet another imaginative fiction -- i.e., the arbitrary positing of a set of perceptions which are supposed to have the continuity and independence which the impressions in our minds were shown to lack. So since adoption of the philosophical view is based on first holding the vulgar view, which is in turn based on confusions, it follows that ultimately the philosophical view itself is based on the same confusions. Thus it seems that the "difficulties" of the philosophical view that it shares with the vulgar view are just their being based on those confusions. And the "difficulties peculiar to itself" are just those additional imaginative postulations that lead from the vulgar view to the philosophical. As for the "absurdity that it at once denies and establishes the vulgar supposition," Hume himself tells us that all he means is that the philosophers realize that the vulgar view is false, but at the same time give in to the same tendencies which originally led to that view in arbitrarily postulating a world of objects with continued and distinct existence. (P. 218).

Hume's argument that we have no <u>reasonable</u> grounds for accepting the philosophical view is essentially as follows (P. 212): The only things about the existence of which we can be certain are the perceptions which are "immediately present to us." The only way we can infer the existence of one thing from that of another is by

causal inference -- i.e., by inferring cause c from effect e, or vice versa. But in order to make such an inference we must have observed things like c constantly conjoined with things like e. But we observe only perceptions, never objects; consequently we never observe objects constantly conjoined with perceptions. Therefore, though we may, from the existence of certain perceptions, infer the existence of certain other perceptions, we cannot infer the existence of the objects posited by the philosophical view. (It might be thought that this argument is based on construing the philosophers' "objects" as not perceptions, but something specifically different. However, the argument loses none of its force if we construe 'objects' to mean perceptions which have continued and distinct existence and which never come into a mind; for since they never come into a mind, we can never observe a conjunction of any of them with any of the fleeting perceptions in our minds, and so cannot causally infer their existence.)

This concludes our survey of the three views about body considered by Hume and his objections to each. To summarize briefly, the three views are: (1) that body is specifically different from perceptions, (2) the vulgar view that body is identical with perceptions in our minds that are supposed to have continued and distinct existence, and (3) the philosophical view that body is just perceptions that have continued and distinct existence, and are not

identical with any perceptions in our minds. Hume claims that (1) is inconceivable, (2) is false, and (3) is groundless, arbitrary, and ad hoc.

Before going into each view and each objection in greater detail, let me indicate, in general outline, what I expect my conclusion to be: As has already been mentioned, Hume claims that our belief in body is the result of confusion; we do not come to that belief by reasoned arguments, but rather are carried away by our imaginations, which manage to keep our confused opinions constantly one step ahead of refutation by obvious facts. Certain aspects of our impressions set off processes in the imagination which culminate in our accepting first the vulgar belief in body, then, for those of us who "reflect a little on the subject," the philosophical belief, and finally (though Hume does not explicitly say that there is this third stage) the supposition that there is a "something, I know not what," specifically different from perceptions. Hume seems to think that this account of the origin of our beliefs in body completely undercuts any claim to truth of any of them. Thus he concludes the penultimate paragraph of I:IV:2, "What then can we look for from this confusion of groundless and extraordinary opinions but error and falsehood? And how can we justify to ourselves any belief we repose in them?" (P. 218). However, if there are good reasons for believing something, then the fact that we originally came to entertain and believe it for different and bad reasons is irrelevant to the question

of its truth. And this seems to be the case with respect to our belief in body. In fact, the very aspects of our impressions which led us, *via* confusions and imaginative postulations, to belief in body can serve to confirm that belief. (The case is somewhat like that of a mathematician who incorrectly thinks he has proved a surprising new theorem, when in fact *his* "proof" is invalid, but *there is a proof* of the theorem from the same axioms.) Each of our three views of body, or of the physical world, may be viewed as a hypothesis which, if true, would *explain* those aspects. And in any case, we can use these hypotheses to *predict* various things about our impressions. Furthermore, it seems that the more we refine each hypothesis (by specifying more precisely what the physical world is like -- I intend to explain how this is possible even on something like the "specifically different" hypothesis) the more features of our impressions we are able to explain and predict by means of it, thus providing a strong argument for its truth. Of course, if there is an experimental refutation of a hypothesis -- as Hume claims there is of the vulgar hypothesis -- then the hypothesis is, at least in part, false, and considerations of explanatory and predictive power are irrelevant (more or less). But Hume's only objection to the philosophical hypothesis is that it is groundless; and we could argue that its explanatory and predictive power provide grounds for accepting it.

However, I will not argue for acceptance of the philosophical view as it stands. I agree with Hume that, to some extent at least, the philosophical view is groundless and arbitrary. My contention is that, once we get rid of the groundless and arbitrary parts, we are left with something very close to the view of physical objects as specifically different from perceptions; and I will try to show how we can, even on Hume's theory of ideas, conceive of such things. More precisely, I will argue that the philosophical view, though belief of it is unjustified, allows us to conceive of physical objects, which may or may not be (we cannot know) specifically different from perceptions; just as, e.g., the Bohr model of the atom allowed us to conceive of atoms and subatomic particles, which were nevertheless believed to be specifically different from anything in our perceptual experience.

My contention is, then, that even if we accept Hume's basic principles -- i.e., that only impressions and ideas are ever immediately present to the mind, that every simple idea is preceded by an exactly similar impression, and that ideas are separable in the imagination if and only if they are distinct -- we are still justified in believing in the existence of the physical world; at any rate, as much as we are justified in accepting any scientific theory that is confirmed by observational data. Of course, there are arguments in Book I of the Treatise -- most prominently the problem of induction

raised by the discussion of causality in Part III -- to the effect that we are not justified in accepting any general hypothesis on the basis of particular data. But Hume never used these arguments against our belief in body. And consequently I will be satisfied if I can show that our impressions justify us in accepting "the external-world-hypothesis" in the same way and to at least the same extent as, e.g., scientific observations justify us in accepting quantum theory.

I am not claiming that the hypothetico-deductive method is better than causal reasoning, or that it avoids, or solves, Hume's problem of induction. I am claiming only that hypothetico-deductive reasoning is as good as causal reasoning, or induction, in justifying beliefs. Hume's problem of induction is still a problem for my view about our knowledge of the external world. But Hume argued in I:IV:2, I:II:6, and I:IV:4 that there are other problems, and it is these other problems that have led philosophers to believe that subjectivism entails skepticism about the external world. Therefore it is worthwhile to try to solve these problems, even if the problem of induction remains.

CHAPTER 3

THE CASE FOR THE PHILOSOPHICAL VIEW

This chapter has five parts. In the first I discuss what Hume means by 'constancy' and 'coherence' and the relationship between constancy and coherence. (See Treatise I:IV:2, pp. 194-195.) In the second I deal with the problem of interpreting Hume's distinction between the inferences we make from the coherence of our impressions and causal inferences (See pp. 195-198), and in doing so I show how belief in the external world is a presupposition of causal inference, at least as analyzed by Hume. In the third part I show how what I showed in the second part fits in with a general approach to the interpretation of the Treatise, and I further apply this approach to problems involved in Hume's argument that there is no logical impossibility in perceptions' existing apart from the mind. (See P. 207.) In the fourth part I examine the argument which, according to Hume, leads people to reject the Vulgar View of Body and to adopt the Philosophical View. (See pp. 210-211.) The related question of what Hume meant by 'external and independent existence' is also discussed. Finally, in the fifth part (the conclusion of the chapter), I show how there emerges from the preceding discussions a justification of, or argument for, the Philosophical View of Body.

I

I have said (in Chapter 2) that belief of the Philosophical View (PV) is justified by PV's explanatory, or systematizing, power. There are many general aspects of the sequences of our perceptions (or impressions and ideas) that would be explained by PV. Locke mentions some of them in Chapter 11 of Book IV of his Essay Concerning Human Understanding. Bertrand Russell mentions some others in Chapter II of The Problems of Philosophy. The "constancy and coherence" of our impressions that Hume picks out as the causes of our belief in "body" are just two more. Hume does argue that constancy and coherence are special in that the only thing common and peculiar to perceptions to which we attribute continued and distinct existence is their being parts of series of impressions that exhibit constancy or coherence (or their being of the type that usually are parts of such series). This is his reason for picking out constancy and coherence, rather than, e.g., the vivacity or involuntariness of impressions[1], as the causes of our belief in body -- or, as some commentators would have it, as the essential ingredients of the analysis of 'body'.[2] But from the viewpoint of PV as an explanatory

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[1]See Locke's Essay IV:xi:5. See also Descartes, Meditation VI, P. 188 of The Philosophical Works of Descartes, translated by E.S. Haldane and G.R.T. Ross (Cambridge: Cambridge University Press, 1911), vol. I. And see Hume's Treatise, P. 194 of the Selby-Bigge edition.

[2]See Price, op. cit., P. 15. See also Eric Steinberg, Hume's Attitude Toward Common Sense, Doctoral Dissertation at Columbia University, New York. 1974.

hypothesis, constancy and coherence are merely two aspects of our impressions, along with vivacity and involuntariness etc., that are explained by PV.

My reason for concentrating on Hume's discussion of constancy and coherence is that I believe a close examination of this discussion will bring to light a kind of systematizing power of PV that is much greater than any that is indicated by Locke or by Russell -- or by Hume himself, according to the usual interpretations. This great systematizing power of PV lies in its allowing us to formulate relatively simple, exceptionless universal laws. Without PV (and without any similar theory of the external world), any exceptionless universal laws would have to be much more complex; and practically speaking, even the complex laws we could formulate would be relatively few in number. Several philosophers have recognized this fact[3], but have blamed Hume for (supposedly) not recognizing it.[4] I will argue that Hume did recognize it, at least to some extent, and that he is to blame only for not realizing that it -- along with PV's offering explanations of constancy and coherence, as well as a number of other aspects of our perceptions -- justifies us in accepting, or believing, PV.

First of all, what are constancy and coherence? Hume

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[3] See, e.g., Price, op. cit., pp. 6-8.

[4] Price, op. cit., pp. 6-8; G.E.M. Anscombe, "Causality and Determination," in Ernest Sosa (ed.), Causation and Conditionals (New York: Oxford University Press, 1975), P.68.

indicates what he means by these terms primarily through exapmles. First constancy:

> Those mountains, and houses, and trees, which lie at present under my eye, have always appear'd to me in the same order; and when I lose sight of them by shutting my eyes or turning my head, I soon after find them return upon me without the least alteration. My bed and table, my books, and papers, present themselves in the same uniform manner, and change not upon account of any interruption in my seeing or perceiving them. (pp. 194-195).

> The perception of the sun or ocean, for instance, returns upon us after an absence or annihilation with like parts and in a like order, as at its first appearance. (P. 199).

> There is such a constancy in almost all the impressions of the senses, that their interruption produces no alteration on them, and hinders them not from returning the same in appearance and in situation as at their first existence. I survey the furniture of my chamber; I shut my eyes, and afterwards open them; and find the new perceptions to resemble perfectly those, which formerly struck my senses. (P.204).

In the above examples constancy seems to be a property of bodies, or physical objects -- e.g., furniture, the ocean, mountains, houses, trees -- consisting in their looking or appearing (qualitatively) the same to us as they did earlier, after intervals during which we did not perceive them. However, it seems that what Hume was looking for, and what he found in constancy, was a property of impressions that leads us to attribute continued and distinct existence to impressions that have that property. (See P. 194). I think we can safely say that Hume's reference to physical objects in his examples is merely a short-hand device for indicating the types of impressions or groups of impressions which exemplify constancy, and that constancy

is a property of some groups of noncontemporaneous impressions, consisting in (a) their being mutually resembling, or qualitatively identical, and (b) there being intervals of time separating at least some of the impressions of the group from all the earlier ones.

Now for coherence:

> Bodies often change their positions and qualities, and after a little absence or interruption may become hardly knowable. But here 'tis observable, that even in these changes they preserve a coherence, and have a regular dependence on each other.... When I return to my chamber after an hour's absence, I find not my fire in the same situation, in which I left it: But then I am accustom'd in other instances to see a like alteration produced in a like time, whether I am present or absent, near or remote. (P.195).

> I am here seated in my chamber with my face to the fire.... I hear on a sudden a noise as of a door turning upon its hinges; and a little after see a porter, who advances towards me.... I never have observed that this noise could proceed from any thing but the motion of a door.... I am accustom'd to hear such a sound, and see such an object in motion at the same time. (P.196)

> Again, I have always found, that a human body was possest of a quality, which I call gravity, and which hinders it from mounting in the air, as this porter must have done to arrive at my chamber, unless the stairs I remember be not annihilated by my absence. (P. 196)

Hume seems to make coherence, like constancy, a property of physical objects; but again I think we can say that he was merely using a short-hand device to fix the reference of 'coherence' to certain types of impressions. He intended 'coherence' to stand for a property of groups of pairs (or larger sequences) of successive impressions, consisting in (a) the first member of each pair's (or larger sequence's) resembling the first member of every other,

the second member of each pair's (or larger sequence's) resembling the second member of every other (etc.), and (b) the members of each pair's (or larger sequence's) being spatially and temporally related to each other in the same way as the corresponding (i.e., respectively resembling) members of every other pair (or larger sequence).

Having given us a rough idea of what he means by 'constancy' and 'coherence', Hume proceeds to tell us how coherence leads us to believe that some impressions exist when not present to (or not in, or not part of) the mind. For example, in the case, mentioned above, of hearing a squeak "as of a door" but not seeing the door, Hume says,

> I am accustom'd to hear such a sound, and see such an object in motion at the same time. I have not receiv'd in this particular instance both these perceptions. These observations are contrary, unless I suppose that the door still remains, and that it was open'd without my perceiving it: And this supposition, which was at first entirely arbitrary and hypothetical, acquires a force and evidence by its being the only one, upon which I can reconcile these contradictions. (pp. 196-197)

Similarly, in the example of the porter's coming to Hume's (second-story) chamber, Hume comes to believe that the stairs exist even though he does not perceive them. Finally, in still another example,

> I receive a letter, which upon opening it I perceive by the handwriting and subscription to have come from a friend, who says he is two humdred leagues distant. 'Tis evident I can never account for this phenomenon, conformable to my experience in other instances, without spreading out in my mind the whole sea and continent between us, and supposing the effects and continu'd existence of posts and ferries, according to my memory and observation.(P.196)

Again, Hume comes to believe that the sea, the continent, the posts and ferries exist though not perceived by him.

Thus it seems that we infer the unperceived, or extra-mental, existence of perceptions in cases of coherence in much the same way that we infer the existence of a cause from its effect, or of an effect from its cause, at least on Hume's analysis of causal inference. For coherence -- which may be described as the occurring, or existing, together, in a specific spatio-temporal relation, of impressions of one type A with impressions of another type B in a number of instances -- seems to be precisely what Hume calls "constant conjunction" in his discussion of causality. And so coherence inferences (e.g., inferring the existence of the opening door from the squeak) -- which consist in inferring the existence of an impression of type A outside the mind from an impression of type B which is in the mind -- seem to fit Hume's description of causal inference. In fact, Hume at one point calls coherence "the foundation of a kind of reasoning from causation." (P. 195). (But Hume claims that, and tries to explain how, our inferences from coherence differ from bona fide causal inferences. See below, Part II of this chapter.)

So much for coherence. Hume's account of how constancy leads us to attribute continued, or unperceived, existence to impressions of certain types is a much longer story. (See pp. 199-210.) But to make it short: When we have had a series of impressions that exhibit constancy (e.g., when

we have seen the entrance to the Graduate Center from across 42nd Street at intervals, a number of times) and we think about, or remember, the past and present resembling impressions, it is to us very much as if we were remembering, or thinking of, a single uninterrupted, long-lasting impression (e.g., as if we were remembering staring continuously at the entrance to the Graduate Center for a minute or more). We tend to confuse the thought of the intermittent resembling impressions with that of the single continuous, or uninterrupted, impression. (pp. 202-204). Now the notion of a single, long-lasting, uninterrupted perception itself involves some confusion; for strictly speaking, passage of time (and therefore long-lastingness) is inapplicable to a single perception, no matter how many successive perceptions it is contemporaneous with. (pp. 200-201; see also P. 37 and P. 65; and see my Chapter 4). But we have a tendency to (confusedly) apply the passage of time involved in a succession of perceptions $P_1P_2...P_n$ to a single perception P contemporaneous with the whole succession. If we think of P_1 and P_2 simultaneously, along with the contemporaneous P, we think of P twice -- once as together with P_1 and once as together with P_2; and since we know that there is really only one P, we say that P as it exists together with P_2 is <u>identical</u> with P as it exists together with P_1. (P. 201). And so when we assimilate a series of intermittent <u>resembling</u> impressions $C_1C_2...C_n$ (each C_i

existing at a different time) to P, we say that, e.g., C_2 is identical with C_1. However, we realize that identity is applicable primarily only to a single uninterrupted perception like P, not to an "interrupted," or intermittent series like $C_1C_2...C_n$. So, in order to give our attribution of identity to $C_1C_2...C_n$ an air of logical respectability, we suppose (or "feign") that there is one single, uninterrupted perception C lasting through the whole temporal interval from C_1 to C_n; that is, we think of $C_1C_2...C_n$ not as an intermittent series of resembling impressions, but as a single impression which periodically enters our minds, but continues in existence when not in our minds. (pp. 205-206). We then come to believe that $C_1, C_2, ...C_n$ are just the intermittent appearances to our mind of the single C which exists when not appearing to our mind as well, because the liveliness of the memory of $C_1, C_2, ..., C_n$ attaches to the associated supposition (or "fiction") of the single, uninterrupted C (and a belief is simply a lively idea). (pp. 208-209).

Thus Hume gives widely different accounts of the effects of constancy and coherence in leading us to believe that some perceptions exist when not present to the mind. Furthermore he seems to imply that constancy is more important than coherence in giving us that belief; for, besides devoting more time to constancy than to coherence in his discussion, he says that coherence is

> too weak to support alone so vast an edifice, as is that of the continu'd existence of all external

> bodies; and that we must join the constancy of their appearance to the coherence, in order to give a satisfactory account of that opinion. (pp. 198-199)

But H.H. Price has argued[5] that Hume should not have attached more importance to constancy than to coherence, and that he was wrong to give such different accounts of the effects of constancy and coherence; for, he argues, constancy and coherence are two "subspecies" of the same general property[6], and in fact, constancy may be viewed as just a special case of coherence.[7] A number of authors have explicitly gone along with Price on this issue.[8] But I think that Price is wrong.

Price feels that Hume, in his account of the effects of constancy, attributes too many confusions to the vulgar.[9] To exonerate the vulgar from at least one of these confusions, he attributes a mistake to Hume. Hume is mistaken, Price says, in thinking that there could be a single perception contemporaneous with each of two or more successive perceptions (i.e., with two or more perceptions that are not contemporaneous with each other). Rather what Hume thinks is a single perception contemporaneous with a succession of perceptions is really itself a succession, or series, of perceptions -- a

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[5]Price, op. cit., P. 37, pp. 59-60 ff.

[6]Ibid., P. 60.

[7]Ibid., P. 65.

[8]E.g., Penelhum, op. cit., P. 202 n.4; Bennett, op. cit., P. 323; MacNabb, op. cit., P. 129.

[9]Price, op. cit., P. 45.

"monotonous series," i.e., one in which each perception resembles (more or less) exactly every other.[10] Time is, of course, applicable to a monotonous series of perceptions, just as it is to a variegated series; the vulgar are thus exonerated from confusion on that score. Furthermore the vulgar notion of the identity of two perceptions is, according to Price, just the notion of the two perceptions' belonging to a single, uninterrupted, monotonous series, which involves no confusion.

There remains Hume's charge that the vulgar confuse an interrupted series of resembling perceptions with an uninterrupted monotonous series of perceptions. But Price assimilates this alleged confusion to the inferences we make from the coherence of our impressions. Price analyzes coherence and the inferences we make from it as follows: We experience a number of continuous series of impressions $A_1B_1C_1D_1E_1$, $A_2B_2C_2D_2E_2$,...,$A_nB_nC_nD_nE_n$, where all the A's resemble each other, all the B's resemble each other, etc. Then we experience, e.g., A_m---E_m ($m = n+1$), where the spatial and temporal relations between A_m and E_m are the same as those between A_1 and E_1, A_2 and E_2 etc., and A_m resembles all the other A's and E_m resembles all the other E's (henceforth understood). Relative to the oft-repeated series (or series-type) ABCDE, the series A_m---E_m has a gap. We assimilate A_m---E_m to ABCDE by

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[10] Ibid., pp. 46-48, (and see my Chapter 4).

postulating extra-mental impressions, or "unsensed sensibilia," $B_m C_m D_m$ to fill in the gap.[11] This assimilation is not a confusion. Rather "it is something closely akin to that passage 'from an idea to its usual attendant' which occurs, according to Hume, in causal arguments."[12]

Similarly, our "confusion" of an interrupted series of resembling impressions with a continuous monotonous series, in cases of constancy , is the assimilation of a series of the type A---A to the type AAAAA. We have experienced a number of instances of AAAAA before experiencing A---A; so again, relative to the oft-repeated series, the later one has a gap, which we fill in by postulating the appropriate type of extra-mental impressions, or unsensed sensibilia.

Thus, both constancy and coherence involve a number of instances of a "standard series" of impressions, followed by one or more series that bear a "fragmentary resemblance" to the standard series.

> Now whenever we have such a situation as this -- a frequently repeated continuous series, of whatever sort, and then a number of interrupted series resembling it -- a very important characteristic is present for which we need a special name. I am going to call this characteristic Gap-indifference: this is short for 'indifference to the occurrence of gaps'. Gap-indifference is the generic characteristic of which both Constancy and Coherence are species.... And whether this characteristic takes the form of Constancy or Coherence, the imaginitive procedure is essentially the same; it is a passage from an observed partial resemblance to a postulated

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[11] Ibid., P. 50.

[12] Ibid., P. 61.

> complete one.... Again, in both cases alike (not merely in the case of Coherence, as Hume thinks) the imaginative process could be represented as an argument from analogy. It is something closely akin to that passage 'from an idea to its usual attendant' which occurs, according to Hume, in causal arguments.[13]

The only difference between constancy and coherence is that "in the case of Constancy, the original continuous series is a monotonous one."[14] But the resemblance among the impressions in cases of constancy that makes the standard, continuous series monotonous is "irrelevant" to our postulation of extra-mental impressions. In cases of constancy as well as in cases of coherence it is the resemblance "between series of impressions, namely between a broken series and a complete one"[15] that leads us to belief in the external world. Thus constancy is not more important than coherence, as Hume implied it was. In fact, "Coherence is the more fundamental of the two.... Constancy is as it were the limiting case of Coherence."[16]

There is a lot to be said for Price's analysis of constancy and coherence. I think it is the correct analysis of coherence. But I think that in reducing constancy to a "limiting case of coherence," or in reducing both to gap-indifference, Price was missing an important point which Hume saw. Briefly put, the point is that whereas coherence leads us to postulate extra-mental impressions

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[13] Ibid., pp. 60-61.
[14] Ibid., P. 60.
[15] Ibid., P. 61.
[16] Ibid., P. 65.

filling the gaps in a broken series so that it exactly resembles a complete, standard series, constancy leads us to postulate extra-mental impressions between series. Schematically, in cases of coherence we postulate, e.g., BC filling in the gap in A--DE so that the latter, broken series will resemble exactly the standard series ABCDE. If constancy were just a limiting case of coherence, then if we experience several series of the type AAAAA, and then a week later experience a series of the type A--AA, we would postulate AA filling in the gap in the later series so that it exactly resembles the earlier ones; but we would not postulate any A's filling the week between the last of the earlier series and the later one, or between any of the earlier series. However, we must postulate a continuous series of A's between the various complete and broken series of A's that enter our minds in order for us to identify the members of one series with the members of the others. And we do identify the members of one series with the members of the others; this is a prevalent part of our beliefs about the external world. E.g., suppose I stare at the ocean from a certain point on the beach for a minute without shutting my eyes or turning my head, then turn around and go back inland, and return to that spot on the beach only a year later; and suppose that this time I interrupt my minute's staring at the ocean by periodically shutting my eyes; of course I will believe that the ocean continues to exist while my eyes are shut, but I will also believe that the ocean continued to exist during that whole year that I was away

from it. Gap-indifference or coherence, even a limiting case, could not account for the latter belief; for in order to postulate a year's worth of ocean I would have had to have stared at the ocean for more than a full year without interruption, on a number of occasions, in order to establish the standard series of impressions to which to assimilate the broken one, with the year-long gap.

According to Hume constancy is very different from coherence, and the psychological, imaginative processes to which constancy gives rise are very different from those to which coherence gives rise, with the result that the beliefs we acquire on the basis of constancy are of a different sort than the beliefs we acquire on the basis of coherence. Coherence leads us to believe that objects exist which are not present to the mind. But only constancy leads us to believe that the very same objects which are present to the mind continue to exist when not present to the mind. Coherence leads us to believe only that extra-mental impressions exist for as long as is necessary to render customary conjunctions completely constant; e.g., there must be a door turning on its rustyhinges when I hear a certain sort of squeak, but it need not be the same door that I saw earlier. Constancy, on the other hand, leads us to <u>identify</u> interrupted resembling impressions in the mind, and to believe that the <u>same</u> impression continued to exist during the interruption. It is this

identification of impressions in and outside of the mind that coherence alone is insufficient to account for. And it does not matter whether identity is construed according to Hume, as a single impression contemporaneous with a temporal succession of perceptions, or as a single continuous, monotonous series.

But even on this question of whether it is a single impression or a monotonous series of impressions that serves as the basis of our concept of identity and is involved in constancy, I think Hume's view is preferable to Price's. For although Price thinks he is helping out the vulgar by attributing to them the view that it is monotonous series of impressions, rather than individual impressions, that persist through time, I think the vulgar themselves think that individual qualities, or properties (which Hume would identify with individual, extra-mental impressions -- See below, Part IV), persist through time. I see no absurdity in properties' or impressions' persisting through time, so I do not think the vulgar need Price's help on this score. Rather I think that Hume gets himself into some inconsistencies, or at least paradoxes, by denying that an impression which is contemporaneous with a temporal succession of perceptions can be properly said to persist through time (See my Chapter 4); so the vulgar are less confused than Hume thinks they are.

Distinguishing constancy from coherence as we have done is of some importance for my overall project of

justifying the Philosophical View of Body. For part of the justification will be that the Philosophical View explains both the constancy and the coherence of our impressions. If constancy and coherence are basically different phenomena and are to be explained differently, then its explaining both of them is more of a justification of the Philosophical View than if, as Price claims, constancy is just a special case of coherence.

But it may be objected to my interpretation of Hume's differentiation of constancy from coherence that according to Hume, coherence also leads us to identify the postulated extra-mental impression with an earlier and/or later impression in our minds. For Hume does say that coherence leads him to believe that "the stairs I remember be not annihilated by my absence" (P. 196) and that "the door still remains...without my perceiving it" (pp. 196-197), rather than that some stairs or other and some door or other exist unperceived. In fact, Paul Gomberg, in a recent article, suggests that this identification of the inferred object with earlier and later perceived objects is what distinguishes coherence inferences (i.e., the inferences, or postulations, we make on the basis of coherence) from causal inferences.[17]

However, if Hume had really meant that what we infer

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[17]Paul Gomberg, "Coherence and Causal Inference in Hume's Treatise" in Canadian Journal of Philosophy, volume VI, number 4 (December, 1976).

from coherence is not only the existence of unperceived objects but also that these objects are (numerically) the same as objects which existed, perceived or unperceived, before, then presumably he would have mentioned this latter aspect of coherence inferences when he tried to show how these inferences differ from causal inferences (pp. 197-198). Since, as we shall see in the next part of this chapter, he did not mention it, but instead differentiated causal from coherence inferences in a much less clear-cut way, it appears that such expressions as "the stairs I remember" and "the door still remains" were merely slips of the pen, and that according to Hume coherence alone does not lead us to identify the object whose existence we infer with something which we believe existed earlier. So we are free to interpret Hume as saying that it is constancy, not coherence, that leads to such identification.

Of course, if, as Gomberg claims[18], we cannot make sense of the way Hume tries to distinguish causal from coherence inferences, and that therefore Hume was confused about one or both of these inferences when he was explaining the distinction between them, then it will not be quite so clear that his failure to mention an identification-aspect of coherence inferences shows that there is no such aspect. But first of all, I think that it is implausible that Hume should have been so confused as to overlook such

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[18] Ibid., pp. 693-698.

an obvious difference between causal and coherence inferences if he originally intended that difference to be there. Secondly, we can understand Hume's treating constancy and coherence differently if we interpret him as holding that constancy, but not coherence, leads us to identify postulated and perceived objects. Finally, and most importantly, I think that we can make sense of Hume's way of distinguishing causal from coherence inferences, and that he was much less confused about the nature of these two types of inference than Gomberg claims.

II

As has already been mentioned (above, P. 36), it seems that coherence inferences and causal inferences are basically the same according to Hume. Both consist in inferring an unperceived A from a perceived B after having previously perceived A and B constantly conjoined.[19]

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[19] One possible difference between coherence and the constant conjunction involved in causality is that in the latter the spatio-temporal relation between each instance of A and the corresponding instance of B is spatial and temporal contiguity, whereas in the former the corresponding A's and B's need not be contiguous in space and time, as long as the spatio-temporal relation is the same in all instances. However, spatial contiguity is inessential to the causal relation, since the latter relation may hold even between things which are not located in space at all. (See P.75n. and correlative text. See also I:IV:5.) And though Hume argues that temporal contiguity is essential to the causal relation, he remarks, after presenting the argument, that "the affair is of no great importance." (P. 76).

However, Hume claims that coherence inferences are not, and insinuates that they are in some sense inferior to, causal inferences:

> But tho' this conclusion from the coherence of appearances may seem to be of the same nature with our reasonings concerning causes and effects; as being derived from custom and regulated by past experience; we shall find upon examination, that they are at the bottom considerably different from each other, and that this inference [i.e., coherence inference] arises from the understanding and from custom in an indirect and oblique manner. (P.197)

He explains what he means by "in an indirect and oblique manner" as follows:

> whenever we infer the continu'd existence of the objects of sense from their coherence and the frequency of their union, 'tis in order to bestow on the objects a greater regularity than what is observ'd in our mere perception. We remark a connexion betwixt two kinds of objects in their past appearance to the senses, but are not able to observe this connexion to be perfectly constant, since the turning about of our head, or the shutting of our eyes is able to break it. What then do we suppose in this case, but that these objects still continue their usual connexion, notwithstanding their apparent interruption, and that the irregular appearances are join'd by something, of which we are insensible? (pp. 197-198)

Thus the conjunctions involved in coherence inferences are not "perfectly constant," so our inferring an extramental perception of type B from an impression of type A in our minds is not due simply to the "custom" of having impressions of type A <u>always</u> accompanied by impressions of type B. Rather we tend, by means of coherence inferences, to render these conjunctions more constant than they have been observed to be. Hume explains this tendency by subsuming it under a more general one -- what H.H. Price calls "The Inertia Principle"[20]:

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[20] Price, <u>op. cit.</u>, pp. 54-55.

> I have already observ'd, in examining the foundation of mathematics, that the imagination, when set into any train of thinking, is apt to continue, even when its object fails it, and like a galley put in motion by the oars, carries on its course without any new impulse.... The same principle makes us easily entertain the opinion of the continu'd existence of body. Objects have a certain coherence even as they appear to the senses; but this coherence is much greater and more uniform, if we suppose the objects to have a continu'd existence; and as the mind is once in the train of observing an uniformity among objects, it naturally continues, till it renders the uniformity as compleat as possible. (P.198)

Since Hume intended the above remarks as indicating how coherence inferences differ from causal inferences, it seems that according to Hume, whereas the conjunctions, or regularities, on which coherence inferences are based are not perfectly constant, those on which causal inferences are based are perfectly constant, so that coherence inferences require the "Inertia Principle," while causal inferences do not. The problem is that actual , ordinary causal inferences seem to be based on conjunctions that are just as inconstant as those on which coherence inferences are based, and that therefore the Inertia Principle is required just as much in causal as in coherence inferences. We may be tempted to say, e.g., that we have observed an absolutely constant conjunction between stones' being dropped from our hands and those stones' falling to the ground. But it is extremely unlikely that in every case in which we had an impression of dropping a stone from our hand we also had an impression of the stone's falling. After all, we may have, on some occasion, turned our head or shut our eyes and so not gotten the

impression of the stone's falling to the ground. If such things as "the turning about of our head or the shutting of our eyes" can render conjunctions less than perfectly constant, then truly constant conjunctions are extremely rare; so causal inferences, if they must be based on conjunctions which are perfectly constant, are much more infrequent than we might think. Several commentators have noticed the difficulty of interpreting Hume's distinction between causal and coherence inferences without rendering not causal according to Hume such inferences as that of a stone's falling after being released from one's hand.[21] Several interpretations have been suggested, but none is completely satisfactory.

One issue that has been raised in discussions of this problem is how to interpret Hume's statement, on pp. 197-198, that the conjunction in a coherence inference is not perfectly constant, and his implication that the conjunction in a causal inference is perfectly constant.[22] Let us suppose that a coherence inference is made at time t. Is Hume saying that the conjunction of A and B was not perfectly constant even <u>before</u> <u>t</u>, or only that <u>at</u> <u>t</u> we have an impression of A but no impression of B? I see no clear indication in the passage itself one way or the other. Price seems to opt for the second

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[21] See Gomberg, <u>op. cit.</u>, P. 693 for references.

[22] See Gomberg, <u>op. cit.</u>, P. 697.

interpretation. Hume, according to Price, is saying that in coherence inferences we have an impression of A but no impression of B at t, so in inferring a B at t we are rendering perfectly constant what is (at t) observed to be an imperfectly constant conjunction. Causal inference, on the other hand, is simply inference from perfectly constant conjunction of A and B in the past (or in observed cases) to perfectly constant conjunction of A and B in the future (or in unobserved cases); there is no "contradictory phenomenon" of an impression of A unaccompanied by an impression of B. Thus, according to Price, Hume's distinction between causal and coherence inference is as follows: Causal inference is projection onto the future of a past (and present) perfectly constant conjunction; coherence inference is inference of one conjunct from the other, based on past (but not present) perfectly constant conjunction.[23]

However, Price's view misrepresents Hume's conception of causal inference.[24] Hume explicitly states that causal inference too is inference of the unperceived existence of a B from an impression of an A unaccompanied by an impression of a B:

> In all those instances, from which we learn the conjunction of particular causes and effects, both the causes and the effects have been perceiv'd by the senses, and are remember'd: But in all cases,

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[23] Price, op. cit., pp. 51-55.

[24] See Gomberg, op. cit., P. 703 n. 10.

> wherein we reason concerning them, there is only one perceiv'd or remember'd, and the other is supply'd in conformity to our past experience. (P. 87)

Once we recognize that causal inference is inference of the unobserved conjunct of an erstwhile constant conjunction, it will seem highly unlikely that the ambiguous passage on pages 197-198 is to be interpreted as Price interpreted it. For it is highly unlikely that Hume was so muddle-headed as to try to show how coherence inferences differ from causal inferences by spelling out an aspect of the former which he had explicitly stated to be an aspect of the latter as well -- i.e., that the occasion of the inference is the having of an impression of A but no impression of B. Yet Gomberg, though he recognizes Price's error with respect to causal inference, nevertheless adopts Price's interpretation of the passage in question. Not surprisingly, he concludes that Hume has not successfully distinguished coherence inference from causal inference. Furthermore, Gomberg thinks that there is an important difference between causal and coherence inferences as described by Hume (See above, P.46), a difference which Hume somehow overlooked in attempting to distinguish between them. Gomberg thus attributes to Hume confusions of epic proportions -- Hume is viewed as in the same position as one who would try to distinguish between dogs and chairs by pointing out that chairs generally have four legs. To top things off, Gomberg says that Hume realized only in his discussion of

coherence inferences that "the turning about of our head or the shutting of our eyes" tends to make most conjunctions less than perfectly constant, but that he did not have the presence of mind, or the intellectual honesty, to qualify his earlier statements about causal inferences accordingly.[25]

Of course, the above remarks about Gomberg's interpretation can count as a criticism only if we can come up with an interpretation according to which Hume is less confused than Gomberg's interpretation makes him out to be -- or at least an interpretation according to which Hume's confusions are more plausibly attributable to a philosopher who is so subtle and clear-headed in most other cases. But it seems unlikely that we can exonerate Hume from all confusion on this issue. For, as has already been mentioned, even if we interpret Hume as saying, in the key passage, that in coherence inferences the conjunctions were not perfectly constant <u>before</u> the occasion of the inference, we still have the problem that the same is true in most cases of what we generally think of (and what we generally think Hume was thinking of) as causal inferences, so that Hume will not have successfully distinguished between causal and coherence inferences. If we nevertheless say that Hume calls an inference <u>causal</u> (if and) only if it is based on a past observed

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[25]Gomberg, <u>op. cit.</u>, P. 699.

perfectly constant conjunction, we get the further untoward result that we can causally infer a B from an A at most once; for when we causally infer a B from an A, we observe an A but not a B, so we will never subsequently be able to say truly that the conjunction of A and B has been observed to have been perfectly constant until now. It would not help matters to say that the past conjunctions in cases of causal inference need not have been observed, but only believed, to have been perfectly constamt; for then we would again be without any distinction between causal and coherence inferences, since in the latter also we come to believe, via inferences, that the conjunctions are perfectly constant.

However, I think we can free Hume from all the charges of confusion on this issue of causal vs. coherence inferences if we attribute to him the view that the conjunctions involved in most causal inferences are between (types of) events involving enduring external objects which we perceive under certain conditions, while the conjunctions involved in coherence inferences are always between (types of) events involving "fleeting impressions." For nods of the head and shuttings of the eyes are not nearly as devastating to the constancy of conjunctions between (types of) events involving external objects as they are to the constancy of conjunctions between (types of) events involving fleeting impressions, especially if we have certain beliefs (i.e., the usual ones) about the conditions in which we will and those in which we will not perceive

existing external objects. If A and B are types of events involving external objects, then if I turn my head or shut my eyes after seeing an A, then I will expect not to see a B whether or not , or even if, a B in fact occurs. So my not seeing a B is irrelevant to the question of whether A and B are constantly conjoined. The only relevant cases of A's or B's are those in which we are in a position to perceive both the A and the B if they occur. If A and B have been conjoined in all such cases, then we (causally) infer that they were conjoined in all other cases, including the present one in which we perceive an A but no B. But if we do not yet have beliefs about when we will perceive what external objects, and if A and B are types of events involving perceptions, then not having an impression of B is, for the purpose of establishing a constant conjunction between A and B, the same as observing that (at least probably) there is no impression of B; and our inferring the existence of an impression of B in this case involves going against what our experience tells us, and rendering the conjunction of A and B more constant than it has been observed to be.

Thus, if we interpret Hume as allowing that the terms of the causal relation may be not only events involving perceptions in our minds, but also events involving external objects which we perceive only under certain circumstances, then we can make sense of his distinction between causal and coherence inferences. For

there is a clear sense in which conjunctions of (types of) events involving external objects are perfectly constant, even though the conjunctions between the corresponding (types of) events involving impressions in our minds are not perfectly constant. So Hume can reserve the term 'causal' only for inferences based on perfectly constant conjunctions, without thereby ruling out, as non-causal, most of the inferences which we would ordinarily say are causal. Coherence inferences, then, differ from causal inferences in being based on conjunction -- of (types of) events involving impressions in our minds -- which are not perfectly constant. Furthermore, this interpretation fits in with the general contexts of Hume's discussions of causal and coherence inferences. For Hume introduces coherence inferences as part of what gives us belief in the continued and distinct existence of an external world, so those who make coherence inferences must be predisposed to attribute existence primarily (i.e., except in actually making the coherence inferences) to perceptions that are present to them; whereas Hume's analysis of causal inferences is presumably an analysis of inferences made by normal adults, who believe in an external world and whose everyday thoughts are primarily about this external world. Moreover, on this interpretation we can understand why Hume calls our having an impression of A but no impression of B "a <u>contradiction</u> to all past experience" (P. 196) in the context of coherence inference; for 'I had an impression of

A at t but no impression of B at t' really is the contradictory of 'Whenever I have an impression of A I have an impression of B'. But in the context of causal inference, which generally presupposes belief in the external world and our piecemeal perception of it, having an impression of A unaccompanied by an impression of B is not necessarily a counter-example to the generalization that (external events) A and B are constantly conjoined.

It should also be noticed that this distinction between causal and coherence inferences, based on our having already developed a view about the external world and our perception of it when we make inferences that can be classified as causal, but not yet developed such a view when we make coherence inferences, holds for either interpretation of the ambiguous passage on pp. 197-198 of the Treatise. As before, let t be the time at which a coherence inference of an impression of B from an impression of A is made. If we interpret Hume to be saying, in the passage in question, that the conjunction of impressions of A and B was not perfectly constant before t, then the distinction is straightforward; for in a causal inference, though we may not have had an impression of B whenever we had an impression of A, nevertheless we have had an impression of B in all the relevant instances in which we had an impression of A -- i.e., in all the instances in which we were in a position to perceive a B if it occurred -- so that, as far as we can tell, the conjunction between (external events of types) A and B was perfectly constant. And if we interpret Hume as saying that

only at t we have an impression of A but no impression of B, then the distinction between the coherence inference and a parallel causal inference is that in the latter our not having an impression of B is irrelevant to whether a B occurred, but in the former it is relevant and militates against the occurrence of a B. On either interpretation the coherence inference must proceed from somethingstronger than mere habit or custom, for it entails coming to believe that a conjunction is more constant than experience indicates; whereas causal inference could very well proceed from habit or custom, as Hume argued.[26]

Perhaps itshould be added, to avoid misunderstanding, that in saying that the conjunctions involved in most causal inferences are between events involving enduring external objects certain of which we perceive under certain circumstances, while the conjunctions involved in coherence inferences are between events involving short-lived impressions, the emphasis is not on "enduring external object" vs. "short-lived impressions." For according to Hume, 'enduring external object' is intelligible only if it means the same as 'perception with continued and distinct existence' (See my Chapter 2); and in coherence inferences, as well as causal inferences, what we infer are perceptions with continued and distinct existence. Rather the emphasis is on beliefs about the types of

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[26]It may seem that it is coherence inferences, rather than causal inferences, that can properly be said to involve habit. Price, op. cit., pp. 55-58, says that 'habit' was the wrong word for what Hume had in mind.

circumstance in which we do and those in which we do not perceive what is there vs. the lack of such beliefs. For if we lack such beliefs, then a perception's not being present to us, or in our minds, at a given time t counts against its existing at t; but if we have such beliefs, then if we are in circumstances in which we believe that we would not have in our minds at t a particular perception P even if it existed at t, then P's not being in our minds at t does not in any way militate against its existing at t. Thus, any coherence inference must override evidence against the existence of the inferred perception, while in causal inferences there is no such negative evidence to overcome.

It may be objected that the fact that what counts as negative evidence in cases of coherence inference is neutral in cases of causal inference may make causal inferences more justified, or warranted, than coherence inferences; but Hume was more interested in distinguishing causal from coherence inferences on the basis of the different psychological processes he thought were involved in the two kinds of inference. The process involved in causal inferences is supposed to be simply habit, while coherence inferences involve also the inertia principle. Now, it is admitted that in causal inferences, as well as coherence inferences, the impressions of A in our minds are not constantly conjoined with impressions of B in our minds. The difference is merely that in causal inferences,

but not in coherence inferences, the cases in which an' impression of A but no impression of B is in our minds are irrelevant to the question of whether A and B are really constantly conjoined. But this difference does not make the required psychological difference. For the fact that impressions of A and B have not been constantly conjoined in our minds -- relevant or not -- entails that our inferring a B from an A is not just a matter of habit (See bottom half of P. 197): causal inferences, as well as coherence inferences, must involve the inertia principle. So my claim, that causal inference, but not coherence inference, presupposes beliefs about the external world and our perception of it, fails to support Hume's psychological distinction between causal and coherence inferences. It therefore seems unlikely that Hume had my claim in mind in making the distinction. (Or if he did, he was guilty of at least one confusion.)

I answer that my claim does support Hume's distinction, at least on Hume's own principles. For I think it is not at all implausible that Hume would say that what is irrelevant from a logical or rational point of view (in this case our having an impression of A but none of B because, e.g., we turned our head) is also irrelevant to the formation of a habit. That is, just as constant conjunctions feel different from inconstant conjunctions, so inconstant conjunctions where the exceptions (or inconstancies) are believed to be irrelevant feel

different from inconstant conjunctions where the exceptions are not believed to be irrelevant. Consider, for example, his discussion of abstract reasoning (I:I:7), in which he notes the mind's ability to automatically keep "in readiness" (more or less) all and only the relevant ideas. Furthermore Hume acknowledges the role of reasoning in the forming of the habits that are involved in causal inference when he says that sometimes a single experiment creates such a habit via the principle that nature is uniform. (I:III:8, pp. 104-105). In coherence inferences, negative instances of 'B whenever A' are ignored simply because they are negative -- this is inertia. But in causal inferences, the "negative" instances are believed to be irrelevant, and as a result these instances have as little effect on our associating A with B as do cases in which neither A nor B is perceived; they do not come into play at all, so the inertia principle is not needed to counteract their (non-existent) influence -- causal inference is thus purely a matter of habit.

Assuming now that we have correctly distinguished coherence inferences from causal inferences, a question still remains as to whether -- and if so, how -- coherence inferences differ from probabilistic inferences. For in probabilistic, as well as in coherence inferences, the conjunction of A and B has been observed to be less than perfectly constant (even if we count only the relevant observations), and yet we infer a B from an observed A.

Hume does not adress the question. But I think the tone of his discussion of coherence inferences and of our belief in the external world indicates that he felt that coherence inferences are even less justified than probabilistic inferences.

One way in which coherence inferences might differ from, and be less justified than, probabilistic inferences is that the strength of our belief in the conclusion of a coherence inference is greater than the strength of our belief in the conclusion of a probabilistic inference based on the same ratio of positive to negative instances of the respective conjunctions. That is, if we probabilistically infer a B from an observed A, the degree of confidence we have in our inference is proportional to the ratio of cases in which we have observed an A conjoined with a B to (relevant) cases in which we have observed an A not conjoined with a B. But in a coherence inference of a B from an A, presumably the inertia principle totally negates the influence of past cases in which we have observed an A not conjoined with a B, so that we have maximum, or complete confidence in our inference, as if the conjunction of A and B were perfectly constant.

But I think there may be a more fundamental, more significant difference between probabilistic and at least some coherence inferences: We probabilistically infer a B from an observed A only if A and B were conjoined in <u>most</u> of the relevant cases; if in most of the relevant

cases they were not conjoined, then we would infer that probably no B occurred, and if they were conjoined in exactly half of the relevant cases, we would not make an inference at all. But judging from some of Hume's examples -- especially the one of a letter's leading us to posit posts and ferries, seas and continents (P. 196) -- I think there are coherence inferences of a B from an A in which A and B have been conjoined in fewer than half of the relevant instances (for in coherence inferences, all instances are relevant). In such cases the psychological mechanisms operating in probabilistic inferences would lead us to conclude that (probably) a B did not occur. But if there is no other type of impresseion C such that A and C have been conjoined more often than, or as often as, A and B, then the inertia principle leads us to infer a B from an A by simply pushing all the negative instances of 'B whenever A' out of the picture entirely.

Attributing to Hume the view that the causal relation is in most cases a relation between events in an external world which we perceive only partially and only under certain conditions, while coherence is a relation among the mind's "fleeting impressions" is the only way I know of to make sense of Hume's distinction between causal and coherence inference. But I hesitate to attribut this view to Hume, because he nowhere (as far as I know) comes out and states it. The fact that he carries on his discussion of causality in the language of realism with respect to the

external world proves little, since, as we have seen, he often lapses into such realist language even when he has said that he is talking about the properties of our perceptions that cause us to believe in an external world. There are some passages that may be interpreted as stating the view in question, but none of them strictly require such an interpretation. E.g., after saying, "We may well ask, What causes induce us to believe in the existence of body? but 'tis in vain to ask, Whether there be body or not?" he adds, "That is a point which we must take for granted in all our reasonings." (P. 187). Now in deductive reasoning -- i.e., reasoning about "relations of ideas" -- we certainly need not "take for granted" the existence of "body." Does he mean, then, that in our reasonings about matters of fact -- i.e., in causal and probabilistic reasoning -- we must presuppose the existence of the external world? Perhaps. But on the other hand, perhaps the phrase "in all our reasonings" should not be taken so seriously, for it may very well have been thrown in purely for rhetorical or dramatic effect. (See the context on P. 187.) One would think that if Hume did have in mind the view in question, he would have brought it in to explain what he says in distinguishing coherence inferences from causal inferences. The retort that Hume only wanted to point out the psychological difference between the two types of inference, and felt no need to explain the difference (and if he had, we

might have asked why he did not go on to explain his explanation, etc. ad infinitum) does not seem satisfactory, since his statement of the difference, without its explanation, has proven to be so difficult to understand.

On the other hand, Hume certainly did realize that relations among objects are much more regular, or uniform, if our belief in the external world is true than if all there are are the perceptions in our minds: "Objects have a certain coherence even as they appear to the senses; but this coherence is much greater and more uniform, if we suppose the objects to have a continu'd existence." (P. 198) And it seems plausible that he realized that the infernces he was analyzing in the sections on causality are made in the context of belief about the external world and our partial and piecemeal perception of it. So I think the most likely story is that Hume was aware of all the parts of the view I would like to attribute to him, but when he came to write the passage in which he distinguished between causal and coherence inferences, he was not thinking clearly enough to bring all the parts together.

There may appear to be a difficulty in reconciling the claim that most causal inferences presuppose belief in the external world with Hume's saying (P. 231 and P. 266) that causal inferences and belief in the external world conflict. But this difficulty is only apparent, as can be

seen simply by specifying more fully how causal infernces conflict with our belief in the external world.

> When we reason from cause and effect, we conclude, that neither colour, sound, taste, nor smell have a continu'd and independent existence. When we exclude these sensible qualities there remains nothing in the universe, which has such an existence. (P. 231)

Hume had argued (pp. 226-227) that certain premises, which include the causal principle -- like effects proceed from like causes --, entail that there is nothing outside the mind that resembles our perceptions of the secondary qualities; and he had further argued (pp. 228-231) that primary qualities without secondary qualities are inconceivable. It would follow that no qualities -- neither primary nor secondary -- exist outside the mind. Thus the causal principle, conjoined with certain other premises, entails that our belief in the external world is false. But this entailment is compatible with most causal inferences' presupposing our belief in the external world. The problem is that the causal principle, since belief in it is based on belief in the external world, must itself be rejected if we reject the latter belief. But this problem fits right in with the problems that Hume discusses at the end of Book I of the Treatise (pp. 265-267), though he does not mention this specific problem.

This rather lengthy discussion of Hume's distinction between causal and coherence inferences has an important moral: Our beliefs about the external world are useful

in organizing our experience. They provide a framework within which we can subsume impressions and series of impressions under universal laws. For it is only with the help of these beliefs that we are able to frame the vast number of universal laws, causal and non-causal, that we do. There are relatively very few simple conjunctions, or regularities, among our impressions of sensation that are even close to constant, or universal. But within the framework of our beliefs about the external world and our perception of it, we have discovered conjunctions, or regularities, among types of external objects and events, which are constant, or which hold universally, as far as we can tell. And based on these universal regularities, we can make predictions about impressions. The fact that so many of these predictions turn out to be true -- thus keeping the regularities universal, as far as we can tell -- is the best evidence we have that our beliefs about the external world are true.

III

The view that according to Hume, causal inference presupposes our already having, while coherence inference presupposes our not yet having, certain beliefs about the external world is part of a more general thesis, about the interpretation of Hume's Treatise. This thesis is, in a nutshell, that Hume, in explaining how we come to

have certain beliefs or sentiments, often presupposes a specific context of antecedently acquired beliefs, the acquisition of which he accounts for elsewhere in the Treatise.

The view I am advocating is not that once Hume has explained why we all have a certain unjustified belief, he is free to use that belief simply because he, like everyone else, cannot help having it. I doubt whether it is possible to actually have a belief while also believing that that belief is not rationally justified.[27] What I am claiming is that Hume sometimes tells us, in effect, that part of the cause of most people's having one belief is their already having, or not yet having, another belief. (E.g., I may believe neither that there is life on Mars nor that a particular UFO is a Martian spaceship; but I could still explain others' belief that there is life on Mars as based on their believing that the UFO is a Martian spaceship.)

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[27] It seems to be a basic part of Hume's philosophy that he (like everyone else) believes, e.g., in the existence of body and in the validity of induction even though he (unlike most people) also believes that these beliefs are unjustified. If so, then I think this part of his philosophy is false, or perhaps even absurd. But there are passages (e.g., the last paragraph of I:IV:2, P. 218) which seem to indicate that Hume held that at the time that one firmly believes that a certain belief is unjustified one cannot hold that belief, and that it is only later -- "an hour hence" -- that he can come to hold that belief; and presumably then he will no longer believe that the belief is unjustified.

It is now generally recognized that the Treatise of Human Nature is not just a compendium of arguments for skepticism about various issues. It is acknowledged that Hume, in writing the Treatise, was as much (or more) interested in giving a naturalistic, psychological account of our commonsense and common philosophical beliefs as (than) in showing that many of these beliefs are not rationally justified. Now it seems to me that any account of how we come to have certain beliefs would lack plausibility if it assumed (without evidence) that each of these beliefs developed in complete isolation from all of the others. For it seems likely -- if there is no evidence to the contrary -- that the explanatory factors in the acquisition of some of these beliefs should include already having acquired some of the others, or at least that there should be some relation between the development of one belief and the development of another that is relevant to a psychological account of the acquisition of these beliefs.[28] Hume nowhere argues specifically that there is no such relation among the various beliefs that he tries to account for. On the contrary, he sometimes states explicitly that our coming to have one belief or sentiment depends on our already having another -- e.g., he argues at the end of I:IV:2 (pp. 211ff.) that a necessary condition of anyone's adopting the Philosophical View of Body is his having previously held the Vulgar View,

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[28]See Penelhum, op. cit., P. 200 n.9.

and in II:I:10 he says that the pride which people tend to have in their wealth is dependent on their having already developed the tendency to make, and having already made, certain probabilistic inferences. But there are also parts of the Treatise in which, though Hume does not explicitly state that a belief which he accounts for elsewhere is relevant, we can make sense of what he says only if we assume something about that belief. Thus, e.g., some commentators have found difficulty understanding Hume's references to an idea of a (presumably unified, persisting) self in Book II of the Treatise after he had shown in I:IV:6 that we have no such idea[29]; but we can understand these references if we remember that Hume, in I:IV:6 explained how we come to believe that we have an idea of a unified, persisting self, and that he pointed out the idea which we mistake for, and therefore which is for us, an idea of such a self: the passions which involve reference to a self presuppose the mistake and the belief accounted for in I:IV:6. Similarly, as has been explained, our ordinary causal inferences, as analyzed by Hume, presuppose our already having beliefs about the external world as accounted for in I:IV:2. The fact that a particular belief is, according to Hume, unjustified -- as are our beliefs about the external world and about the self -- does not disqualify it from being the basis of another

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[29]See Norman Kemp Smith, The Philosophy of David Hume (London: MacMillan, 1941), P.v.

belief; it entails only that the latter belief, insofar as it is based on the former, is itself unjustified.

A good example of Hume's presupposing, in his account of our acquiring a certain belief, our not yet having a certain other belief which he discusses elsewhere is his explanation of how the vulgar can entertain the seemingly absurd hypothesis that perceptions can exist when not in any mind (pp. 206-208), which is part of his account of our acquiring belief in the existence of the external world. Hume is aware that he must deal with the objection that the notion of impressions' existing when not in a mind is self-contradictory; thus he asks "how we can satisfy ourselves in supposing a perception to be absent from the mind without being annihilated." (P. 207). He answers:

> what we call a mind is nothing but a heap or collection of different perceptions, united together by certain relations, and suppos'd, tho' falsely, to be endow'd with a perfect simplicity and identity. Now as every perception is distinguishable from another, and may be consider'd as separately existent; it evidently follows, that there is no absurdity in separating any particular perception from the mind; that is, in breaking off all its relations, with that connected mass of perceptions, which constitutes a thinking being. (P. 207)

Thus Hume's theory of the mind, or self, which he will elaborate in I:IV:6, entails that there is no absurdity in impressions' existing when not in a mind.

But there are two questions that Hume should be asking about the vulgar belief in the continued existence of impressions -- (1) whether the notion of impressions' existing when not in a mind is really consistent, and

(2) whether the vulgar can accept that notion as consistent, i.e., whether they believe that it is consistent and therefore worthy of being entertained. For if the answer to either of these questions were negative, then Hume's account of how we come to believe in the external world would be defective: If the answer to (1) were negative, then he would have to explain how we could conceive of (or think we were conceiving of) impressions' existing when not in a mind. If the answer to (2) were negative, he would have to explain how the vulgar, in coming to adopt the view that certain impressions exist when not in a mind, neutralize, or overcome, their belief that the view is absurd. The problem is that Hume's answer seems to be addressed only to question (1) -- i.e., he shows that, according to his theory of the mind, the notion of impressions' existing when not in a mind is consistent. It seems that his answer cannot be interpreted as an answer to question (2) unless he is attributing to the vulgar his own sophisticated theory of the mind; but as Barry Stroud points out[30], he had said earlier (P. 189), "'Tis certain there is no question in philosophy more abstruse than that concerning identity, and the nature of the uniting principle, which constitutes a person....and in common life 'tis evident these ideas of self and person

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[30]See Barry Stroud, Hume (Boston: Routledge and Kegan Paul, 1977), pp. 106-107; also Bennett, op. cit., P. 346.

are never very fix'd nor determinate," so how could he attribute this theory to the vulgar? Indeed it seems that most people, if they thought about it at all, would think that impressions' existing when not in a mind is absurd.

To resolve this difficulty it is necessary first to distinguish two different parts of Hume's theory of the mind, or self: (i) All that our senses (internal and external), aided by reason, acquaint us with is a bundle of perceptions. (ii) But our imagination, working according to the rules of association, leads us to postulate a persisting, simple substance which unifies all the perceptions of the bundle, and we call this substance, as opposed to the bundle of perceptions that it is supposed to hold together, the self proper. It is (ii) which is Hume's answer to the question than which "no question in philosophy is more abstruse." That is, it is difficult to give a satisfactory psychological account of how we come to attribute unity and simplicity to the self; in fact, Hume admits, in the Appendix to the Treatise (pp. 633-636) that he is unable to provide a completely satisfactory account. As for (i), it seems to be basically a matter of introspection, and should be clearest to people who have not yet been carried away by their imaginations into postulating a persisting, substantial self.

Next we should notice that in order to account for the vulgar's not finding any absurdity in impressions'

existing when not in a mind we need attribute to them bleief only of (i), not of (ii). More precisely, when the vulgar first attribute continued, extra-mental existence to perceptions, they must still be at the stage at which they see the mind for what it is and (before the imagination does its work) appears to be -- i.e., a disconnected heap of perceptions. For, once they come to think of perceptions as inhering in a substantial mind, or self, it will be difficult for them to attribute extra-mental existence to them.

Thus, in order to make Hume's argument on P. 207 coherent, we must add the premise that the vulgar's belief in the external world antedates their belief in the substantial unity of the mind, or self. At least, according to Hume, by the time we develop a view of the self inconsistent with our impressions' existing when not in a mind, the imaginative process that leads us to attribute extra-mental existence to impressions must have gone on long enough, or gathered enough momentum, not to be stopped by this development. Whether it is true that people's belief in an external world antedates their belief in a unified self in which all their perceptions inhere is a question for empirical genetic psychology. But Hume's giving a positive answer to this question would not be without precedent; for Locke apparently thought it obvious that people generally achieve a relatively advanced knowledge of the external

world before they reflect on "what passes within" (Essay II:I:8), and even Descartes felt that his saying that our knowledge of our minds was more "clear and distinct" than our knowledge of bodies would strike most people as obviously false (Meditation II).

IV

Another case of Hume's implicitly assuming a particular context of beliefs is his discussion (on pp. 210-211) of the "experiments" which are supposed to show that what we are directly aware of in perception is "not possest of any distinct or independent existence" (P.211) -- i.e., his version of the arguments from illusion, hallucination, and perspectival relativity against naive realism. Besides being another example of my thesis about the interpretation of the Treatise, Hume's treatment of these arguments is important in that, as I hope to show in this section, it provides a good reason to prefer the Philosophical to the Vulgar View of Body. In understanding Hume's account of these arguments, it is important to be aware of the context in which it is presented. Hume has just finished explaining how the vulgar come to believe that what is present to our minds in perception continues to exist when not before our minds, and is therefore independent of us. He is now explaining how some of the vulgar, after having adopted this belief,

reflect on such phenomena as the double vision caused by pressing an eyeball and the various changes in what is before our minds consequent on changes in our sense-organs or in our "nerves and animal spirits," and how such reflections lead these people to become "philosophers" -- i.e., to adopt a representative, or causal, theory of perception. If we keep this context in mind, we will not encounter some of the difficulties that some philosophers have found with Hume's discussion.

For example, it is sometimes alleged that Hume, who claims that the only things we are rationally justified in believing exist are the perceptions in our minds while they are in our minds, is not entitled to use as a premise anything about the unobserved states of our "nerves and animal spirits."[31] It is similarly argued that he is not entitled, in the "experiment" involving double vision, to assume that one of the objects before our minds has continued and independent existence and the other does not.[32] But it is not Hume who is making these assumptions. Rather Hume is reporting how certain perople, who do believe in an external world, reason about cases of double vision, distortions in our visual

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[31]Price, op. cit., P. 115. See Mandelbaum, op. cit., pp. 139-140.

[32]See John W. Cook, "Hume's Scepticism with Regard to the Senses," in American Philosophical Quarterly 5 (1968), P.12. See also R.J. Hirst, The Problems of Perception (New York: Humanities Press, 1959), P.47, and Mandelbaum, op. cit., P.128 for arguments like this.

field due to our illnesses, etc. And in the context of their view about the external world, these people are entitled to assume that when we press an eyeball, only one of the things we seem to see exists independently of us, and that there are unobserved goings-on in our "nerves and animal spirits."

But it has been claimed that, whether or not whoever makes these assumptions is entitled to make them, if these assumptions are used as Hume says they are, i.e., as premises in arguments leading to rejection of the vulgar view of perception and the external world, then they invalidate those arguments. For, e.g., we cannot validly infer, from premises one of which is that one of a pair of impressions has and the other lacks independent existence, that neither of the impressions has independent existence. Generally, it is claimed, in each of the arguments mentioned by Hume in the paragraph spanning pp. 210-211, and in most (if not all) versions of the argument from illusion, the premises are true only if the vulgar view (or naive realism) is true; so the denial of the vulgar view (or of naive realism) cannot be validly inferred from them.[33]

However, it is simply a mistake to think that the negation of a proposition p cannot be validly inferred from premises that are true only if p is true. Arguments

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[33]Mandelbaum, op. cit., pp. 123-148.

in which such inferences are made are usually called reductio ad absurdum, or indirect proofs. So the argument from illusion is best interpreted as a reductio ad absurdum of naive realism, in which the tenets of naive realism are shown to entail the negation of one or more of those tenets.[34] This interpretation fits Hume's treatment of the argument particularly well. For Hume was not arguing directly against the vulgar view from some standpoint outside that view. Rather he was showing how people who hold the vulgar view come to refute it -- so to speak, from within: The vulgar believe, e.g., about the two objects before the mind in cases of double vision caused by pressing an eyeball, that (a) one of those objects depends for its existence on the pressing of the eyeball, the other does not, (b) the two objects are exactly alike, and (c) when two objects are exactly alike, their causes are exactly alike. But (a),(b), and (c) are mutually inconsistent; it follows from (b) and (c) that (a) is false, i.e., that if one of the objects is causally dependent on the state of the eye of the perceiver, then so is the other. Generally it follows, from (c) plus the fact that impressions which are causally dependent on the state of the perceiver are of the same nature as impressions which we tend to believe are independent of the perceiver, that the latter are also dependent on the state of the perceiver. The vulgar conclude that no

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[34] Price, op. cit., P. 122; also Pitcher, op. cit., P.31. And see Bertrand Russell, An Inquiry into Meaning and Truth (Baltimore: Penguin, 1940), P.13.

impressions are independent of the perceiver.

It may be asked why those among the vulgar who come to see the inconsistency of (a),(b), and (c) conclude that (a) is false. Why do they not continue to believe that (a) is true, and reject (b) or (c) instead? Or even if they deny (a), why conclude that neither object is independent of the perceiver? Why not conclude instead that both objects are independent of the perceiver?[35] Apparently according to Hume, the reflective vulgar find the belief in the independent existence of impressions the most likely to be false, or the easiest to reject, of all the beliefs involved. It would be difficult to believe that two things that are exactly alike have vastly different causes, or that despite the qualitative identity of the two images in double vision, they are of entirly different natures. Similarly, it is hard to believe that both of the things before our minds in cases of double vision exist independently of us; for, e.g., we can touch only one of the two, and it certainly seems that there being two things, rather than just one, is due to the pressing of the eyeball.

Of course, it is also difficult to deny the independent existence of what is before the mind when we (as we would normally say) see a tree, for example. But the

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[35]See Ayer, Foundations of Empirical Knowledge, Chapter 1 for some suggestions along these lines.

difficulty is lessened by the availability of the Philosophical View of Body, which accommodates, to some extent, our natural tendencies to attribute continued and independent existence to objects but allows us to avoid the difficulties posed by double vision, hallucination, etc. -- i.e., we can admit that all perceptions in our minds are dependent on us and still believe in persisting, independent objects. It would seem (though I do not know what, or even whether, Hume thought about it) that the Philosophical View (PV) has the further advantage of fitting in with the individual's developing view about the mind, or self; in particular, PV allows us to say that all our impressions and ideas inhere in, and are completely dependent on, a substantial mind, or self. So we could say that just as not yet having belief in a substantial mind is required for developing the Vulgar View of Body, so already having that belief is instrumental in coming to reject that view in favor of PV. Another attractive feature of PV (and I think Hume really did have this feature in mind) is that it leaves intact our beliefs about the physical world itself, changing only beliefs about the relation between the physical world and the mind that perceives it -- i.e., instead of the objects' actually being before, or in, the mind[36] during perception, the

[36]Professor Michael Levin (in conversation with me) argued that nobody believes that trees and stones are actually in our minds when we perceive them. He suggested that "the vulgar" have no clear view about the relation between a perceived object and the perceiver, but if and when they think about it, they come to believe that objects cause us to perceive them. I think Hume could go along

objects cause impressions in the mind. And most people would rather retain their views about the physical world and change their views about our perception of it than vice-versa, since (as was said before) our views about the physical world are developed before our views about the mind and so are more fully articulated and more firmly entrenched at the time that we come to reflect on the "experiments" of double vision etc.

It might be objected that the argument that Hume attributes to the reflective ones among the vulgar (and to which he himself seems to subscribe, as an "ex-vulgar") begs the question. For Hume assumes that the objects of perception are impressions in the mind, and that the vulgar take them to be such. Is it any wonder, then, that he-- and according to him, the reflective vulgar -- conclude that the objects of perception are dependent on the mind and cannot continue to exist when no longer in the mind?

But this objection misses a number of key points in Hume's theory of the mind and perceptions, as well as in his account of the Vulgar View of Body. First of all, though we, in reading the Treatise, might assume that what Hume calls an impression is something which, if it exists at all, must exist in a mind, Hume did not make

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with this. He would add only that when the vulgar think a little more about what is entailed in objects' causing us to perceive them, they end up with PV.

this assumption. In fact, in the section entitled "Of the Immateriality of the Soul" (I:IV:5) he professes to find unintelligible the notion of impressions' inhering in any substance, whether physical or mental; and in the section on "Personal Identity" (I:IV:6), as well as in our section "Of Scepticism with Regard to the Senses" (I:IV:2, P. 207), he says that an impression's being "in" a mind consists in its having certain relations to other perceptions, and that "there is no absurdity in ... breaking off all its relations with that connected mass of perceptions" (P. 207). Secondly, an impression of sensation for Hume is not just an image or representation of a property, or quality, that may belong to a physical object; it just is that very property, or quality. Thus, e.g., Hume argues that the view that impressions of extension inhere in a simple, immaterial mind, or soul, is just as absurd as Spinoza's view that (real) extension inheres in the simple substance he calls "God." (pp. 239-243). Thirdly, a physical object for Hume, as for Locke and for Berkeley, is a collection of sensible qualities, perhaps <u>supposed</u> to be held together by some kind of substratum of which we can have no idea. (See I:I:6 and I:IV:3). Fourthly, it is not quite correct to say that Hume attributes to the vulgar the belief that what they perceive and what they think exists independently of being perceived are impressions; for, as Hume says repeatedly (in I:IV:2), the vulgar make no distinction

between impressions and perceivable objects. As for Hume's talk of what we perceive -- whether we call them impressions or objects -- being at least sometimes in, or present to, the mind, I think he is simply trying to do justice to the vulgar (or naive realist) view[37] that the mind is in direct contact with physical objects; and all that Hume means by objects' being "present to the mind" is that "they acquire such a relation to a connected heap of perceptions, as to influence them very considerably in augmenting their number by present reflexions and passions, and in storing the memory with ideas." (P. 207).

Thus Hume's calling the objects of perception "impressions" does not preclude their existing apart from the mind or their being physical objects, or properties of (i.e., constituting) physical objects. But the most important point to notice in answering the present objection is that Hume does not use any of the possible question-begging associations of the term 'impression' in his (or the reflective vulgar's) argument. Granted, Hume's statement of the argument is not very detailed; but there is no reason to read into it an assumption of the logical absurdity of impressions' existing independently of minds, for it is detailed enough to indicate how naive realism, once it reaches a certain stage of development, is inconsistent, even without any such assumptions.[38]

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[37]See preceding footnote.

[38]See Cook, op. cit. Cook claims that Hume does tacitly assume the logical absurdity of impressions' existing

So far, then, it seems that Hume's argument (on pp. 210-211) is a sound reductio ad absurdum of the vulgar view, or naive realism. But it may still be objected that Hume is begging an important question in assuming the existence of the objects of subjective experience. E.g., in the case of double vision, he assumes that there are two qualitatively identical things each of whose existence can be causally explained, and in cases where a round object looks oval, or a white object looks yellow, he assumes that there is something oval and there is something yellow whose existence can be accounted for. Without these assumptions his argument fails. For it requires the premise that something (e.g., a second pen, an oval penny, yellow snow) is dependent for its existence on the state of the perceiver; only then can the causal principle (Same effect, same cause) come into play to entail that other things (e.g., the first pen, a round penny, white snow), since they are of the same nature, must also be dependent for their existence on the perceiver. The data of the "experiments" could be reported just as (or more) properly by saying, e.g., that things appear to be double, or to come in pairs, that the penny appears to be, or looks oval, that the snow looks yellow; and of course the fact that something appears to have a certain property at a certain time does not entail that it has, or that

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apart from the mind, despite Hume's explicit denials. I think some of what I have said in the present chapter goes some way toward refuting his arguments.

there is something else that has, that property at that time.[39]

But though this way of reporting the facts of double vision, perspectival relativity, etc. may represent a different ontology from Hume's -- e.g., where Hume would say there is an impression of yellow snow, our objector would say there is white snow that looks yellow -- nevertheless Hume does not commit the fallacy of begging the question in the arguments in question, since those arguments can be stated just as cogently with either way of reporting the facts. (The argument based on double vision _might_ be an exception. For Hume seems to treat double vision as a sort of hallucination -- e.g., we see one pen, and we _hallucinate_ another just like it, since there is, we believe, no such second pen. The alternative way of reporting the facts _could_ treat double vision as an _illusion_[40] -- one pen appears to have the _property_ of being double. However, Hume's position would not be damaged, for we could simply use cases which are hallucinations on either way of reporting the facts.) E.g., suppose that while suffering from a disease we see snow ans it looks yellow to us, and that we realize that its looking yellow to us is due to the disease. Then we recover from the disease and the snow looks white. It

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[39] See, e.g., Austin, _op. cit._, P. 88, and Pitcher, _op. cit._, pp. 32ff.

[40] See Price, _op. cit._, pp. 109-112.

stands to reason that if the snow's appearing yellow to us was caused, at least in part, by the state of our sensory apparatus brought about by the disease, then the snow's appearing white to us must be caused, at least in part, by the state of our sensory apparatus brought about by the functioning of our bodies when undiseased. So it seems likely that, in general, how things appear to us is dependent on the state of our sensory apparatus -- or, as Hume put it, "on our argans and the disposition of our nerves and animal spirits." (P. 211).

But now it may be asked, if Hume's "experiments" show only that how things appear to us depends on the state of our sensory apparatus, how can Hume infer, from this seemingly innocuous result, that nothing that we perceive continues to exist when we no longer perceive it? In answer to this question, it should be noticed first of all that, despite the tone of Hume's remarks following his presentation of the "experiments" (pp. 211ff.), he does not (or at least <u>should</u> not) claim that the experiments prove that there is no external world of persisting objects; for the Philosophical View of Body (roughly, the Representative Theory of Perception) can accommodate the experiments and still posits such an external world. Hume claims only that the experiments prove that none of the impressions <u>in our minds</u> continue to exist when not in our minds. The experiments show that any impression existing in my mind at time t is dependent for its

existence at t on the state of my nervous system at t; so all indications are that when my nervous system is not in the requisite state, the impression does not exist. In terms of appearances, rather than impressions, the experiments show that the properties we tend to attribute to external, persisting objects are really just how things appear to us; and how things appear to us is dependent on the state of our nervous system. So, again, all indications are that when our nervous system is not in the requisite state, and consequently nothing appears to us to have a certain property, then nothing in fact has that property. The Vulgar View is that, though perhaps in some cases a thing's appearing to us to have a certain property is due in part to (the distorting influence of) our nervous systems, in "regular" cases of perception nothing gets between us and the external object to distort its appearance. To the extent that the nervous system is acknowledged to be involved in "regular" perception, it is thought to be a vehicle through which the properties are brought before us; it is only in unusual cases that the vehicle itself influences how things appear to us. Causal reasoning, however, based on the "experiments" (or on the unusual cases) indicates that in "regular" cases as well, how things appear to us is influenced by our sensory apparatus.[41]

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[41]Most recent writers on the argument from illusion seem to think that once we express the facts in terms of

It may be thought that Hume is left with a problem: for even if what is before our minds in sense-perception is dependent on the operation of our sensory apparatus, and therefore does not exist unperceived, there is still this sensory apparatus itself -- our eyes, ears, brain, nervous system, etc. -- which is independent of the mind and exists unperceived. So Hume's argument from the "experiments," even if it were sound, would have the odd conclusion that no physical objects exist, or that nothing exists independently of minds, except sense-organs, brains, and nervous systems -- or perhaps except human (and animal?) bodies.

But this problem can be resolved. For our belief in the continued and independent existence of our sensory apparatus either has the same basis as our belief in the continued and independent existence of other things (e.g., we can feel our eyes and ears) or else it is based on (scientific) reasoning which presupposes belief in the external world. So once we undermine belief in the external world in general, we can have no reason to believe in the existence of our sensory apparatus.

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appearances, instead of impressions or sense-data, naive realism is saved. But I think it is clear, from what I have said, that more is needed to save naive realism. Talk of how things appear, rather than of sense-data or impressions, does avoid ontological commitment to a realm of entities which duplicate and represent parts of the external world. But it does not avoid skepticism about the real nature of the external world. And furthermore PV, which does involve a realm of entities representing parts of the external world, is a quite natural way to explain what is involved in how things appear.

Hume's arguments based on the "experiments" raise a question about what he meant by 'independent existence': Of what are our impressions really not, but vulgarly believed to be, independent according to Hume? And what kind of independence is at issue -- causal, logical, or some other kind? The notion of the independent existence of the objects of the senses appears early in I:IV:2:

> We ought to examine apart those two questions, which we commonly confound together, viz. Why we attribute a continu'd existence to objects, even when they are not present to the senses, and why we suppose them to have an existence distinct from the mind and perception. Under this last head I comprehend their situation as well as relations, their external position as well as the independence of their existence and operation. (P. 188).

In the above passage independence and externality together are said to constitute the "distinct existence" which we attribute to external objects (or"body"). It is "the mind and perception" that body is supposed to be distinct from; so presumably it is the same mind and perception that body is supposed to be external to and independent of.

However, what the "experiments" show, according to Hume, is that the objects of our senses are dependent not on the mind and perception, but "on our organs, and the disposition of our nerves and animal spirits." (P.211). And yet Hume identifies the independence which the "experiments" show that perceptual objects lack with the independence that he had earlier (i.e., in the passage on P. 188, quoted above) said is an aspect, or constituent, of distinct existence. Thus he infers from the "experiments"

that "our sensible perceptions are not possest of any distinct or independent existence." (P. 211). Moreover, immediately after introducing the notions of continued, distinct , external, and independent existence, he had said that continued existence and distinct existence entail one another:

> These two questions concerning the continu'd and distinct existence of body are intimately connected together. For if the objects of our senses continue to exist, even when they are not perceiv'd, their existence is of course independent of and distinct from the perception; and vice versa, if their existence be independent of the perception and distinct from it, they must continue to exist, even tho' they be not perceiv'd. (P. 188).

And he concludes, from the lack of independence which he infers from the "experiments," that "our perceptions have no more a continu'd than an independent existence" (P. 211), after having reminded us (P. 210) of the earlier passage in which the mutual entailment of continued and independent existence had been asserted. Hume thus seems to vascillate between saying that the independence in question is independence of our mind and that it is independence of our body.

Furthermore, the dependence on our sensory apparatus that Hume infers from the "experiments" is causal dependence -- i.e., changes in our sense organs or in our nerves or "animal spirits" are uniformly followed by changes in the objects before our minds. But since, according to Hume, the objects before our minds are

perceptions, it might seem that the important issue is that of these objects' (or perceptions') logical independence of our minds or of being perceived. We might therefore interpret Hume as originally meaning by 'independence' logical independence and then switching to causal independence (because that's all he could reasonably deny, given his theory of the mind, or self) and confusing the two.[42]

However, this latter interpretation would be based more on reading into Hume our own expectations and prejudices, than on any actual vascillation or confusion on Hume's part. For Hume consistently means causal independence when he uses the expression 'independent existence' to talk about what, along with continued and external existence, we attribute to body, and for the attribution of which he wants to find the psychological causes. He does, of course, deal with the question of logical independence -- i.e., he shows that perceptions' existing apart from a mind is not logically absurd (P.207, see above). But this logical independence is, and is viewed by Hume as, a presupposition of our attributing continued existence to objects, whereas what he calls "independent existence" is, he claims, attributed to objects only after we already believe in their continued

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[42] I think this is Bennett's interpretation, in op. cit., pp. 314-315. At least he seems to interpret 'independence' as logical independence of being perceived. He apparently thinks that Hume's "experiments" are supposed to show only

existence. (See, e.g., P. 210).

As for Hume's vascillation over what body's independence is independence of, I think that it is only apparent, and that the appearance is due to Hume's carelessness (perhaps partially intentional) in stating the strategy of the section "Of Scepticism with Regard to the Senses" (on P. 188). Hume's strategy was to isolate each of the various properties which we think of as essential to body, and then to inquire "whether it be the senses, reason, or the imagination" that leads us to ascribe these properties to things, as well as which of the properties we are led to ascribe by which faculty. (P. 188). The properties which we think of as essential to body are, according to Hume, continued existence and "existence distinct from the mind and perception." And what we have in mind when we think of distinct existence are, he thinks, externality and independence. But what can be meant by "external position" relative to "the mind and perception" is at least problematic, since presumably the mind and perception are neither extended nor located in space. So it seems that, contrary to the implication given by Hume, externality at least should not be construed as relative to the body.[43] Perhaps, then, we should not put

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that perceptions lack continued existence, not independent existence. See P. 315.

[43]Bennett, op. cit., pp. 317-318, thinks that Hume meant by 'external' external to the body, but he argues

much weight on the implication that independence is supposed to be relative to the mind and perception, but should rather accept Hume's later (pp. 210-211) identification of the independence he is talking about with independence of "our organs and ... nerves and animal spirits."

I think Hume was intentionally vague about what he meant by 'continued, distinct, independent, and external existence' at the beginning of the section, because he did not want to beg any questions at the outset as to which we come to believe in first. He simply listed several properties which different people, at various stages of intellectual or imaginative development, attribute to body; and he planned to inquire which of these properties we come to attribute to body before which others, and how we come to attribute them. Upon examination, it turns out that

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that we could make sense of externality to the mind and perception "based on my perceptual slant on the world, i.e., on the fact that at any given time I perceive the objective realm from a particular point of view." (P. 318). However, Hume could not use the notion of "point of view," or "slant on the world," since he accepts the view of Berkeley (see Berkeley's Essay Towards a New Theory of Vision) that "our sight informs us not of distance or outness (so to speak) immediately and without a certain reasoning and experience," (Treatise, P. 191). Presumably for Hume, as for Berkeley, we infer the distance of objects from us only after we have acquired belief in a persisting external world accessible to touch as well as to sight.

our attribution of continued existence to objects must precede our attributing independent and external existence to them. Sometimes Hume talks as if this precedence is merely a matter of contingent fact. But other times he tells us that we must first ascribe "a real and corporeal existence" to our own bodies before we can ascribe external existence to things, since externality is relative to our bodies. (P. 191). And if we construe 'independence' univocally as independence of our sensory apparatus, then obviously we must first believe in the continued existence of the latter. It also turns out, according to Hume, that belief in a double existence of objects and impressions comes only after we have already accepted (and, to a certain extent, cast doubt upon) the continued, independent, and external existence of what is before our minds in sense-perception.

V

What emerges from the foregoing discussions is a defense of the Philosophical View of Body (PV) as a theory explaining the data of our perceptions. First of all, we get a pretty clear picture of the data. The pre-theoretic[44] view of the world is an extreme form of solipsism, according to which nothing exists except a chaotic stream of perceptions. Both the Vulgar View of Body and PV impose some order on this stream of perceptions, and therefore each has some claim to be accepted as true. However, the Vulgar View, once it reaches a certain level of development, or sophistication, is inconsistent; and the most convenient way to iron out the inconsistency is simply to shift to PV.

If we follow Hume in rejecting the notion of spiritual, or mental, substance, we have the view that the mind is a bundle of impressions and ideas (Part III). And if we follow him in rejecting the notion of material, or physical, substance, then we have the view that a physical object is a bundle of qualities of certain kinds -- e.g., colors, shapes, textures (See Part IV, P.83). Furthermore, the general tendency of his statements about the nature of impressions and ideas leads to the view that impressions

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[44] That is, pre-external-world-theoretic: what Hume takes as given may itself be "theory-laden," but that is not our concern right now.

of sensation are themselves qualities of the same sort that could constitute physical objects (See Part IV, P.83).[45] What makes such a quality a constituent of a mind is its influencing in a certain way the other perceptions -- the thoughts (or ideas), beliefs, desires, etc. -- that, together with it, constitute a mind. (See Part III, P. 72). What would make such a quality a part of the physical world would be its existing in causal independence of its influencing, or being in, a mind, as well as its existing when not in a mind, and its having certain causal and/or spatio-temporal relations to other such qualities.

The question is, what reason is there to believe that any quality is part of the physical world -- i.e., that it exists apart from the mind? Hume says there is no reason. First of all, that no quality in a mind is also part of the physical world (i.e., that the Vulgar View is false) is shown by experience plus the principle that like effects have like causes (Part IV). Hume's argument that we have no reason to believe (the Philosophical View) that there are extra-mental qualities constituting a physical world and giving rise to impressions in our minds is that the only possible reason would be a causal

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[45]Thus we are interpreting Hume to be a neutral monist. See Bertrand Russell, The Analysis of Mind (New York: Humanities Press, 1921) and Mysticism and Logic (Garden City: Anchor Doubleday, 1917).

inference, which would require that we observe such extra-mental qualities conjoined with perceptions in our minds, and since all we ever observe (at least in the relevant sense of 'observe') are the perceptions in our minds, we cannot make the required inference. (See above, Chapter 2). But we can reject Hume's argument if we deny that the only way to infer the existence of something x is through a causal inference based on having observed things of the same sort as x constantly conjoined with things of another sort. And it does seem that something like the hypothetico-deductive method, or inference to the simplest hypothesis, or to the best explanation, can give us reason to believe that something exists of a sort that we have not observed to be constantly conjoined with another sort of thing.

It seems that we would have reason to posit the existence of something if by so doing we could effect a greater subsumption of phenomena under universal laws. As we have seen, the view that there is a physical world which we perceive piecemeal (either directly by having in our minds some of the qualities that constitute the physical world, or indirectly by having in our minds impressions caused by and representing those qualities) and only under certain conditions allows us to establish universal causal laws, constant conjunctions, which could not be established otherwise. (See Part II). By

means of these universal laws, we can explain the constancy and coherence of our impressions: Cases of coherence become instances of universal causal laws connecting qualities in the physical world, the lack of constant conjunction of the relevant impressions in our mind being explained by means of the general laws (or rules of thumb) about the conditions under which we can perceive qualities in the physical world. The qualitative identity of impressions in cases of constancy is explained by their numerical identity (according to the Vulgar View) or by the numerical identity of the external objects perceived (according to PV); and the numerical identity (in either case) can be explained via laws stating how long groups of qualities in the external world tend to remain unchanged under various conditions. Further aspects of our perceptions that are explained by the hypothesis, or theory, of the external world include the following: We get impressions of sensation when and only when our sense-organs are functioning properly and the external objects can affect them. Pleasure and pain accompany some impressions, but not the corresponding ideas. Impressions are generally livelier, more vivid, than ideas.[46] (In fact, it has been argued that Hume's distinction between impressions and ideas on the basis of degrees of liveliness does not make sense unless we assume the truth of PV.[47])

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[46]See Locke, Essay IV:xi:4-7.

[47]See Mandelbaum, op. cit., pp. 156-170.

Predictions as to what impressions we will have in our minds at given times, based on the laws governing external objects and our perception of them, generally turn out to be true. And when they do not, there is usually an indication of how we can explain this failure within the framework of these laws. Explaining the discrepancies between prediction and observation often leads to refinements in the laws, and sometimes even to replacement of old laws by new ones. But these refinements and replacements do not alter the fact that the hypothesis of the external world, whether the vulgar or the philosophical, and at any given stage of development, is to be accepted over the solipsistic view that all that exist are the perceptions in our minds while they are in our minds.

As for the question of the relative merits of PV and the Vulgar View, or naive realism, it should be noted that Hume's "experiments" do not strictly (logically) entail the falsity of the latter. For, as was said above (Part IV, P. 80), naive realism could accommodate the experiments -- i.e., the cases of double vision, perspectival relativity, and generally the variations in how things appear due to changes in our sensory apparatus -- by simply denying that cases of normal, or veridical, sense-perception depend on our sensory apparatus in the same way as cases of illusory, or non-veridical perception.

However, it seems that a theory of perception should account for the basic similarity between veridical and non-veridical perception. PV does this quite simply: the proximate cause of our impressions, whether veridical or non-veridical, is always the state of our sensory apparatus; the difference between veridical and non-veridical perception lies in the external cause of the sensory apparatus' being in the given state. It is hard to see how the Vulgar View could account for the similarity without turning into PV -- which is, after all, what Hume was claiming in his argument from the "experiments."

CHAPTER 4

THE CONCEIVABILITY OF CHANGELESS PERSISTENCE THROUGH TIME

We have said that according to Hume, while the view that external objects are specifically different from perceptions is unintelligible, both the Vulgar View (V) and the Philosophical View (PV) of the external world are intelligible, though V is false and PV is unfounded. (See above, Chapter 2) However, it seems that insofar as V and PV involve saying that something persists unchanging through a period of time, they too are unintelligible according to Hume. That both PV (at least as normally interpreted)[1] and V do involve saying that things persist unchanging through time is obvious; for the constancy exhibited by our impressions is explained in V by the unchanging persistence of certain perceptions through

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[1]PV might avoid the charge of unintelligibility on this count, since, as I will argue in the next chapter, we need not construe it as involving time at all. However, as I will argue in chapter 6, PV would be unintelligible according to Hume on other counts were it not for the fact that it has a model, PVn, which is intelligible and in terms of which we can imagine (or picture) what is asserted in PV. And since PVn does involve things' (i.e., qualities') persisting unchanged through time, it would seem that PVn itself is really unintelligible, and therefore so is PV. Furthermore, any way of picturing (and thereby rendering intelligible) PV which does not involve time, even if we could find one, would take PV too far from our ordinary conception of the external world for its defense to be properly called "a defense of our belief in the external world" (some people would say we've already gone too far).

periods of time when they are not (as well as when they are) inour minds, and in PV by the unchanging persistence through time of extra-mental perceptions which from time to time give rise to impressions in our minds. That the notion of unchanging persistence through time is unintelligible is one of the main points argued for in I:II:3-5 of the Treatise; Hume there argues that it follows from the fact that the idea of time is derived from having a succession of different perceptions:

> I know there are some who pretend that the idea of duration is applicable in a proper sense to objects which are perfectly unchangeable [i.e., unchanging]; and this I take to be the common opinion of philosophers as well as of the vulgar. But to be convinc'd of its falsehood we need but reflect on the foregoing conclusion, that the idea of duration is always deriv'd from a succession of changeable objects, and can never be conveyed to the mind by anything stedfast and unchangeable. For it inevitably follows from thence that since the idea of duration cannot be deriv'd from such an object, it can never in any propriety or exactness be apply'd to it, nor can any thing unchangeable be ever said to have duration. Ideas always represent the objects or impressions from which they are deriv'd, and can never without a fiction represent or be apply'd to any other. By what fiction we apply the idea of time even to what is unchangeable, and suppose, as is common, that duration is a measure of rest as well as of motion, we shall consider afterwards(in Sect. 5). (P. 37).

And in I:IV:2, in discussing the notion of identity -- which according to Hume presupposes unchanging persistence through time -- as it applies to V, he says:

> I have already observed (Part II, Sect. 5) that time, in a strict sense, implies succession, and that when we apply its idea to any unchangeable [i.e., unchanging] object, 'tis only by a fiction of the imagination, by which the unchangeable object is suppos'd to participate of the changes of the co-existent objects, and in particular of that of our perceptions. (pp. 200-201).

Thus Hume claims that we have no idea of changeless persistence through time, but we think we do because of a "fiction of the imagination."

I believe the solution to our problem of rendering V and PV intelligible according to Hume lies simply in examining in detail exactly what Hume says about the idea of changeless persistence through time. For it will be found that, though he denies the existence of this idea, he is willing to allow ideas that can serve all the purposes it was supposed to serve, at least as far as V and PV are concerned.

Hume's denial of our having an idea of changeless persistence through time is primarily in I:II:3 and 5 of the Treatise. In Section 3 he gives his definitions of 'space' and 'time' (or rather his descriptions of the essential features of our ideas of space and time)and claims that a consequence of these definitions is that we can have an idea neither of a vacuum, or empty space, nor of something remaining unchanged over a period of time. In Section 5 he defends this consequence against objections and thereby clarifies (and to a certain extent, qualifies) his own position. However, Section 5 consists almost exclusively of a discussion of these objections as they apply to space, with only a couple of paragraphs at the end devoted specifically to time.

It is suggested throughout Part II that space and time are analogous, and that therefore what is said about a vacuum applies, mutatis mutandis, to unchanging persistence through time. Let us therefore examine Hume's account of space and a vacuum, and then try to apply what he says to time and unchanging persistence through time. But first the definitions.

Before defining 'space' and 'time' Hume argues in I:II:1 that no idea or impression is infinitely divisible; it would follow that any idea of an object extended in space, since it is divisible, must be only finitely divisible -- i.e., composed of discrete, indivisible points. Having established this, at least to his own satisfaction[2], he proceeds to trace our idea of space back to the original impression from which it was derived, and concludes that our idea of space is an abstract idea (see I:I:7) of an array of colored or solid points. That is, we have in our minds an idea of some particular extended thing -- i.e., some particular array of colored or solid points -- with an indefinite number of other ideas of particular arrays of colored or solid points ready to come into the mind should we

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[2] I do not want to discuss here his arguments against infinite divisibility. But see Antony Flew, "Infinite Divisibility in Hume's Treatise," in Hume: A Reevaluation, ed. Donald W. Livingston and James T. King (New York: Fordham University Press, 1976), for an extremely negative appraisal, with which I do not completely agree.

be tempted to mistakenly ascribe to all arrays of colored or solid points some property of the particular idea in our minds. Space, or extension, itself may be considered, through a distinction of reason (see I:I:7, pp. 24-25)[3], as "that disposition of points, or manner of appearance in which they [i.e., all the ideas of arrays of colored or solid points] agree." (P. 34).

It follows that any idea which is not of an array of colored or solid points is not an idea of space, or extension. So if an idea of a vacuum, or of empty space, is supposed to be an idea of space but without anything visible or tangible, then there is no such idea.

The idea of time, like that of space, and for basically the same reasons, cannot be infinitely divisible, and therefore must be composed of indivisible points, or moments. And as each point of an extension is filled by color or solidity, so each moment of a period of time is filled by a perception -- any perception. A period of time is just a succession of different perceptions. The idea of time in general is, of course, an abstract or general idea: We have in our minds an idea of a particular succession of perceptions, with an indefinite number of other ideas of particular successions of perceptions ready to come into our minds should we be

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[3]Hume does not use the term 'distinction of reason' in this connection. But I think that his calling space the "manner of appearance" of any array of colored or

tempted to mistakenly ascribe to all successions of perceptions some property of the particular idea in our minds. Time itself may be considered, through a distinction of reason[3], as "the manner in which impressions [and ideas] appear to the mind." (P. 36; see also pp. 34-37.)

It follows that any idea which is not of a succession of different perceptions is not an idea of time. So if an idea of changeless persistence through time is supposed to be an idea of a period of time but without a succession of different perceptions (but rather with just one perception), then there is no such idea.

In I:II:5 Hume considers the following three objections to his denial of the conceivability of a vacuum: (1) The fact that people dispute whether a vacuum actually exists proves that we have an idea, or can conceive, of a vacuum; for otherwise the disputants would have nothing to dispute about. (2) We can imagine (i.e., have an idea of) a clump of motionless matter (e.g., a room in which nothing moves); and we can imagine part of that matter (e.g., everything within the four walls of the room) to be "annihilated" -- i.e., to cease to exist -- while the remaining matter (the four walls)

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solid points is consistent with his first principle if it is understood in terms of this concept, contrary to what Norman Kemp Smith says in The Philosophy of David Hume, pp. 274ff. Similarly for Hume's calling time "the manner in which impressions appear to the mind."

remains motionless. Where the annihilated matter was (the distance separating the walls) is now a vacuum. Thus we can imagine (i.e., have an idea of) a vacuum. (3) We can conceive of motion. But motion is impossible without a vacuum; for one body cannot move into a place occupied by a second body unless the second body moves fromthat place into another; and if the latter place is occupied by a third body, then that body must move into yet another place; etc. ad. infinitum, unless there is a place unoccupied by any body -- i.e., a vacuum. If the idea of motion involves the idea of an infinite number of motions, then that idea is impossible. Therefore we must have an idea of a vacuum. (pp. 54-55).[4]

Hume answers all three objections by describing a type of idea which we do have and which we tend to mistake for an idea of a vacuum (hence an idea of this type may be called a "pseudo-vacuum idea"). E.g., imagine two colored points at a (non-zero) distance from one another, but with nothing between them except utter darkness. Hume argues that the idea of the invisible distance between the two colored points is not an idea of a vacuum, for

> the idea of darkness is no positive idea, but merely the negation of light, or more properly speaking, of colour'd and visible objects. A man who enjoys his sight receives no other perception from turning his eyes on every side when entirely deprived of light than what is common to him with one born blind; and 'tis certain such-a-one has no idea either of light or darkness. (pp. 55-56).

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[4] I have taken the liberty of "enlarging upon" the third objection beyond what Hume says. See P. 55 of the Treatise.

Thus to see or imagine total darkness is, in itself, not to have any impression or idea at all, and consequently not to have an idea of a vacuum. And

> when two bodies present themselves, where there was formerly an entire darkness, the only change that is discoverable is in the appearance of these two objects, and ... all the rest continues to be as before, a perfect negation of light, and of every colour'd or visible object. (P. 57).

That is, an invisible distance between two colored points is still "no positive idea," just as if the two colored points were not there. The pseudo-vacuum idea is just an idea of two points related (spatially) to each other in a certain way, and nothing more; in particular, there is no additional idea (or part of the idea) which could properly be called an idea of empty space, or a vacuum.

Now the invisible distance between the two points in our pseudo-vacuum idea is certainly empty, but it is not space. However, we tend to confuse it with a distance, in another idea, which is space, but is not empty: Imagine two colored points exactly like the ones imagined before, the same distance apart from each other, but this time with the distance filled by other colored points. This idea is related to the former in three ways. First, the two pairs of colored points look the same, both in themselves and with respect to the distance between the members of each pair. "Secondly, we find that ... an invisible and intangible distance may be converted into a visible and tangible one, without any

change in the distant objects." (P. 59). That is, we have often had an impression of two objects separated by an invisible distance (as in the pseudo-vacuum idea) followed by an impression of the same two objects, as it were, separated by the same distance, but with the distance filled by visible objects (as in the second idea imagined above). Thirdly, we find by experience that the diminution of "all qualities, such as heat, cold, light, attraction, etc." over distance is the same whether the distance is visible (as in the second idea above) or not (as in the first). (pp. 58-59). Thus,

> As the first species of distance is found to be convertible into the second, 'tis in this respect a kind of cause; and the similarity of their manner of affecting the senses [i.e., the similarity in appearance between the two pairs of points], and diminishing every quality, forms the relation of resemblance. (P. 62).

Because of the relations of resemblance and causation between these two ideas, having one of them before the mind often leads us to have the other also before the mind. Furthermore,

> Resembling ideas are not only related together, but the actions of the mind which we employ in considering them are so little different, that we are not able to distinguish them.... and we may in general observe that wherever the actions of the mind in forming any two ideas are the same or resembling, we are very apt to confound these ideas, and take the one for the other. But tho' resemblance be the relation which most readily produces a mistake in ideas, yet the others of causation and contiguity may also concur in the same influence. (P. 61).

Thus we confuse the idea of an invisible distance -- i.e.,

of a distance which is empty, and is therefore not an extension, or space, -- with the idea of a visible distance -- i.e., of an extension, or space, which is not empty. As a result of this confusion we come to think that the former idea is an idea of empty space, or a vacuum.

Hume thus answers the first objection by explaining how we come to think we have an idea of a vacuum, and therefore can dispute whether a vacuum really exists, even though we have no such idea. The second objection is answered simply by saying that the idea of the invisible distance between, e.g., the walls of a room all the contents of which have been annihilated is just that sort of pseudo-vacuum idea which we tend to mistake for an idea of empty space; and therefore the second objection is just an example of this kind of mistake. As for the third objection, we simply find <u>by experience</u> that an object can move into an invisible and intangible distance (which, again, is not a vacuum) between two other objects without displacing either of those objects or any other. (pp. 62-63).

(We have stated Hume's argument using only visual perceptions as examples of pseudo-vacuum ideas and of the ideas with which they are confused. But, as is evident from some of the quotations from Hume, we could just as well have used tactual perceptions. For we can have an idea of two solid objects separated by a distance

through which our hand can move freely since no other solid, or tangible, objects are there; and this idea is related, by resemblance and causality, to the idea of the same two objects separated by a distance filled with other tangible objects, thus making us apt to confuse the two ideas and then mistake the former for an idea of a vacuum.)

But given all that Hume does with the idea of an invisible and intangible distance, one is tempted to object: "Why is this idea not an idea of empty space? What more can be required of an idea for it to be one of empty space that this idea lacks? If this is not an idea of a vacuum, then I don't know what would be." Hume's answer is:

> If you are pleased to give to the invisible and intangible distance, or in other words, to the capacity of becoming a visible and tangible distance, the name of a vacuum, extension and matter are the same, and yet there is a vacuum. If you will not give it that name, motion is possible in a plenum, without any impulse <u>in infinitum</u>, without returning in a circle, and without penetration. But however we may express ourselves, we must always confess that we have no idea of any real extension without filling it with sensible objects and conceiving its parts as visible or tangible. (P. 64).

That is, whether we decide to call the idea of an invisible, intangible distance "the idea of a vacuum" does not matter as long as we remember that such an idea is not an idea of a "real extension" -- presumably because it is "no positive idea" at all. As for our not knowing what a positive idea of a real vacuum would have to be, Hume would say that that is exactly what he has been saying --

i.e., we have no idea of a vacuum.

It seems that Hume's position that the pseudo-vacuum idea is not an idea of empty space depends entirely on his claim that the idea of darkness is "no positive idea." But though Hume's equation of a visual field of total darkness with the total absence of visual perceptions may be plausible (and even here I would have qualms[5]), his equating the darkness between visible points in a perception with nothing (i.e., with no visible points, no perception) is not plausible. For it seems to me that the closest I can come to imagining two points not touching, yet separated by nothing is imagining two brightly colored points separated by an array of black (and therefore colored and visible) points. I am not quite so clear about tactual perceptions of intangible distances. But I am inclined to say that here too the perception of a distance separating two solid objects between which the moving hand encounters no resistance (see P.58) is to be equated not with the absence of all tactual perceptions, but only with the absence of tactual perceptions of a certain kind (i.e., perceptions of resistance to motion), and that the perception of unimpeded motion is enough to make the distance tangible -- i.e., we discover the distance and its size through

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[5]See Flew, op. cit., P. 267.

the impressions gotten when we move our hand through it -- and so we can get the idea of space from it. Of course, this is no objection to Hume's position that we have no idea of space not filled with anything visible or tangible; on the contrary, I think Hume need not even have admitted that we have an idea of an invisible and intangible distance, an admission which necessitated his appeal to the doubtful (or at least disputable) claim that such an idea is "no positive idea" in order to maintain that position. He could have dealt with at least the second and third objections in the same way using 'black, non-solid (or non-resisting) distance' instead of 'invisible, intangible distance' throughout. As for his answer to the first objection, we would have the complication that there is no <u>empty</u> non-space to confuse with a non-empty space thereby to come to mistake the former for a vacuum. But I think we could save Hume's account of our coming to think we have an idea of a vacuum by adding to his account (with 'black' substituted for 'invisible') that we first mistake a black, non-solid distance for an intangible, invisible distance. (This last seems plausible, at least within the Humean framework; besides, Hume himself apparently made the mistake.)

Perhaps Hume, in admitting an idea of an invisible, intangible distance, was confusing perception- (or sense-data-) description with realist (or physicalist) description. (Hume speaks like a Lockean realist throughout

his discussion of space and time.) For if I have impressions or ideas of two colored points separated by darkness and of two solid objects separated by a distance through which I feel my hand move freely, I would normally (i.e., in realist language) describe what I see and feel or imagine or remember as two things (i.e., physical objects) separated by nothing (i.e., a distance not occupied by any physical objects). But Hume is supposed to be talking about perceptions -- i.e., about an idea of a vacuum. And in perception-language I would have to describe it as two points of a certain color or colors separated by an array of points of a different color (i.e., black), and two solid (i.e., offering resistance to the motion of my hand) objects separated by a distance in which the sensations of my hand moving are unaccompanied by any sensations of resistance.

Let us now leave Hume's discussion of empty space and turn to his parallel, though much more succinct, discussion of changeless persistence through time (CPT):

> As to the doctrine that time is nothing but the manner in which some real objects exist; we may observe, that 'tis liable to the same objections as the similar doctrine with regard to extension. If it be a sufficient proof that we have the idea of a vacuum because we dispute and reason concerning it; we must for the same reason have the idea of time without any changeable existence; since there is no subject of dispute more frequent and common. (pp. 64-65).

The first thing to be clarified about this passage, and about Hume's position in general, is exactly what he is

claiming we have no idea of. For in the first sentence of this passage he seems to indicate that his claim, or "doctrine," is that we have no idea of a time in which no "real objects" exist; while in the second sentence he seems to say that he denies only the idea of a "time without any changeable existence [i.e., changing existent]." Obviously these are two different claims; for though the second entails the first, the first does not entail the second -- i.e., we may have no idea of a time when nothing exists, and yet have an idea of a time when nothing is changing. But neither of these two claims seriously affects the conceivability of PV or V, because it would be simple and natural to stipulate that there has always been something (i.e., some perception) in existence and changing -- e.g., the sun moving across the sky, the earth turning, the stars twinkling, nebulae exploding into existence.

However, in other passages (e.g., the first two quoted in this chapter) Hume says that we have no idea of anything's remaining unchanged through a period of time, since passage of time, or duration, is not "applicable" to an unchanging object.[6] I indicated at the beginning of this chapter both that Hume argues that this third and strongest claim -- i.e., that something's persisting

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[6] I have been taking it as obvious that by 'unchangeable' Hume means unchanging, and by 'changeable' changing. See Price, op. cit., P. 45.

without changing over a period of time is inconceivable -- follows from the nature of the idea of time, and that this very sort of unchanging persistence through time is persupposed in both V and PV. I will argue later that CPT (i.e., changeless persistence through time), in the sense in which it is presupposed by V and PV, does not really follow from the nature, according to Hume, of the idea of time. But for now, back to our quoted passage.

Applying to time the third objection to Hume's denial of the conceivability of empty space -- i.e., that motion is impossible without a vacuum -- is at least problematic; for what familiar (or at least authenticated) phenomenon, analogous to motion in space over time, is even prima facie impossible without something's persisting unchanging through time? So perhaps we should not take literally his saying that his denial of the conceivability of CPT is "liable to the same objections." However, the first objection -- i.e., since we dispute and reason about x, we must have an idea of x -- does apply, and Hume applies it in the above passage (though I am unaware that the question of time without change, in the sense relevant to the conceivability of V and PV, is so "frequent and common" a "subject of dispute"). In answer to this objection Hume first (rhetorically) challenges the objector to tell from what impression the supposed idea is derived; then he describes the sort

of idea which we mistake for an idea of CPT, thereby answering also the second objection, which was basically a supposed example of the sort of idea the existence of which Hume denies.

Hume's description of the idea we mistake for one of CPT and how we make the mistake is as follows:

> we may observe that there is a continual succession of perceptions in our mind; so that the idea of time being ever present with us, when we consider a stedfast object at 5 o'clock, and regard the same at 6; we are apt to apply to it that idea in the same manner as if every moment were distinguished by a different position, or an alteration of the object. The first and second appearances of the object, being compared with the succession of our perceptions, seem equally removed as if the object had really changed. To which we may add, what experience shows us, that the object was susceptible of such a number of changes betwixt these appearances; as also that the unchangeable or rather fictitious duration has the same effect upon every quality, by encreasing or diminishing it, as that succession which is obvious to the senses. From these three relations we are apt to confound our ideas, and imagine we can form the idea of a time or duration, without any change or succession. (P. 65).

The idea in question is apparently of two appearances of one object, one at five o'clock, the other at six, where the appearance of the object at six is exactly like its appearance at five; and there is no appearance of the object between five and six. In terms of perceptions, the idea is of two exactly resembling impressions, one accompanied by an impression of a clock's striking (or otherwise indicating) five, the second accompanied by one of a clock's indicating six, with a lot of other perceptions, but no impressions resembling the two above-

mentioned, in between. Hume's description of the two resembling impressions as "the first and second appearances" of "the same," "stedfast object" must therefore be interpreted as a use of Lockean realist language. The question is: to what extent is our mistaking this idea for one of CPT dependent on this realist description? I.e., would we make the mistake if we did not already believe in the continued existence of external objects when not present to the senses? Perhaps the best way to find the answer to this question is to examine the way in which, Hume claims, we come to make the mistake, and to compare this with the way in which we come to mistake the idea of an "invisible and intangible" distance with an idea of empty space.

First of all, from the point of view of a perception-ontology, the idea we mistake for that of CPT is analogous to the idea which we mistake for that of empty space. For the pseudo-vacuum idea is one of an array of colored points surrounding an area of darkness in which, according to Hume, there are no colored points; and the pseudo-CPT idea is one of contemporaneous successions of perceptions, in one of which there is a part in which there are no perceptions. Both can be represented by the following diagram (if we ignore the spaces between the marks):

```
        ..... sounds
thoughts !!!!!!!
        xx    xx visual impressions of a (changing) landscape
  smells --------
        ******** kinaesthetic sensations
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In representing the pseudo-vacuum idea, all the dots, dashes, asterisks, etc. stand for colored (or tangible) points. In representing the pseudo-CPT idea, each row stands for a succession of perceptions of a certain kind -- e.g., the top row stands for a succession of sounds, the next row for a succession of thoughts, the bottom row for a succession of impressions of bodily movement, the next-to-bottom row for a series of different smells, and the middle row for a succession of impressions of a (changing) landscape, interrupted by a closing of one's eyes (i.e., let the third and sixth asterisks in the bottom row represent the closing and opening respectively of the eyes.) Secondly, the idea with which the pseudo-CPT idea is confused is analogous (or parallel) to the idea with which the pseudo-vacuum idea is confused -- either can be represented by the above diagram if we fill in the middle row with x's. And finally, the respects in which the confused ideas are related to each other are the same: the perceptions represented by the second and third x's in the above diagram look the same as would similar perceptions with a variegated series of others in between, and experience shows that impressions of one type often succeed those of the other and that the relations between the qualities of the perceptions represented by the x's are pretty much the same whether there are other x-impressions in between or not. Therefore, the same kind of confusion

is just as apt to occur in either case.

But what is the result of the confusion in the CPT case? What idea do we think we have? Is it an idea of a period of time during which nothing happens (i.e., during which there are no perceptions)? No, because there are series of perceptions even in the pseudo-CPT idea. Is it an idea of a period of time in which nothing changes (i.e., in which there is no succession of different perceptions)? No, because there are successions of different perceptions even in the pseudo-CPT idea. Is it an idea of one unchanging object persisting through a period of time? No, because there is no one object, changing or unchanging, in either the pseudo-CPT idea or in the idea with which it is confused; for in the former there is a group of successions of different, variegated perceptions, one member of the group being interrupted, and in the latter there is the same thing only without the interruption. Thus we would not come to disagree with Hume on any of the three possible interpretations I stated above (pp. 115-117) of his denial of the conceivability of CPT. The only result would be, again on analogy with the case of empty space, that we would think we have an idea of a period of time in which there are no perceptions of a certain kind (e.g., visual impressions, or impressions of arrays of colored points), though there must be perceptions of other kinds (e.g., impressions of reflection). But does Hume deny that we

have such an idea? I don't think so. And if I'm right, there is a lack of analogy between what Hume says is inconceivable with regard to space and what he says is inconceivable with regard to time.

Thus if we stick to a strict perception-ontology, Hume's account of our confusions does not accomplish what he wanted it to. But if we allow a realist interpretation (along the lines of V or PV) of this account, then it does the job. For then the pseudo-CPT idea is an idea of one object's appearing to us, and then the same object's appearing to us again unchanged after a succession of perceptions of different senses, or of reflection; and the idea with which it is confused is one of an object's constantly appearing to us and constantly changing until, after a series of changes, it again looks (or feels, etc.) as it did before. In the former the object is unchanged, but (presumably) time is not "applicable" to it; and in the latter the object does exist through a period of time, but it is constantly changing throughout that period. The two ideas are confused for reasons analogous to those in the case of the pseudo-vacuum idea (and the case of the ideas in our perception-ontology-interpretation of Hume's account of CPT above), and the result is that we think we have an idea of an object's persisting unchanged through a period of time.

But then our belief that we have an idea of CPT presupposes a belief in the existence of an external world (i.e., continued and distinct existence of perceptions when not present to the senses). Now I am not one of those who complain when Hume in the Treatise utilizes a belief which he elsewhere in the Treatise says is unjustified or false (see above, Chapter 3, especially Part III). For I realize that Hume generally admits that we all have the belief in question, though unjustifiedly, and he tries to explain why we have it; and our already having this belief may be a (causally) necessary condition of our having some other belief according to Hume's account of how we come to the latter. However, there is a special problem here in that whereas his account in I:II:5 of our coming to believe that we have an idea of CPT implies our already believing V or PV, his account in I:IV:2 of our coming to believe V or PV seems to imply our already believing that we have an idea of CPT. For on the latter account, we confuse an idea of resembling, non-consecutive impressions with an idea which we antecedently took to be one of CPT, and thereby come, via some further processes of the imagination, to believe that perceptions continue to exist even when not present to the senses (see pp. 199-209, especially pp. 200-201); and on the former account, we come to mistake the pseudo-CPT idea for an honest-to-goodness idea of CPT only after we believe in such

continued existence. Both of these accounts cannot be correct.

But perhaps there is only the appearance of circularity here. For, to be precise, what is presupposed in Hume's account of our adopting V and PV is belief not in CPT, but rather in identity; and though identity involves CPT, it does not necessarily presuppose the account of our belief in CPT in I:II:5. Our thinking that we have an idea of identity is, like our thinking that we have an idea of CPT, the result of confusion. (See pp. 200-201.)[7] We have an idea (what we might call a "pseudo-identity idea") of one perception "co-existent" (which I guess means co-temporaneous) with a succession of other perceptions. The succession of perceptions constitutes a period of time; but the idea of time is not "applicable" to the one perception co-existent with the succession. However, by a "fiction of the imagination," we apply the passage of time involved in the succession of perceptions to the one perception co-existent with it, and thereby come to think that that perception persists unchanging through a period of time; we then say that the perception at one moment of that period is identical with itself at another moment of it. Now Hume nowhere explains why "this fiction of the imagination [whereby we apply passage of time to the single perception] almost

[7]See also Price, op. cit., pp. 39-41.

universally takes place." If it is because we assimilate the pseudo-identity idea to the pseudo-CPT idea which we have antecedently mistaken for an idea of CPT, then the accounts in I:II:5 and I:IV2 are (viciously) circular. On the other hand, if it does not depend on our antecedently having mistaken pseudo-CPT for CPT, then there is no circularity; but we are left with two questions: First, why do we "almost universally" mistake pseudo-identity for CPT and identity? Secondly, if we originally came to think we have an idea of CPT through an idea of pseudo-identity and a process that does not presuppose acceptance of V or PV, then why did Hume not say so in I:II:5? Why did he instead account for our belief in CPT in a way that entailed antecedent acceptance of V or PV?

In answer to the first question, I think we can construct a fairly plausible explanation along the lines of Hume's explanation of our mistaking a pseudo-vacuum for a vacuum: Suppose one perception P is co-existenct with a succession of perception $P_1P_2...P_{10}$; and suppose we consider P_1 together with the co-existent P and also P_{10} together with the co-existent P. Now imagine two co-existent successions of perceptions, one of which is $P_1...P_{10}$ as before, the other $P_aP_b...P_j$, where P_1 and P_a are co-existent, P_2 and P_b are co-existent, etc., and where P_a and P_j are qualitatively identical with each other and with P. The idea of P_1 together with P_a

and P_{10} together with P_j resembles the idea of P_1 with P and P_{10} with P; for the perceptions co-existent with P_1 in the two ideas are qualitatively identical, as are the perceptions co-existent with P_{10} in the two ideas. Furthermore, experience tells us that instead of P co-existing with $P_1 \ldots P_{10}$, $P_1 \ldots P_{10}$ could have had $P_a P_b \ldots P_j$ co-existent with it (i.e., we have had experience of an impression co-existent with a succession of perceptions followed by a succession of impressions the first and last of which resemble the original impression, and which is of the same duration as the succession with which the original impression was co-existent -- or at least our experience indicates that such a series of perceptions is possible). And finally, experience tells us that the increase or diminution of any quality may be the same whether it is P or $P_a \ldots P_j$ that is co-existent with $P_1 \ldots P_{10}$. (I have my doubts about this last point, but I am at least as sure of its truth as I am of that of Hume's analogous points with regard to a pseudo-vacuum and pseudo-CPT -- see above, P. 109 and P. 110.) Therefore we confuse the idea of P co-existent with $P_1 \ldots P_{10}$ and the idea of $P_a \ldots P_j$. The former is an idea of one perception which is unchanging, but not through time; the latter is an idea of a period of time, but there is no one unchanging perception. So as a result of the confusion of the two, we come to mistake the former for an idea

of one perception remaining unchanged through a period of time (i.e., for an idea of CPT). We then (mistakenly) think of P as consisting of temporal parts, like the succession $P_a \ldots P_j$; but since we know that P is just one perception, we say that one temporal part of P is identical with another.

In answer to the second question, the best I can do is to suggest that perhaps Hume believed that people who claim to have an idea of CPT really have in mind the idea of pseudo-CPT, rather than the idea of pseudo-identity, and that their mistake is in fact the mistake, dependent on antecedent acceptance of V or PV, described in I:II:5. And perhaps he wanted to describe what actually goes on in those people's minds at the time they make their claim, even though we all originally came to believe that we have an idea of CPT through incorrectly applying the idea of time to the "unchangeable object" in our pseudo-identity idea.

But, as in the case of our "mistaking" a pseudo-vacuum for a vacuum, we may ask, "Why isn't the psuedo-identity idea an idea of CPT and identity?" Hume's answer would probably be:

> If you are pleased to give to a perception co-existent with a succession of perceptions the name of an object persisting unchanging through a period of time, then duration and change are the same and yet there is something that persists unchanged through time. If you will not give it that name, then one object may be co-existent with a succession of others and yet not endure, or persist through time. But

> however we may express ourselves, we must confess that we have no idea of a changeless object persisting through a real period of time. (An adaptation of the quotation on P. 112 above, from P. 64 of the Treatise.)

Whether we call such an idea one of CPT doesn't matter as long as we realize that the persistence is not through a "real" period of time -- presumably because time is not "applicable," without a "fiction of the imagination," to any one unchanging object.

But what does Hume mean by his denying that "the idea of duration is applicable in a proper sense to objects which are perfectly unchangeable" (P. 37)? I think the following passage tells us:

> 'Tis evident that time or duration consists of different parts: For otherwise we could not conceive of a longer or shorter duration. 'Tis also evident that these parts are not co-existent: For that quality of the co-existence of parts belongs to extenseion, and is what distinguishes it from duration. Now as time is composed of parts that are not co-existent; an unchangeable object, since it produces none but co-existent impressions, produces none that can give us the idea of time. (pp. 35-36).

Hume is talking like a Lockean realist -- i.e., he talks of objects producing impressions. But his saying that "an unchangeable object ... produces none but co-existent impressions" seems to imply that he holds that an impression co-existent with a succession of other perceptions (which I take to be what he means by 'the impressions produced by an unchangeable object') is not divisible into distinct temporal parts, for all the parts are

co-existent. But anything that endures over a period of time must be composed of distinct non-co-existent parts (for otherwise we could not conceive of "its" lasting a longer or shorter period of time). So one (temporally) simple and indivisible perception cannot be said to endure for any period of time, even if it is co-existent with a succession of other perceptions.

If we assume that Hume held this view, we can explain certain striking lacks of parallel between his accounts of space and time. E.g., (1) an impression of one uniformly colored expanse gives us the idea of extension (see P. 34), but "a man ... strongly occupy'd with one thought is insensible of time" (P. 35), (2) he is careful to point out that nothing can be smaller than a perception of a colored or solid point (P. 28), but he seems to agree with Locke that states of matter can succeed each other faster than any of our perceptions (P. 35). For (1) whereas a uniform colored or solid expanse co-extensive with (i.e., the same size as) an array of colored or solid points is itself an array of (uniformly) colored or solid points, a perception co-existent with a succession of perceptions is not itself a succession of mutually (qualitatively) identical perceptions; another way to express the same point is: a single colored or solid point cannot be co-extensive with an array of other colored points, whereas a single perception can be co-existent with a succession of other perceptions; and (2) any

perception in our minds may be co-existent with a succession of states of matter, whereas, again, a colored or tangible point (in a perception) cannot be co-extensive with a physical extension.[8] And in terms of this view we can understand what Hume means by the unapplicability of time to a single perception: Each perception, no matter how many other successive perceptions it is co-existent with, has no duration, but is only one moment in any succession of perceptions of which it is a part.

But of course this view has paradoxical consequences. E.g., suppose one perception P is part of a succession S of three perceptions; and P is co-existent with a succession S_1 of four perceptions; and finally, the perceptions of S_1 constitute two-thirds of a succession S_2 of six perceptions, the fifth and sixth of which are co-existent with the second and third perceptions respectively of S. We would have to say that S_2 constitutes

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[8]There is no problem for Hume in comparing size in a visual or tactual field with physical size (or with size in another visual or tactual field). For first of all, he probably thinks of matter, or physical objects, as themselves perceptions (see P. 218). Secondly, he takes seriously such things as the extendedness, shape, size, etc., of perceptions -- e.g., he argues in I:IV:5 that the extendedness of our visual and tactual perceptions precludes their inhering in an immaterial substance just as much as, according to theologians, the extendedness of a physical body precludes its (see pp. 242-243), and thirdly, and most importantly, size is not purely relative according to Hume, since there is an absolute unit, namely the colored or solid point, by which the size of anything can, in principle, be measured.

a duration twice as long as that constituted by S, since it consists of twice as many temporal (i.e., consecutive, non-co-existent) parts; and yet S and S_2 are co-temporaneous! I.e., S_2 lasts twice as long as S, and yet they begin at the same time and end at the same time (and neither is interrupted). Since I do not know how to resolve this paradox while still holding the view in question, I would say that Hume (assuming he held the view) is wrong, and that therefore (assuming we have correctly interpreted his denial of the applicability of the idea of time to unchanging objects) it is perfectly correct to speak of a single perception or unchanging object enduring through a period of time. I would maintain that the pseudo-identity idea -- i.e., the idea of one perception co-existent with a succession of other perceptions -- really is an idea of CPT.

But H.H. Price argues, in effect, that we have no idea of one single perception co-existent with a succession of others, i.e., no pseudo-identity idea:

> But where shall we find an unchanging sense-impression? That is what we must have if Hume's account of Identity is to be saved. Obviously we cannot find it at all. Every sense-impression contains temporal parts within it. We see a colour-expanse, which, as we say, remains quite unchanged throughout a period. But what we are aware of is still a <u>series</u>, a continuous series whose members are exactly similar to each other. It differs from other series only in being monotonous, whereas most series of sense-impressions are variegated. But why do we say that it is a series, if there is no discernible variegation within it? First, because we know from past instances that it <u>could</u> have been interrupted at any point (by a blink, say)

> even though actually it was not. And secondly, because there actually is a difference of relational characteristics within it, though there is no difference of quality. We have to distinguish temporal parts within it, because we want to say that one part of it is contemporary with A and another part with not-A. While we look at our so-called unchanging colour-expanse, we see other neighboring colour-expanses come and go; we hear a squeak, then silence, and then another squeak; there is a succession of different organic sense-impressions. All round it and contemporary with it there is succession and perpetual qualitative differende. Consequently we must regard it not as a single particular, but as a process or series, divisible into a succession of temporal parts. Nor is there any fiction in this, as Hume would suggest. Relational characteristics are not less real than qualities. Two mutually incompatible relational characteristics cannot belong to a single entity, no more than two mutually incompatible qualities.[9]

A problem, of sorts, with Price's arguments is that, though he thinks they apply only to Humean impressions and ideas[10], they in fact apply equally to material substances. E.g., Mt. Rushmore is "contemporary" with my existing and with my not existing; therefore Mt. Rushmore must consist of at least two distinct temporal parts, one contemporary with me, the other not. But we normally do not think of Mt. Rushmore as composed of distinct temporal parts (a monotonous series of momentary Mt. Rushmores, as it were); rather we think of it as basically one and the same thing existing throughout a period of time, or at least contemporary with a succession of other things. So we should not accept Price's conclusion with respect to perceptions either. On the

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[9]Price, op. cit., pp. 46-47. See my Chapter 3, Part I.

[10]Ibid., P. 47.

other hand, perhaps we should change our normal way of looking at the world and accept Price's conclusion with respect to material substances as well as perceptions.[11] (I do not see any reason to distinguish between perceptions and substances so that perceptions must, but substances need not, be composed of distinct temporal parts in order to accommodate incompatible relational peoperties.) After all, what do we accomplish by way of explaining how one thing can have two incompatible properties P and not-P when we say that it has P at one time and not-P at another time, if we do not mean that it is really one thing that has P and another thing that has not-P? (I assume that talk of four-dimensional objects -- three spatial and one temporal dimension -- entails that objects consist of distinct temporal parts, just as we ordinarily think of objects as consisting of distinct spatial parts.)

Without going any further toward evaluating Price's arguments, let us see what the consequences of our discussion so far are for the conceivability of V and PV. We have three views about CPT based on Hume's basic

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[11] See R.M. Chisholm, "Identity Through Time," in Language, Belief, and Metaphysics, ed. Howard E. Kiefer and Milton K. Munitz (Albany: State University of New York Press, 1970) P. 180 n.4 for references to other arguments for the distinct-temporal-parts view of the world. Chisholm says that, arguments aside, the view "has at least the disadvantage of multiplying entities beyond necessity."

principles: (1) Hume's own express view that we have no idea of CPT, though we do have an idea of one perception co-existent with a succession of other perceptions; (2) the view I suggested, that one perception's co-existence with a succession of other perceptions is, even on Hume's principles, a case of CPT: and (3) Price's view that, on Hume's principles, there is an idea neither of CPT nor of one perception's co-existing with a succession of other perceptions -- i.e., what Hume spoke of as one perception co-existent with a succession of others is really a "monotonous series" of perceptions co-existent with a variegated series of other perceptions.

Obviously, on the second view, there is no problem. We have an idea of a perception's persisting unchanged through a period of time which we may use in V and PV. We got the idea from having in our minds one perception which lasted as long as a succession of other perceptions in our minds at the same time. Since a perception's existing outside any mind is conceivable according to Hume (see P. 207; see also my Chapter 3, Part III), there is nothing wrong with conceiving of a long-lasting perception existing outside any mind, and periodically either entering a mind, as in V (see pp. 207-208), or causing perceptions in our minds, as in PV.

Even the first view, Hume's own, poses no insurmountable problem for V or PV. For we can express both V and PV in such a way as to include no mention of CPT, but

only of a perception's co-existing with a succession of others; i.e., a perception's "continued existence" can be interpreted to mean its co-existence with a succession of other perceptions. There may be some problems expressing what we mean by 'one perception's coming into a mind, or causing perceptions in a mind, at different times or periods of its existence'. But these problems are basically the same as those presented by the very notion of one perception's not enduring through any period of time and yet being co-temporaneous with each of a number of perceptions (P_1 and P_2) which are not co-temporaneous with each other (i.e., which form a succession of perceptions); if he can accept the latter, paradoxical notion, then I guess he can accept such a perception's having relation R (e.g., being in a mind) while co-temporaneous with P_1, and not-R while co-temporaneous with P_2.

But on the third view, Price's, we have (according to Hume's principles) an idea neither of CPT nor of one perception's co-existing with a succession of perceptions. How are we to make sense of the "continued existence" posited in V and PV? Price himself tells us.[12] In V we are saying that certain impressions in our minds are parts of the same monotonous series of perceptions, most of which are not in our minds; and in PV we are saying that certain impressions in our minds are caused by extra-mental perceptions which are parts of the same monotonous series of extra-mental perceptions.

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[12] Price, op. cit., P. 48 f.

But what about our justification of belief in PV? On the second view everything remains unchanged (pardon the pun). On the first view, Hume's, all that is changed is terminology, so our reasons for believing what we express in different ways are unaffected. But on Price's view, which necessitates a re-interpretation of PV, it is not clear that we have the same justification as on our original interpretation. To properly assess the changes (if any) in our justification of PV entailed by this re-interpretation, we must distinguish between, on the one hand, PV's justification on the basis of its systematizing and simplifying the facts of experience and, on the other hand, its justification on the basis of its providing a satisfying explanation of those facts. With respect to the former, the justification will be the same; all we have to do is talk about (or quantify over) monotonous series of perceptions, rather than (as on the original interpretation) individual long-lasting perceptions, in order to achieve the same systematization and simplification.

With respect to the latter, the matter is not so clear. However, I think that a good case can be made for sayingthat this type of justification too remains unaffected.

It may be objected that PV's explanation of the constancy exhibited by our impressions is better when

construed in terms of enduring perceptions than when construed in terms of monotonous series of resembling perceptions. For on the former construal we explain qualitative identity of effects by numerical identity of the causes -- i.e., this impression is just like that because they were both caused by one and the same thing; while on the latter construal we explain qualitative identity of effects by qualitative ientity of the causes -- i.e., this impression is just like that because what caused this one was just like what caused that one -- which does not advance us beyond the banal "same effect, same cause," and the fact that the resembling causes are parts of a continuous, monotonous series is irrelevant for explaining the resemblance either of the causes or of the effects.

But I think the above objection is based on a confusion. For I think the fact that the resembling causes are parts of a continuous, monotonous series seems irrelevant to the objector only because he still interprets his experience in terms of enduring objects. If we so interpret our experience, then it tells us that, e.g., this doorman's being the (numerically) same one I saw here two months ago would explain why he looks the same, while his being part of a continuous series of (numerically) different doormen, beginning with the one I saw two months ago, all of whom looked

alike, far from explaining the resemblance, would only add to the mystery. But of course the question at issue is: If we interpreted all our experience in terms of monotonous series of objects instead of in terms of single enduring objects, would two objects' being parts of the same continuous, monotonous series provide as satisfactory an explanation of their resembling each other as their being numerically identical does in the context in which we interpret our experience in terms of enduring objects? And the answer to this question is: Yes! For the same experience which, under the latter interpretation gives us reason to be satisfied with the latter type of explanation would, under the former interpretation, give us the same reason to be satisfied with the former type of explanation. That is, in Humean terms, the same observed constant conjunctions which, interpreted one way, make us think there is a necessary connection between numerical identity and resemblance would, interpreted the other way, make us think there is a necessary connection between being a member of a continuous, monotonous series and resemblance.

It may be further objected that when we answer the question, 'Why do these two objects resemble each other?' by saying that they are both parts of the same continuous, monotonous series, we invite the same question with regard to the series itself -- i.e., 'Why do all the objects in the series resemble each other?' -- and we

have no immediate answer. But we can counter that when we answer the initial question by saying that it is the same object appearing at different times, we invite the question, 'Why did the same object endure throughout that period?' and to this also we have no immediate answer. It might be claimed that there generally is an answer to the latter question, for in most cases the object in question is one of a type that usually endures for that long a time. But then we can equally well answer the question of why there was such a continuous, monotonous series by saying that such objects usually come in continuous, monotonous series of such length.

To recapitulate: We started out by noticing that V and PV entail that perceptions endure unchanging through periods of time, and that Hume denies that we have an idea of a perception enduring unchanging through time; hence, V and PV should be unintelligible according to Hume. But we found that Hume admits that we have an idea of one perception co-temporaneous with a succession of other perceptions, though he denies that such a perception can correctly be said to endure through a period of time. And V and PV can be expressed in such a way as to entail that a perception is co-temporaneous with a succession of others, but not that a perception endures unchanging through time (assuming, with Hume, that the latter is not entailed by the former). We also

saw that there is reason to believe that Hume is inconsistent in admitting the conceivability of one perception's co-existing with a succession of others and yet denying the conceivability of a perception's persisting unchanging through time, because, even on Hume's principles, the former conception entails the latter; and that therefore we need not even change the terminology of V and PV in order to make them intelligible, according to Hume's basic principles. Finally, we considered the view that what Hume called "one perception co-existent with a succession of others" (and what we called "a case of changeless persistence through time") is just a continuous succession of mutually resembling perceptions, and that we have an idea neither of changeless persistence through time nor of one thing co-existent with a succession of others. And we found that, though adoption of this view would require a substantive change in V and PV, the change is easily made, and the justification of PV thus changed is the same as the justification of the original version of PV.[13]

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[13]Sydney Shoemaker, in a very interesting article, "Time Without Change," Journal of Philosophy 66 (1969), argues that a period of time in which absolutely nothing changes is conceivable. But his argument presupposes that the changeless persistence of one thing through a period of time in which other things change is conceivable. So his argument is not directly relevant to Hume's denial of the conceivability of CPT, or the kind of CPT involved in V and PV.

CHAPTER 5

JUSTIFICATION AND INTELLIGIBILITY

So far I have shown that we have some good reasons to accept the Philosophical View of Body (PV) and that Hume's views about changeless persistence through time pose no insuperable difficulties. However, there are some important details of PV which Hume claims we could never have any reason to accept, even if we could reasonably infer the existence of persisting external objects; the incorporation of these details into PV show "its dependence on the fancy," rather than on reason:

> First, we [i.e., the philosophers] suppose external objects to resemble internal perceptions. I have already shewn that the relation of cause and effect can never afford us any just conclusion from the existence or qualities of our perceptions to the existence of external continu'd objects: And I shall farther add that even tho' they cou'd afford such a conclusion, we shou'd never have any reason to infer that our objects resemble our perceptions. That opinion, therefore, is deriv'd from nothing but the quality of the fancy above-explain'd, that it borrows all its ideas from some precedent perception. We never can conceive any thing but perceptions, and therefore must make every thing resemble them.
>
> Secondly, as we suppose our objects in general to resemble our perceptions, so we take it for granted, that every particular object resembles that perception, which it causes. The relation of cause and effect determines us to join the other of resemblance; and the ideas of these existences being already united together in the fancy by the former relation, we naturally add the latter to compleat the union. (pp. 216-217).

These remarks of Hume's give rise, first of all, to the historical question, Did the proponents of PV

actually (1) "suppose external objects to resemble internal perceptions" and (2) "take it for granted that every particular object resembles that perception which it causes"? PV is basically a representative theory of perception. But most proponents of a representative theory of perception would deny that external objects resemble at least some of the perceptions they cause. Locke, for example, did not believe that the external causes of what he called our "ideas of secondary qualities" -- i.e., our impressions of colors, sounds, tastes, odors, and heat and cold -- resemble those impressions, though he did say that the external causes of our "ideas of primary qualities" -- i.e., our impressions of shape, size, number, and motion -- resemble these impressions.[1] So, though Locke was guilty of (1), he was guilty of (2) only with respect to primary, not secondary qualities; and the same can be said of most proponents of a causal theory of perception (i.e., they believe that the primary qualities, but not the secondary qualities, of matter resemble the impressions they cause).

Why, then, did Hume not distinguish in (2) between primary and secondary qualities? Probably because he believed that primary qualities, without secondary qualities resembling the ideas of them (I use Locke's terminology, but it would be true also with Hume's sense of 'ideas' -- only he would have used the more general 'perceptions'), are inconceivable. (See I:IV:4). E.g.,

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[1]See Essay, II:viii:15.

our conception of extension is the conception of an array of colored or tangible points; take away the color and the tangibility and there is nothing left. So since Hume was going to argue that it is impossible to hold (2) with respect to primary qualities only, he felt no need to explicitly mention such a position and say that it is unjustifiable.

Another possible reason for Hume's ignoring the distinction in the paragraphs quoted above is that, in that context, it made no difference. That is, both the view that only primary qualities resemble the perceptions they cause and the view that both primary and secondary qualities do are unjustifiable for the same reasons, so it would have been of merely historical interest to distinguish these views here. This brings us to another question, namely, What were Hume's reasons for claiming that (1) and (2) are unjustifiable?

It seems strange that Hume, who believed that there is no good argument for the existence of an external world in general, should nevertheless claim that even if there were such an argument, there still would be no good argument for either of these two specific claims about the external world. For he showed (P. 212) that, on his principles, there can be no good argument for the general claim; so if it turns out that there is one, then at least one of his principles must be wrong; but then how can he know that rejecting the faulty

principles will not open the door to good arguments for the specific claims? What makes the situation even more strange is that whereas Hume explicitly argues for his position that we can have no reason to believe the general claim, he presents no argument for his position that we can have no reason to believe the specific claims. One would expect that, since he apparently expressed more confidence in the latter position than in the former, the situation should have been the reverse -- at least he must have had some arguments up his sleeve for the latter position as well. But all he does is give his theory of the naturalistic origins of philosophers' adoption of (1) and (2), without bothering to explicitly argue that there is no rational reason to adopt them. Perhaps the reason for this omission is that relevant arguments were already well-known in his day. But, though Berkeley had some arguments to the effect that the causes of our perceptions cannot resemble perceptions[2] those arguments are based on a cause's having some "active power," which Hume denies, so Hume could not consistently use them. As for Hume's having greater confidence in his position on (1) and (2) than on PV in general, perhaps he had in mind a particular argument for the existence of an external world that might convince some people, and <u>that</u> argument does not lend any support to (1) or (2). (My guess is that Hume figured that his readers would

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[2]See, e.g., <u>Principles</u>, Section 25; also Section 8.

be convinced that everything must have a cause. He here claims that even if our impressions must have causes, we cannot know the nature of these causes, or that they resemble their effects.)

In any case, we must now consider whether, given our argument for the existence of persisting external objects, we have any reasons to believe (a) that these objects resemble perceptions and/or (b) that each one resembles the impressions it causes. Let us begin with (b). Proponents of PV would say that normally when I have in my mind an impression, e.g., of red, there is another impression of red, outside and independent of my mind, which exists before and after the impression in my mind. (For brevity, I will call impressions with continued and distinct existence "qualities," instead of "impressions" or "perceptions.") But it seems that the explanatory power of PV would not be diminished if, instead of a red-quality existing when I have a red-impression, it were a blue-quality or a green-quality, etc., that existed. Thus if I alternately open and close my eyes every few seconds and see a red tomato every time I open them, perhaps it is because there is a blue tomato in front of my eyes which continues the same all along. Perhaps such things as fire-engines, ripe tomatoes and apples, and "red" roses are really all blue, while clear sunny skies, etc., are really red. The same goes for any other color -- corresponding

impressions and qualities may, for all we can tell by the argument from the explanatory power of PV, be systematically of different colors. Similarly for tastes, smells, sounds, and heat and cold: e.g., a bitter taste-quality may correspond to a sweet taste-impression.[3]

But further, not only may it be a quality of a different color that corresponds to my impression of red; the quality may not be of a color at all. Perhaps the different color-impressions in my mind correspond to, or are caused by, qualities of correspondingly different degrees of heat and cold, or by the various shapes, sizes, or motions of the atoms which make up the "colored" surfaces. All that is confirmed by the constancy and coherence of our impressions is that some qualities or other exist, that the same quality gives rise to impressions that resemble each other, and that resembling qualities give rise to resembling impressions (this last is confirmed in that it provides the uniformity -- the constant conjunction of x-quality and y-impression -- that allows us to say that y-impressions are <u>caused</u> by x-qualities).

The above holds for the so-called "primary" as well as "secondary qualities." As was indicated above, according to Hume (at least), extension, figure, motion, and solidity are just (spatial) relations among colored or

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[3]This is a variation of the familiar "inverted-spectrum-argument" used in discussions of our knowledge of other minds.

tangible points (see I:IV:4 and I:II:3). But the relations among the qualities that give rise to our impressions of spatially related colored or tangible points may not be spatial at all. Suppose, e.g., that the qualities that correspond to colored or tangible points (let us call such a quality a "CTP" for short) are themselves not located in space and that each one is associated with a particular odor. (See I:IV:5, pp. 235-236 ff. for Hume's arguments that odors and most other types of perception are non-spatial.) These odors differ from one another in degree of bitterness, sweetness, and/or pungency -- corresponding to the three dimensions of space. There is a plenum of discrete odors in these three dimensions, and no two odors have exactly the same degrees of bitterness, sweetness, and pungency. Each CTP is associated with one and only one odor at any given time at which it exists, and no two CTP's are associated with the same odor at the same time -- this would correspond to saying that every colored or tangible point is somewhere in space, but never in two or more places at once, and that no two things can be in the same place at the same time. The same CTP will often associate with different odors at different times, but it must associate successively with each member of a series of successively contiguous (in terms of the three odor-dimensions) odors -- this corresponds to saying that a thing may move from one place to another, but must pass through every part of space

on a path connecting the two places. An impression of any macroscopic physical object, such as our own bodies, corresponds to a group of CTP's associated, at each moment of its existence, with a cluster of odors. As far as I can see, all our impressions of shape, size, motion, and solidity can be accounted for just as well in terms of such a system of odors as in terms of a system of colored and tangible points located in space; thus our impressionsof spatial relations need not resemble the relations, among qualities, that cause them.[4]

It may be objected, against the above line of reasoning and against Hume's second point in general, that the assumption that each impression resembles the quality that gives rise to it, when incorporated into PV, renders PV more intelligible; for we would not understand why, e.g., a blue-quality should give rise to a red-impression. My answer is that we as little understand why a red-quality should give rise to a red-impression. Granted, it is most natural to assume that corresponding impressions and qualities resemble each other; Hume would be the last to deny this. But it does not follow that we understand the correspondence any better if this assumption is true. Whether the quality resembles the impression

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[4]Compare Leibniz's view of spatial relations as well-founded phenomena which are in reality correspondences among the perceptions of monads. See, e.g., Frederick Copleston, S.J., A History of Western Philosophy (Garden City: Image-Doubleday) vol. 4, P. 311.

or not, in neither case do we have any notion of the mechanism by which the quality gives rise to the impression.

It may, however, be objected, against my discussing Hume's second point, that doubts about the resemblance of corresponding qualities and impressions are, at bottom, the same doubts that constitute the problem of induction; and, though these doubts may completely undermine any attempted justification of belief in an external world on the basis of our perceptions, we should not be concerned with them here since, as was stated earlier (Chapter 2), none of Hume's own arguments for skepticism with respect to the external world were based on them. The identification of our doubts about impression-quality resemblance withthe Humean doubts about induction may be made to seem plausible as follows: When presented with the fact that all so-far observed A's have been found to be B's, we infer -- with varying degrees of assurance, depending on various circumstances -- that all A's are B's; that is, from the premise

(1) All so-far observed A's are B's,

we infer the conclusion

(2) All so-far observed A's are B's and all as yet unobserved A's are B's.

The problem is, why is inferring (2) more rational than inferring

(3) All so-far observed A's are B's and all as yet unobserved A's are C's,

for any C dirrerent from B? Apparently the only reason for preferring (2) to (3) is the underlying assumption that the unobserved resembles the observed; and then there are the well-known difficulties about justifying this assumption. Similarly, our only reason for inferring, from our having A-impressions, that there is an A-quality, rather than that there is some B-quality, is again the assumption that the unobserved (in this case, qualities) resembles the observed (i.e., impressions).

But there is an obvious difference between the two inferences. For in induction we infer, from a conjunction of A's and B's in all observed cases, that A's and B's are conjoined in all unobserved cases as well. In inferring an A-quality from a group of A-impressions, however, there are no two kinds of things which have been conjoined in all observed cases and which we infer to be conjoined in all unobserved cases as well; rather we infer that the unobserved conjunct of a hypothesized conjunction resembles the observed conjunct. Therefore it is only in a trivial sense that both types of inference are based on the same assumption, namely that the unobserved resembles the observed. If we make the assumptions more precise, we find that the one underlying inductive inference is that kinds of things that were conjoined in observed cases are conjoined in unobserved cases as well, while that underlying our "resembling quality" inferences is that each unobserved conjunct of a hypothesized

constant conjunction resembles the corresponding observed conjunct. These two assumptions are independent of each other -- i.e., we may accept one and reject the other. In fact, I think we would naturally accept the first and reject the second. For inferences based on the first assumption are always grounded on previous experience, while those based on the second are not.

It may still be objected, again against Hume's second point, that the hypothesis that qualities resemble their corresponding impressions is simpler than any other, and therefore should be accepted over any other. My answer is that it is not clear to me in what way it is simpler. For how is the hypothesis that e.g., a number of red-impressions were caused by a red-quality simpler than the hypothesis that they were caused by a blue-quality? And how is the hypothesis that red-qualities generally cause red-impressions simpler than the hypothesis that blue-qualities generally cause red-impressions? As was said above apropos of the first objection considered, both hypotheses need to be augmented by an account of the mechanism by which the quality gives rise to the impression; and there is no indication that the mechanism in the case of the resembling-quality hypothesis will be any simpler than that in the case of any of its competitors. In fact, our current account of the mechanism entails that our impressions of "secondary qualities" represent, or are caused by, "primary

qualities."

Thus it seems clear that Hume's second point does hold as far as our argument for PV is concerned. It seems equally clear that his first point also holds; that is, we have no reason to believe that external objects are perceptions, or qualities, at all. The constancy and coherence of our impressions is explained just as well by the hypothesis that there is something, of the nature of which we can have no conception, with continued and distinct existence. But Hume argued that such a hypothesis is unintelligible.[5]

However, the view we are left with after taking into account Hume's two points is not that there are inconceivable objects, specifically different from perceptions. Rather it is the view that there are objects which may or may not be specifically different from perceptions; for all we know, these objects may be perceptions (that is, perceptions with continued and distinct existence, or qualities). But then what do we know about these objects? If the objects posited by PV may be anything, including inconceivable, then it would seem that PV boils down to the mere assertion that there is something -- we know not what --

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[5]Compare Anthony Quinton, The Nature of Things (Boston: Routledge and Kegan Paul, 1973), pp. 174-175: "How can any theory about the nature of objects or their relation to impressions be understood if the only words we can understand are those which refer to impressions and it is assumed that words for objects cannot be defined in terms of them?"

besides our perceptions.

However, though the intrinsic nature of the objects posited by PV is completely open, certain relations among these objects are determined by the aspects of our impressions which PV explains. What these relations are can perhaps best be understood by considering first the most naive version of PV (PVn) -- i.e., the version according to which the objects are qualities and each impression resembles the quality that caused it -- and then, rather than asking exactly what in it is not needed to explain the constancy and coherence of our impressions, as we have been doing until now, asking instead what is needed.

First of all, in any of the possible versions of PV (i.e., no matter what the intrinsic nature of the posited objects is supposed to be) there must be, for each quality in PVn, something which continues to exist as long as the quality does in PVn. For the constancy of our impressions is explained by the (numerically) same object's giving rise, at different times, to distinct but mutually resembling impressions. Secondly, objects in any version of PV corresponding to qualities in PVn that resemble each other must themselves resemble each other -- e.g., my seeing two scarlet patches is explained in PVn by there being two scarlet-qualities, so it is explained similarly in any version of PV by there being two entities, or objects, of the same kind. Thirdly, the durations of these objects must be temporally related just as the durations of the

corresponding qualities are in PVn. For coherence is explained in terms of one quality being replaced by another in a certain order over a certain period of time. Fourthly, there must be a set of relations among the objects analogous to the spatial relations among the visible and tangible qualities in PVn. For in order to explain the sequences of impressions which follow upon the impressions of my body moving -- i.e., why I get just such visible and tangible impressions when I get impressions of moving or traveling here and just such other visible and tangible impressions when I get impressions of moving or traveling there -- there must be some dimensions representable by a three-coordinate system in which the objects, including my body, are "located" and in which they "move." (See above, in the discussion of odors.)

Thus there must be a one-to-one correspondence between the qualities of PVn and the objects or entities of any other possible version of PV such that, whatever temporal and spatial relations hold among any qualities in PVn, there are corresponding relations that hold among the corresponding entities in the other possible versions. That is, if we represent the spatial and temporal relations among the qualities of PVn by the coordinates (x,y,z,t), relative to any arbitrary axes, then among the entities of any other version of PV there must be a set of relations representable by the coordinates (x',y',z',t'), relative to some axes, such that for

every set of values of x,y,z, and t associated with any quality in PVn there is the same set of values of x',y',z', and t', respectively, associated with the corresponding entity in each of the other versions. In short, there must be a one-to-one correspondence between the qualities of PVn and the entities of any other possible version of PV and a one-to-one correspondence between the set of spatio-temporal coordinates of each quality in PVn and some set of four-dimensional coordinates associated with the corresponding entity of any other version of PV such that the values of corresponding coordinates of corresponding entities are the same.

Would such a view be intelligible according to Hume? Well, as long as it is allowed that the entities posited by PV and their relations may be specifically different from anything ever encountered in impressions, he would find it at least problematic.

But perhaps Hume's concept of abstract or general ideas could be of help. (See I:I:7). According to Hume, when we think of man in general, or in the abstract, we have before our minds an idea of a particular man, but there are countless other ideas of other particular men in the back of our mind, so to speak, ready to come to the fore in case we start reasoning from any of the aspects of the idea before our mind which are not shared by all the ideas in the back of our mind as well; all these ideas thus keep our reasoning properly general,

or abstract. Similarly, we may have before our mind PVn, which certainly is intelligible according to Hume, with all the other possible versions of PV in the background but ready to come into the foreground in case we begin to take seriously any aspect of PVn not shared by all the other possible versions.

However, we have no idea (in the required sense of a group of copies of impressions) of the versions in which the entities are specifically different from perceptions, so we could very well reason from aspects of PVn shared by all the possible versions of PV of which we have ideas, but not by all possible versions of PV; for there would be nothing "coming to the fore" to stop us. We would, in effect, accommodate Hume's second point (i.e., that corresponding impressions and qualities need not resemble each other), but not his first (i.e., that the causes of impressions need not be perceptions, or qualities, at all).

Perhaps we could make a "distinction of reason" whereby we consider only the relations among entities, apart form the related entities. (See pp. 24-25). We can think of the color of a white sphere apart from its shape by having an idea of both its white color and spherical shape (for they are really inseparable) but "carrying our eye" to the resemblance this white sphere has to a white cube. Similarly, we can have an idea of

the qualities and their relations as posited in PVn but "carry our eye" to its resemblance to a different set of perceptions in the same relations, i.e., to a different version of PV.

However, this will not help us either, and for basically the same reason that the notion of abstract ideas did not. For we can "carry our eye" only to PVn's resemblance to other versions of which we have an idea. But PVn resembles these other versions in ways in which it does not resemble _every_ other version of PV. And we are looking for the ways in which PVn resembles _every_ other version.

Must we say, then, that on Hume's principles PV is unintelligible? Hume himself probably would see no way of rendering PV intelligible.But if we add to Hume's principles a conception of scientific theories developed in the twentieth century[6], then perhaps we can find a way. In the next chapter I will argue that we may consider PV as an "uninterpreted," or "partially interpreted calculus," some terms of which are linked to our impressions by a "dictionary" or by "rules of correspondence." PVn may serve as the _model_ associated with the theory. I will argue that PV is intelligible because it has a model, PVn. Whether Hume would agree, I don't know. But I think he would have to admit at least that a theory with a model is less unintelligible than one without a model.

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[6]See, e.g., Norman R. Campbell, _Foundations of Science_ (New York: Dover, 1920), chapter 6; Ernest Nagel, _The Structure of Science_ (N.Y.:Harcourt,Brace&World,1961),chapts.5&6.

CHAPTER 6

MODELS, THEORIES, AND THE PHILOSOPHICAL VIEW OF BODY

In the latter part of the preceding chapter I distinguished between what I called "the naive version" (PVn) of the Philosophical View of Body and a completely abstract construal (PV) of the same Philosophical View. According to PVn, the impressions of sensation in our minds are caused by other, generally longer-lasting impressions, or qualities, outside and independent of our minds, each such quality resembling the impressions it causes. The most important (for the purposes of prediction and explanation, or systematization[1]) features of PVn are: (a) a unified spatio-temporal framework within which all the posited extra-mental qualities exist, (b) the persistence of qualities through time, (c) the resemblance among certain distinct qualities at different places and/or times[2], and (d) laws,

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[1] By 'systematization' I mean basically the same as 'explanation' -- see the beginning of C.G. Hempel's "The Theoretician's Dilemma" in his Aspects of Scientific Explanation (New York: Free Press, 1965), P. 174 -- except that the question of the truth of the explanans (or the "systematizans") is left open.

[2] I am using 'quality' to mean the same as 'extra-mental impression'. Therefore it is proper to talk of qualitative, as well as numerical, identity between or among qualities. E.g., if two (numerically) distinct fire-engines are indistinguishable with respect to their (red) color, then the reds of the two fire-engines are two numerically distinct qualities which exactly resemble one another, or are qualitatively identical.

or lawlike statements[3], about the concomitance of various qualities, and about the succession of qualities by one another. Through the laws, governing the spatial and temporal relations among qualities, we can systematize a wide variety of facts about the impressions in our minds -- facts about their constancy and coherence, among others. (See above, Chapter 3.)

PV differs from PVn only in being "abstract" -- i.e., instead of calling the posited entities "impressions" (or "qualities") and attributing to them properties and relations applicable to impressions, as in PVn, PV leaves the nature of these entities and their properties and relations (almost) entirely open. The relationship between PV and PVn therefore boils down to this: there is a one-to-one correspondence between the entities (i.e., qualities), properties, and relations of PVn on the one hand and the entities, properties, and relations, respectively, of PV on the other such that corresponding entities have corresponding properties andrelations. But, as was argued in Chapter 5, the nature of the posited entities, properties, and relations is irrelevant to the systematizing power of PVn; all that is relevant is how

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[3]By 'lawlike statement' I mean a statement which is a (natural, or physical) law if it is true. See Nelson Goodman, Fact, Fiction, and Forecast (Indianapolis: Bobbs-Merrill, 1965), 2nd ed., P. 22. I do not distinguish between "experimental laws" and "theoretical laws."

the properties and relations are distributed over the entities. So the same systematization of the impressions in our minds is effected by PV as by PVn.

In this chapter I want to examine, in greater detail than I did in Chapter 5, the consequences of the relationship between PV and PVn as regards (i) the intelligibility of PV and (ii) the hypothetico-deductive justification of both PV and PVn. As was indicated in Chapter 2 (above, P. 27), these two issues, of intelligibility and of justification, are closely interconnected. But I will concentrate on the former in the section immediately following and on the latter in the final section.

I

The hypothetico-deductive (h-d) method is to (1) formulate a hypothesis, (2) deduce (or otherwise infer) testable consequences from the hypothesis, (3) test the consequences to see whether they are true, and (4) accept (provisionally) or reject the hypothesis according as the consequences turn out to be true or false. In the case of PVn the hypothesis is just everything we ordinarily believe about the external, or physical, world (with such terms as 'substance' and 'cause' interpreted along Humean lines -- e.g., a substance is just a bundle of qualities). But no statement about the external world

is testable in the relevant sense of the word, according to Hume; for only statements about perceptions in our minds are testable in this sense, and according to PVn nothing in the external world ever comes into (or is part of) our minds. PVn is in this respect (at least) like scientific theories which posit "unobservables." Testable, or observable, consequences can be inferred from such theories only if some of the unobservables posited by the theory are linked with observables. Statements which assert such linkage -- by stating that such-and-such is true of unobservables x if (or only if, or if and only if) so-and-so is true of observables y -- are called "correspondence rules."[4] Therefore, in order to be able to use the h-d method to justify belief of PVn, we need correspondence rules linking at least some of the things posited by PVn with impressions in our minds. Such correspondence rules are not hard to find. For part of what we believe about the external world (at least those of us who distinguish between impressions in our minds and qualities outside and independent of our minds) is that generally when certain parts of it

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[4] See, e.g., Ernest Nagel, op. cit., pp. 93-95 and 97-105. Nagel calls these statements "rules of correspondence," but gives a list of alternative appelations on the bottom of P. 93, to which we may add 'dictionary' (See N.R. Campbell, op. cit., P. 122) and 'bridge principles' (See C.G. Hempel, Philosophy of Natural Science (Englewood Cliffs: Prentice-Hall, 1966), P. 72.)

(including our bodies) are in certain states, we get impressions of certain sorts in our minds, and when we get impressions of certain sorts in our minds, it is because certain parts of the external world are in certain states. As it turns out, the testable, or observable, consequences of PVn and its correspondence rules are found, upon being tested, to be true; so we accept PVn as true.[5]

The situation is basically the same if PV is our hypothesi instead of PVn. The correspondence rules of PV are exactly the same as those of PVn, except of course that, for the terms of PVn designating the qualities posited by PVn and their properties and relations, the corresponding terms of PV are substituted. Clearly then, the testable consequences of PV are exactly the same as those of PVn. So PV would seem to be as acceptable, on the basis of the h-d method, as PVn.

However, as was stressed in Chapter 5 (following Hume's line of argument), PVn asserts far more than PV. PVn tells us what might be called the "intrinsic nature" of its posited entities, properties, and relations, whereas PV leaves the question of the intrinsic nature of the corresponding entities, properties, and relations open. And since the observable consequences of PV and of PVn

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[5]Actually the situation is much more complicated than this. But my oversimplification of the h-d testing of PVn is in keeping with my oversimplifying the account of the h-d method in the first place. My justification for these oversimplifications is my belief that on a more complicated (and more correct) account of the h-d method and its application to PVn, PVn would still come out acceptable. The main complications I am thinkingof are those described in Pierre Duhem's The Aim and Structure of Physical Theory (New York: Atheneum, 1974) Part II, Chapter VI.

are the same, there is no reason -- as far as the h-d method is concerned -- to accept the additional content of PVn. That is, if we consider PV and PVn as competing theories, then we are not justified in accepting PVn, but only PV. (But there are serious problems with accepting PV, as will emerge presently.)

Though the entities etc. posited by PVn and PV are equally unobservable, nevertheless those posited by PVn are asserted to be the same in kind as what we observe; they are described in the same terms that we use to describe the impressions in our minds, without any difference in the meanings of those terms. On the other hand, the intrinsic nature of the entities etc. posited by PV is left unspecified; the terms used to refer to them have no definite descriptive meaning. Thus PV may be compared to a theory expressed as an "abstract calculus"[6] the undefined, or "primitive" terms of which have no definite meaning, with the correspondence rules effecting a "partial interpretation" of the calculus by (conditionally or biconditionally) linking some of the sentences (or formulas)

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[6]See, e.g., Nagel, op. cit., pp. 90-91. In describing PV as an "abstract calculus" I do not mean to imply that PV is an axiomatized system; it could be expressed as an axiomatized system, but it need not be. The emphasis is, therefore, on the 'abstract', not the 'calculus'. (Of course, there are deductive relations among the sentences of PV -- the same as among the corresponding sentences of PVn -- so the term 'calculus' is not totally out of place.)

of the calculus expressed in primitive terms (and/or in terms stipulatively defined in terms of the primitives, which therefore also lack a definite meaning) with statements about observables. The correspondence rules only partially interpret the calculus because they do not give necessary and sufficient empirical conditions for the applicability of primitive (and stipulatively defined) terms in isolation or of many sentences involving them. (E.g., the correspondence rules of PVn assert a correlation between the unobservable state of affairs described as "my standing in front of a tree in daylight with my eyes open..." and the observable state of affairs in which I get a visual impression of a tree; but there is no observable state of affairs connected with a tree's standing unobserved in a quad; and no definite meaning is given to the term in PV which corresponds to 'tree' in PVn.)[7] PVn provides a meaningful correlate for each term and each sentence of PV; thus PVn may be called a "model" for the theory PV, in the sense of an interpretation of

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[7] If the ill-fated phenomenalist programme of "translating" all material-object sentences into sense-data sentences could be carried out so that for every material-object sentence M we could (truly) assert that M is true if and only if S is, where S is a sense-data sentence, then we could use the resultant set of sentences of the form 'M is true if and only if S is' as correspondence rules and thus effect a complete (as opposed to partial) interpretation of PVn; and by substituting for each M the corresponding sentence M' of PV, we would have a complete interpretation, via correspondence rules, of PV. But, needless to say, the prospects for successfully carrying out the phenomenalist programme are not very bright.

the abstract calculus which assigns a definite meaning to the primitive terms of the calculus (-- it is therefore a complete interpretation).[8] (E.g., where 'R' is a term of PV corresponding to 'impression of red' in PVn, we may view PVn as assigning the meaning impression of red to 'R'.) However, PV thus interpreted would be identical with PVn; and, as we said above, there is no h-d reason to believe that PVn is true. It seems, then, that we must be satisfied with a partially interpreted PV.[9]

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[8]For this definition of 'model' see Richard Bevan Braithwaite, Scientific Explanation (Cambridge: Cambridge University Press, 1953), pp. 88-90, and "Models in the Empirical Sciences," in Baruch Brody (ed.), Readings in the Philosophy of Science (Englewood Cliffs: Prentice-Hall, 1970), pp. 269-270; also Nagel, op. cit., pp. 95-97. Braithwaite says 'model' refers to a set of statements, or to "another theory." Nagel says 'model' may refer either to a set of statements or to "the system of 'things'" described by the statements. Since I will be concerned with the relation between PV and PVn, rather than between PV and the system described by PVn, I will use 'model' to refer to a set of statements (e.g., PVn).

[9]I do not think that the above account of PV and PVn commits me to the "logical positivist" analysis of theories in general as composed of axiomatized formal calculus, partially interpreted by correspondence rules, possibly with a model (as defined by Braithwaite and Nagel) in the background for didactic purposes. Rather, by following what I believe to be Hume's own line of argument (see the beginning of my Chapter 5), I have come to the conclusion that the particular theory we are dealing with, namely "The Philosophical View of Body," is really two theories, PV and PVn, fitting the descriptions of an uninterpreted calculus and of a (Braithwaitean) model respectively; and there are obviously correspondence rules linking entities posited by PV with impressions in our minds. Furthermore, the Humean distinction between impressions in our minds and impressions (or objects, or whatever) outside our minds provides a clear-cut distinction between observable and theoretical entities, though theoretical entities may be (perhaps must be, if the theory

But the fact that the terms of PV have no descriptive meaning raises problems form Hume's (and generally from an empiricist) point of view. For since the entities etc. posited by PV cannot be described sufficiently for us to form ideas of these entities which we could trace back to corresponding impressions in our minds, Hume would say that such entities are inconceivable, and that PV is unintelligible.

It is true that the terms of PV are "implicitly defined" by the sentences of PV.[10] But this is hardly the type of definition that would satisfy Hume. The correspondence rules may help somewhat in assigning empirical correlates to terms of PV. But, besides the fact that not all the sentences of PV appear in correspondence rules, these corresopndence rules tell us only that some theoretical states of affairs, which are not intelligibly described, are associated, or correlated, as necessary (or sufficient, or necessary and sufficient) conditions, with some observable states of affairs; and the assertion of this association does not render the

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is to be intelligible) described in terms applicable primarily to observables. This being the case, we may make use of the concepts developed by the "logical positivist" philosophers of science, however inadequate their view may be as an "explication" of scientific theories in general. (See Frederick Suppe's introductory essay and afterward in his The Structure of Scientific Theories (Urbana: University of Illinois Press, 1977).)

[10]See Nagel, op. cit., pp. 91-93. See also Campbell, op. cit., P. 122, where he says that "the propositions composing thehypothesis" of a theory provide "a 'definition by postulate' of the ideas which are characteristic of the hypothesis."

descriptions of the theoretical states of affairs themselves any more intelligible, at least according to Hume. The point is that according to Hume, a description of a state of affairs s is intelligible if and only if s is picturable in the imagination, and therefore if and only if each simple component of s has been an object of our experience; and PV just is not picturable, even with the implicit definitions provided by the sentences of PV, and with the associations with observables given by the correspondence rules.

It might be thought that PVn, as a model of PV, could somehow render PV intelligible. But, as was said above, we cannot simply identify PV with PVn -- or, in other words, simply accept PVn -- because there is no h-d justification for accepting certain aspects of PVn. And it is hard to see, at this point, how we could combine PV and PVn so as to get a theory which has the h-d justification of the former and the intelligibility of the latter. (But I will argue below that such a combination is possible, after having introduced some further technical concepts from the philosophy of science.)

Concern over the meanings of terms which do not refer to (and are not definable in terms referring to) observables did not die with Hume. Philosophers of science in the first half of this century have tried, unavailingly, to either define theoretical terms in terms of observables or else eliminate reference to unobservables altogether.[11]

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[11]See, e.g., Rudolf Carnap, "Testability and Meaning,"

More recently, however, there have been attempts to show that there is no special problem about the meanings of theoretical terms (as opposed to general problems about meaning). Hilary Putnam, in an influential article, argued that such a problem "does not exist."[12] And Carl G. Hempel, who at one time thought there was such a problem and tried to find a solution, quotes Putnam approvingly, suggesting "some considerations" of his own "in support" of the view that "the presumptive problem ... is misconceived."[13] I will argue briefly that Putnam and Hempel are merely sweeping a real problem under the rug.

After a few introductory paragraphs, Putnam's article is divided into three sections: in the first he argues that any so-called "observation term" could conceivably refer, or apply, to unobservable entities (e.g., why couldn't photons be red?); in the second he

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in Philosophy of Science 3 (1936) and 4 (1937); and Carl G. Hempel, "The Theoretician's Dilemma"; and see Nagel, op. cit., Chapter 6 for a discussion of the view that theories are translatable into observational terms and the view that theories are "logical instruments" whose terms do not refer.

[12]Hilary Putnam, "What Theories Are Not," in Logic, Methodology, and Philosophy of Science, ed. by Ernest Nagel, Patrick Suppes, and Alfred Tarski (Stanford: Stanford University Press, 1962), reprinted in Theories and Observation in Science, ed. by Richard E. Grandy (Englewood Cliffs: Prentice-Hall, 1973).

[13]C.G. Hempel, "On the 'Standard Conception' of Scientific Theories," in Analyses of Theories and Methods of Physics and Psychology, ed. by Michael Radner and Stephen Winokur, volume IV of the Minnesota Studies in the Philosophy of Science (Minneapolis: University of Minnesota Press, 1970), especially pp. 162-163.

argues that there is no sense of 'partial interpretation' which "is of any use in connection with 'the interpretation of scientific theories'"; and in the third he claims that there is no problem about learning or "introducing" theoretical terms as opposed to ordinary "observation" terms.

I have no objection to his argument in the first section. In fact, I construe PVn as a theory whose terms are all observational, even though they refer to unobservables; and I even admit that the entities posited by PV might be impressions (or qualities) and have properties and relations applicable to impressions. But I think that Putnam is blind to the point, which I have been stressing with regard to PVn and PV, that generally, though scientists may describe a theory's unobservable entities in familiar, observational terms, nevertheless we are not justified by the evidence in accepting the theory with those terms taken literally. For the information conveyed by the literal meanings of those terms does not enter into the deduction of observable consequences of the theory, and is therefore not confirmed by the truth of those consequences. All we are justified in accepting of the theory is that there are entities, properties, and relations which are referred to by the terms of the theory and which satisfy the theory's formal calculus (or its open sentences, or sentential functions). There are cases in which causal or analogical

arguments provide some justification for believing the information conveyed by familiar terms applied to the posited unobservable entities etc. (See below, in my discussion of Marshall Spector's suggestions along these lines.) But it is debatable whether such cases are the exception or the rule. (I will argue, in the final section of this chapter, that the availability of such analogical arguments is not necessary for a theory's acceptability.)

In the second section Putnam distinguishes three senses of 'partial interpretation'. His claim seems to be that <u>if there were</u> a special problem about the meanings of theoretical terms, it could not be solved or alleviated by means of partial interpretation, in any of these senses. Again, I have no objection to this claim. In fact, I proposed a basically Humean argument above to show the very same thing. Furthermore, apart from the fact that generally the same people who think there is a special problem about the meanings of theoretical terms also think they can alleviate the problem by means of partial interpretation (in one or more senses), Putnam's claim im this section seems to run <u>counter</u> to his overall thesis that there is no special problem about the meanings of theoretical terms; for he rejects a proposed way to account for the meanings of theoretical terms. So in objecting to his overall thesis I need say no more about this section. (But see below.)

In the third section Putnam tries to state a problem

about the meanings of theoretical terms which is not also a problem about the meanings of non-theoretical, or observational terms; and he concludes, from his inability to do so, that there is no such problem. But he overlooked the problem which I pointed out in my comment on the first section of his article. Another way to describe this problem is as follows: If we accept even "a rather minimal scientific realism," as Putnam does[14], then we must admit that theoretical terms refer to things which are not identical with observables (or with "constructions" out of observables). All we know about the referents of theoretical terms is what is involved in explaining, or systematizing, what we know about observables. And, as was argued in the case of PV, that amounts to knowing only how the theoretical properties and relations are distributed over the theoretical (and perhaps some observable) entities. I.e., all we know about theoretical predicates is their extension. So a theoretical predicate does not have the same kind of meaning that terms such as 'red', 'circular', or 'painful' have. What kind of meaning does a theoretical predicate have, as compared with such terms? That is the problem.

The only point that Hempel makes that has not already been dealt with is the following: Hempel admits that the sentences, or formulas, of a theory (including the

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[14]Grandy (ed.), op. cit., P. 120.

correspondence rules), though they "offer the learner the most important access to an 'understanding' of its expressions," nevertheless cannot render those expressions "fully specified" or "fully understood." But,he claims,

> even for terms that are generally regarded as quite well understood there are open questions concerning their proper use. For example, there are no sharp criteria that would determine, for any strange object an astronaut might encounter on another planet, or indeed for any object that might be produced in a test tube on earth, whether it counts as a living organism. Theoretical concepts, just like the concept of living organism, are "open-ended"; but that, evidently, is no bar to their being adequately understood for the purposes of science. 15

Thus, theoretical terms are no less fully understood than many ordinary terms, such as 'living organism'.

But there are at least two ways of viewing the concept of a living organism such that what Hempel says about it does not show that the problem I have been pointing out is not a special problem for theoretical terms. From one point of view, the concept of a living organism is complex, made up of a number of simpler concepts, with each of which we are acquainted (in Russell's sense of 'acquaintance'[16]) from experience. The"open-ended" nature of the complex concept is due to a certain degree of <u>indecision</u> as to exactly which of the simpler concepts to consider as <u>essential</u> to it, and the consequent possibility that in some unusual case

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[15]Radner and Winokur (eds.), <u>op. cit.</u>, P. 163.

[16]See Bertrand Russell, <u>The Problems of Philosophy</u> (Oxford: Oxford University Press, 1912), Chapter V.

we might feel compelled to make a decision, or to change our minds about which of the simpler concepts to consider essential. The problem with theoretical terms, on the other hand, is that we do not know enough about what they refer to to form a concept of them. Furthermore, theoretical terms may, for all we know, refer to entities, properties, and relations the likes of which we have no acquaintance with in experience, so that a concept of such an entity etc. all the constituents of which we are acquainted with is impossible. In a sense, then, we know too much about the living organisms with which we are familiar, but not enough about the referents of theoretical terms.

From another point of view, 'living organism' stands for some as yet unidscovered property or group of properties (e.g., some special sort of molecular or inter-molecular structure), with which we may or may not be acquainted from experience. The properties we think of as belonging to living organisms are all inessential and serve merely to fix the reference of the term. As long as we have not discovered the property or properties for which 'living organism' stands, cases may come up in which we are unsure whether to apply the term (because, e.g., one of the usual reference-fixing properties is missing). But from this point of view, 'living organism' is itself a theoretical term, the use of which involves the positing of an unobservable property; so its "open-ended-ness" is

an example of the problem of theoretical terms, not a counter-example.

Having argued that there is a problem about theoretical terms, I will now present the solution of the problem.[17] We can eliminate from a theory all terms referring to unobservables by uniformly replacing each such term by a variable. The result of this replacement is a string of open sentences, or sentential functions, which do not assert anything, and which are thus neither true nor false. To re-obtain a theory (which is true or false) we conjoin all these open sentences and existentially quantify (or generalize) over each variable, the scope of each quantifier being the whole conjunction. The result of all this is the "Ramsey sentence"[18] for the theory in question. The Ramsey sentence will have all the observable consequences of the original theory[19], so it will have all the systematizing power and all the h-d

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[17] See Grover Maxwell, "Structural Realism and the Meaning of Theoretical Terms," in Radner and Winokur (eds.) See also the articles by Maxwell that I referred to in Chapter 1 (P. 7 n.14).

[18] See F.P. Ramsey, op. cit., P. 231.

[19] See Herbert G. Bonhert, "Communication By Ramsey-Sentence Clause," in Philosophy of Science 34 (1967), pp. 342-343 for a proof that every observable deductive consequence of a theory is also a deductive consequence of its Ramsey sentence. See also Bonhert, "The Logico-Linguistic Mind-Brain Problem and a Proposed Step Towards Its Solution," in Philosophy of Science 41 (1974), P. 7 for a more informal proof. Israel Scheffler, The Anatomy of Inquiry (Indianapolis: Bobbs-Merrill, 1963), pp. 218-222, argued that a theory may have observable inductive

confirmation that the original theory has. And since it preserves the distribution of properties and relations over entities of the original theory, it incorporates all the content of the original theory that we are justified in accepting. But it has the advantage, over the original theory, that it contains no theoretical terms, or (non-logical) terms referring to unobservables, all such terms having been replaced by existentially quantified variables. Thus, e.g., instead of asserting that an increase in the (felt) heat of the air in a room is due to an increase in the mean kinetic energy of the molecules constituting the air in the room, the Ramsey sentence asserts that there are entities and there is a property such that the entities constitute the air in the room and the increase in the (felt) heat of the air in the room corresponds to an increase in the degree to which those entities have that property.[20]

It should be noted that "Ramsification" of a theory

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consequences not shared by its Ramsey sentence. But see Bonhert's "In Defense of Ramsey's Elimination Method," in Journal of Philosophy 65 (1968), pp. 279-280 for a rejoinder to Scheffler.

[20]This is not strictly correct, unless the theory involved is just that single assertion. For the Ramsey sentence of a theory is a single, monolithic sentence, which cannot be broken up into separate sentences, or separate explanations, the way the original theory could be. But see Bonhert, "Communication by Ramsey-Sentence Clause," where it is argued that there is no harm in extracting "clauses" from within a Ramsey sentence and using them in isolation from the rest of the sentence.

eliminates the non-logical terms that were supposed to refer to unobservables, but it does not eliminate reference to the unobservables, or commitment to the existence of these unobservables. If anything, the Ramsey sentence makes more explicit than does the corresponding un-Ramsified theory the assertion that there are, or there exist, such-and-such unobservable entities, properties, and relations. At one time -- i.e., when it was still in vogue to try to get rid of reference to unobservables -- this fact about Ramsification was viewed as a shortcoming.[21] But in more recent years scientific realism has come to be the dominant approach in philosophy of science, so the ontological commitment involved in the Ramsey sentence is now viewed as a point in its favor.[22] Thus, e.g., Putnam says that "the third sense of 'partial interpretation'," which is basically the sense in which I said earlier in this chapter that PV is only partially interpreted, is "unacceptable" because "it leads to the view that theoretical terms have no meaning at all, that they are mere computing devices," and therefore is "incompatible with a rather minimal scientific realism,"[23] If we supplement partial interpretation via correspondence rules by Ramsification, then it is clear that we are committed to scientific realism. But it also becomes clear in what

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[21] See, e.g., Hempel, Aspects of Scientific Explanation, P.216.

[22] See Maxwell in Radner and Winokur (eds.), op.cit., pp. 187-188 and 191.

[23] Grandy (ed.), op. cit., P. 120.

sense the constants referring to unobservables in an un-Ramsified theory are meaningless -- namely, they have (or should have) no more meaning than the existentially quantified variables which replace them in the corresponding Ramsey sentence.

There are, of course, (nominalist) philosophers who, though they are scientific realists, would consider extravagant the ontology to which use of a Ramsey sentence would commit us. For Ramsey sentences generally entail that _there are_ (or _exist_) _properties_, as well as individuals.[24] But it seems possible to construe the properties whose existence is asserted in a Ramsey sentence as sets of individuals, i.e., to construe them extensionally "for the benefit of the nominalistically inclined."[25]

Getting back to PV and PVn, it seems that the Ramsey method is the most obvious and natural way to express PV. We could start with PVn, then uniformly replace every term that refers to an impression (or quality) outside the mind or to a property of or relation among such impressions by a variable, then conjoin all the resulting open sentences and finally existentially quantify each

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[24] See, e.g., Israel Scheffler, "Reflections on the Ramsey Method," in _Journal of Philosophy_ 65 (1967), P. 270.

[25] See Maxwell in Radner and Winokur (eds.), _op. cit._, P. 188, n. 5.

variable, the scope of each quantifier being the whole conjunction. The result is PV -- i.e., all and only what is hypothetico-deductively justified in PVn.

Getting back to Hume now, does Ramsified PV meet Hume's standards of intelligibility? (Of course this question involves extrapolating quite a bit from what Hume actually said, for Hume hardly envisaged a Ramsey sentence with all its logical apparatus. But I think the extrapolation is straightforward. And besides, it will lead us to what I think is an interesting approach to PV and to theories in general.) Before we introduced the Ramsey method we found (in Chapter 5) that Hume could not admit the intelligibility of a term so abstract that what it refers to might be anything whatsoever, including something which is not a perception. Ramsification eliminates all non-logical terms (or predicate constants) of such unlimited abstractness. But there remain variables whose range is similarly unlimited -- i.e., any of the variables in Ramsified PV might take as a value something which is not a perception. And Hume's account of abstract reasoning (I:I:7) seems to entail that we could not properly manipulate variables which might take non-perceptions as values; for there would be no way to check whether we were using the unwarranted assumption that the values of the variables are perceptions (See Chapter 5, pp. 155-157.) Thus it seems that Ramsified PV is unintelligible on Hume's principles.

It is tempting at this point to simply reject some of Hume's principles and accept Ramsified PV as our theory of the external world. In particular, it would be adequate, and it seems reasonable, to reject Hume's view of thinking as perceiving, i.e., as the having of sequences of ideas, where by 'ideas' are meant faint copies of impressions -- what we might call "images" or "pictures in the mind's eye."[26]

However, we need not go that far (at least not just yet). For first of all, there seems to be an inconsistency involved in the argument which led us to say that PV is unintelligible on Hume's principles. But even if this inconsistency can be removed, there is a way to reconcile the intelligibility of PV with Hume's view of thinking. This reconciliation will give PVn a role in making PV intelligible. And since PVn is closer to our ordinary beliefs about the external world than is PV, having PVn play a role in our overall view of the external world is more in keeping with the goal of defending our beliefs about the external world than simply adopting PV alone.

Our Humean argument for the unintelligibility of PV was essentially as follows (see above, Chapter 5): We have no reason to believe that the entities that constitute the external world and their properties and relations are exactly as described in PVn; if we want our theory of the external world to be justified, we must make it more abstract. But no matter how abstract

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[26]See, e.g., G.E. Moore, Some Main Problems of Philosophy (New York: Collier, 1953), Chapter 4.

we try to make it, it will always be presupposed that the posited entities are perceptions, and this presupposition is unjustified. If we try to express the theory without making the problematic presupposition, the result will be unintelligible, for the possibility that the posited entities are not perceptions is literally inconceivable. So PV, which is supposed to be completely abstract, and therefore justified, is unintelligible.

But if the possibility of the posited entities' not being perceptions is inconceivable, how could Hume (or anyone) entertain, or conceive of, that possibility in objecting to the theory? Does it not follow from the inconceivability of non-perceptions that whoever suggests that the entities posited by PV might not be perceptions is talking nonsense? It seems that we have been using a double standard, implicitly allowing the intelligibility of talk about non-perceptions when arguing against PV, while blocking a proposed defense of PV by denying the intelligibility of anything that countenances non-perceptions. (E.g., in the passage quoted in Chapter 5, P. 141 above, Hume asserts that what causes our impressions of sensation are most probably not perceptions. It would seem that he is talking nonsense, according to his standards of meaning.) The point is that once we have an expression of PV which is as abstract as possible according to Hume -- i.e., where each term is a variable representing any perception (or any property or relation

of perceptions) whatever -- the suggestion that this expression is not abstract enough because the variables should range also over non-perceptions is illegitimate, or unintelligible; on Hume's view of meaning, it is meaningless. If, however, Hume would insist that the suggestion is meaningful, then it would have to be the case that non-perceptions are conceivable after all; but then, by parity of reasoning, PV would be able to accommodate the suggestion, for its variables could then intelligibly range over non-perceptions as well as perceptions. So either Hume's objection to PV is unsound or else there is a simple, satisfactory answer to the objection.

It may be possible to distinguish, within Hume's view of thinking, between (1) entertaining the possibility that something is not a perception and (2) having that possibility in the back of one's mind, keeping his reasoning about that thing properly general, or abstract; and to argue that (1) can be done but (2) cannot, thus validating the objection to PV and invalidating the defense. But I see no way to do this. So it seems that PV is, after all, intelligible on Hume's principles.

However, the conclusion of this argument is not quite satisfactory. For, Hume's theories aside, the point that the entities etc. posited by PV might not be perceptions, i.e., might not be at all like anything in our experience, does seem intelligible, and in fact well-taken. And yet

it is not at all obvious that we could reason about these entities etc. without (illegitimately) applying to them concepts which are applicable only to perceptions, i.e., only to objects of our experience, or acquaintance. The argument showed that *if* the point is intelligible, then we *should* be able to accommodate it, i.e., to reason about the posited entities etc. at the proper level of abstraction. But it does not tell us whether the point *is* intelligible, or whether we *can* reason so abstractly. In any case, it would be preferable to either explain *why* the point is not intelligible or else give an account of *how* we can accommodate it. I will do the latter.

The account is really quite simple. It involves expressing PV as a "formal system," or "formal calculus."[27] In constructing a formal system, we explicitly set forth certain terms (i.e., certain types of marks on paper and perhaps corresponding types of sounds) as undefined, or primitive, and we may define other terms by means of the primitive ones. Then we explicitly state rules of formation which specify exactly what combinations of terms do and which do not constitute sentences (or well-formed

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[27] See Rudolf Carnap, *Foundations of Logic and Mathematics*, which is vol. 1 no. 3 of the International Encyclopedia of Unified Science (Chicago: University of Chicago Press, 1939). See also Geoffrey Hunter, *Metalogic* (Berkeley: University of California Press, 1971), pp.4-10.

formulas). Certain sentences are specified as being axioms or postulates of the system (and in the intended interpretation, these sentences are all true). Then rules of transformation, or of inference, are explicitly stated, specifying which sentences can be derived from which other sentences (in the interpretation, which sentences are true if certain other sentences are true, or which sentences follow from, or are entailed by, which others). The result of introducing these terms, axioms, and rules is a formal system -- formal in the sense that no meaning is given to the terms or sentences. But though no expression of the system has any meaning, we can manipulate the terms and sentences by means of the rules, forming new sentences and deriving sentences from the axioms. Now we may interpret the system by assigning meanings to some of the terms in the system (so that the system is no longer purely formal) such that we can tell by observation whether sentences formed from those terms, with the assigned meanings, are true. We can then determine by observation whether or to what extent sentences made up of the now meaningful terms which are derived via the rules of inference from the axioms are true. If it turns out that, as far as we can tell, all meaningful sentences derived via the rules of inference from the axioms are true, and if we do not assign meanings to some of the terms that appear in the derivations of (observably) true sentences, then I think

we can say that, though we have (literally) no idea of what those meaningless terms refer or apply to, nevertheless we can use them in reasoning -- we can reason about whatever-it-is that they refer or apply to -- simply by adhering strictly to the rules of the (erstwhile formal) system. If the axioms of the system entail (via the rules of inference) a number of empirical laws, then we may be led to conclude that whatever is asserted by the axioms is true and that whatever the uninterpreted (i.e., not assigned any meaning) terms refer to really exist, though we may have no idea of the intrinsic nature of those referents.

An exposition of Ramsified PV as a formal system could start with any established formal system of (higher order) logic and mathematics together with its usual interpretation. Then we would add to this system as new primitive and defined terms all the terms of PV (or PVn) that (when interpreted will) stand for impressions in the mind or their properties or relations, plus rules of formation for those terms. (No new rules of inference need be added to those of the original logical-mathematical system, except the rule that the original rules apply to sentences containing the new terms.) We would then append Ramsified PV (See above, pp. 177-178) as an axiom. Finally, we will give the primitive terms from PV (or PVn) their usual meaning. The resulting system will lead, via

the logical and mathematical rules of inference, to true statements about the impressions in our minds, thus providing h-d confirmation of the system. Reference to the unobservable entities etc. posited by PV will be properly abstract because, since the referring term is always an existentially quantified variable, all that is presupposed about those entities etc. in referring to them is that they obey the rules of logic and mathematics. By strictly adhering to the expressly stated definitions and rules of the system, we will be able to properly manipulate the variables as well as the interpreted terms -- i.e., we will not backslide into reasoning from the (unjustified) premise that the values of the variables are perceptions. PV, thus construed, will be intelligible in the sense that we will know how to <u>use</u> it in reasoning without compromising its abstractness.

It might be objected, to our program of grafting PV onto a traditional system of logic and mathematics, that we have no reason to believe that the entities etc. posited by PV obey the rules of logic and mathematics, and that therefore our formal treatment of PV, in asserting that they do obey these rules, is not completely justified, and so is not the <u>totally</u> abstract formulation of PV that we are looking for. That is, in using this formalized PV we would be reasoning from only what is common to all versions (or interpretations, or models) of PV in which the entities obey the rules of logic and mathematics, but

not from only what is common to all versions of PV.

The answer to this objection is that if the rules of logic and mathematics are not obeyed by the entities posited by PV, then PV can have no h-d justification; for we could not say what such an alogical system would entail about the impressions in our minds. Of course it is possible, in a sense, that the external world -- the thing in itself -- is alogical, just as it is possible that any unjustified contingent assumption is true. But we need not be concerned with this possibility, for the following reason: In Chapter 5 we, in effect, defined 'a version of PV' as any theory of the external world which had a certain specific isomorphic relation to PVn, because it is in virtue of this relation that all the versions of PV have the same h-d justification based on the data of the impressions in our minds. Thus the primary requirement for being a version of PV is to have a specific h-d justification -- to entail certain statements about the impressions in our minds in a certain way. Now an alogical "version" of PV would not entail just those statements about the impressions in our minds; and even if it somehow would, it would not do so in the same way as PVn and the other bona fide versions do. Indeed, it is not even clear how we could determine whether an alogical "version" of PV has or lacks the requisite isomorphic relation to PVn. In short, a "version" of PV

according to which the posited entities do not obey the usual rules of logic and mathematics is not really a version of PV at all, and we need here give it no more consideration than the view that the external world is a homogeneous, undifferentiated mass -- a view whose explanatory value (and h-d justification) is just about nil.

It might be objected, however, that we do not "really understand" what is asserted by formalized, Ramsified PV, even if we know how to manipulate its symbols; PV is unintelligible in an important sense -- that is, it is not intelligible in the sense in which our view (even the Philosophical View) of the external world should be intelligible to us.

This objection raises two questions: (1) How intelligible is PV? and (2) How intelligible <u>should</u> an account of what we believe (or ought to believe) about the external world be? The objection is, of course, that the answers to (1) and (2) (in terms of degrees of intelligibility) are not the same. In answer to (1), let us compare our understanding of PV with our understanding (according to Hume) of general terms.

"To think," says Hume, "is nothing but to perceive," (I:II:6, P. 67). That is, according to Hume, thinking is just the having of ideas, or images, before the mind. Abstract thinking -- e.g., thinking, or reasoning, about man (or men) in general, or about triangles in general, or about geometrical figures in general -- is problematic

for Hume; for we seem to be capable of it, and yet there are no "abstract ideas" the having of which would constitute such thinking: e.g., there can be no idea (or image, or mental picture) of a man of no particular determinate color or shape or size, or of a triangle whose sides are neither equal nor unequal to each other. Hume solves the problem as follows: "A particular idea becomes general by being annexed to a general term; that is, to a term which from a customary conjunction has a relation to many other particular ideas, and readily recalls them in the imagination." (I:I:7, P. 22). We apply a single term to a number of distinct but resembling objects. The term then brings to mind <u>one</u> of those objects, but also puts a representative sampling of all the others "in readiness," or gives them the "power," to present themselves should we start reasoning from some aspect of the idea in our mind not shared by all the other ideas "in readiness" to come into the mind. E.g., suppose I am thinking about triangles in general, and the term 'triangle' happens to bring to mind a particular equilateral triangle; if in my reasoning I should ever try to make use of the premise that the sides of the triangle are equal, another idea of a triangle, one which is not equilateral, will come into my mind and stop me, thus keeping my reasoning properly general, or abstract -- i.e., keeping it about triangles in general, not just

equilateral triangles. (I:I:7, pp. 20-21).

On this account of abstract thinking, it is impossible to think in general terms about a class which includes things of which we have no idea (i.e., things which we cannot picture, or imagine); for there would be nothing "in readiness" to stop us from reasoning from properties shared by all the members of the class of which we do have ideas but not by those of which we have no idea. A term which purportedly applied to such a class would therefore be unintelligible to us, in the sense that we could not use it correctly in our reasoning; we could not distinguish in our reasoning between such a term and a term which applied only to the (proper) subset of the class in question composed only of the members of which we have ideas. Thus we were led to say that PV is unintelligible in that our use of it in reasoning would be no different from our use of a less abstract (and therefore unjustified) model of PV in which the posited entities are asserted to be perceptions.

Now we have seen that there is a way to keep our reasoning about the entities etc. posited by PV at the proper level of generality, or abstraction, namely, presenting PV as a formal system. But Hume's account of abstract thinking requires that a particular idea be before the mind. Can this requirement be met with respect to PV? Yes. This is where PVn comes in. Since PVn is a

model of PV, the qualities etc. posited by PVn satisfy the sentences of PV; so the idea of the particular qualities etc. posited by PVn is related to PV, in the relevant respect, just as the idea of a particular man is related to the general term 'man', or as the idea of a particular triangle is related to the general term 'triangle'. Therefore in thinking, or reasoning, about the entities etc. posited by PV we can have before our minds ideas of the corresponding entities (i.e., qualities) etc. of PVn, just as in reasoning about man in general we have before our minds an idea of a particular man. In both cases we will be able to avoid reasoning from fortuitous aspects of the particular idea before our minds. The difference is that in the case of PV we do not and cannot rely on the natural faculty of the mind that keeps our reasoning properly general in the case of 'man'. We rely rather on a sort of artificial device -- the formal system -- to keep our reasoning at the level of generality we want.

Thus it seems that our understanding of PV is on a par with our understanding (according to Hume, at least) of sentences involving general terms such as 'man' or 'triangle'. In either case we have before our minds a particular idea to which the theory or sentence applies, and we take precautions -- either naturally and automatically or artificially and consciously -- to ensure that we do not reason from fortuitous aspects of the particular idea

before our minds. This being our answer to question (1) above, we need not bother answering question (2). For surely we cannot demand any more intelligibility of our theory of the external world than that of most sentences in general terms. It thus seems that PV is as intelligible as it should be.

The view of the external world I have been defending is PV. But I have also argued that when we think about the external world, we may (or even should, if we want our view to meet Humean standards of intelligibility) have before our minds the picture described by PVn -- which happens to be the picture we generally do have before our minds when we think about the external world. When, out of a desire to believe and/or assert only what we are justified in believing or asserting, we want to avoid the excesses of PVn, we can advert to PV, or check our reasoning about PVn against the corresponding operations in formalized, Ramsified PV.

Now I mentioned above, in my discussion of Putnam, that it is sometimes justifiable to apply familiar, observational terms to the unobservables posited by a theory. Such an application of terms is tantamount to identifying the posited unobservables with the entities etc. described by a model of the theory, or to identifying the theory with its model. I have said that PVn is a model

of PV (see above, P.164). And it would be nice if we could justifiably identify PV withPVn. Our view of the external world would simply be PVn, and we could dispense with all the abstractions and formalisms that PV got us into. The h-d confirmation, or justification, of PV would devolve upon PVn, and so we would have justified something very close to our ordinary conception of the external world. Let us therefore examine the prospects for identifying PV with PVn. Even if (as I am afraid it will turn out) the identification cannot justifiably be made, the examination will prove to be of use in the next section of this chapter.

It has been argued by several philosophers of science that a theory and its associated model should never be identified, or confused, in this way. E.g., Nagel says

> ...a model for a theory is not the theory itself. In consequence, the adequacy of a theory for systematic explanation and prediction cannot be taken without further scrutiny to establish the physical reality of every aspect of the substantive model in terms of which the theory may be interpreted. 28

Thus, though calculations made using PV lead to true predictions about our impressions and a unified systematization of a lot of otherwise unrelated facts about our impressions, and though PVn is a model of PV, there is no reason to believe that the entities etc. posited by PVn exist. Braithwaite adds that

> Thinking of scientific theories by means of models is always as-if thinking; hydrogen atoms behave (in certain respects) as if they were solar systems

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[28] Structure of Science, P. 116.

> each with an electronic planet revolving round a protonic sun. But hydrogen atoms are not solar systems; it is only useful to think of them as if they were such systems if one remembers all the time that they are not. 29

Thus all we can say is that our impressions behave as if they were occasioned by extra-mental impressions as described in PVn; but we should always remember that they are not so occasioned. Similarly, the entities etc. posited by PV behave (or can be described) as if they were the corresponding entities etc. of PVn; but we should always remember that they are not.

However, Marshall Spector[30] has pointed out that scientists sometimes do identify a theory with its model; and more importantly, he has suggested a distinction between models that are identified with their associated theories and those that are not, and has argued (I think cogently) that this distinction makes a difference as to the reasonableness of making the identification: In the cases in which the theory is identified with its model, the meanings of the terms referring to observables linked via correspondence rules to theoretical terms are the same as (or similar to) the meanings of the terms in the model corresponding (respectively) to those (observational) terms;

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[29] Scientific Explanation, P. 93.

[30] "Models and Theories" in British Journal for the Philosophy of Science (1965), reprinted in Brody (ed.) op. cit., pp. 276-293.

or, more generally, if 'x if [or only if, or if and only if] y' is a correspondence rule for a theory T, where 'x' refers to a theoretical and 'y' to an observable state of affairs, then there is in the model M a (true) sentence of the form 'a if [or only if, or if and only if, respectively] b' where a is what in M corresponds to x in T, and 'b' and 'y' have the same (or similar) meaning (A special case would be where 'a' means the same as 'y', so the relevant sentence in M would be simply 'a iff a'.). E.g., in the oft-cited kinetic theory of gases, the model is a description of a closed container with tiny elastic balls in random motion inside.

> ...there is an expression in the calculus which, by the interpretation given the calculus in the model, represents the total rate of momentum transfer per unit area of the walls of the container in which the elastic spheres are moving. This expression, when the calculus is used to represent the theory, appears in a correspondence rule of biconditional form... with the observation term P, which designates the pressure of the gas. But according to classical dynamics, rate of momentum transfer per unit area is equal to force per unit area, which is (by definition) the pressure on the walls of the container. [31]

Similarly the term in the calculus linked by a correspondence rule of biconditional form with the volume of the gas corresponds in the model to the volume taken up by the elastic balls in the container. And the theoretical term linked by a correspondence rule of biconditional form with the temperature of the gas (as measured by a

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[31] Brody (ed.), op. cit., P. 284. Emphasis added.

mercury thermometer) corresponds in the model to the mean kinetic energy of the elastic balls, which would have an effect on an analogue of a mercury thermometer analogous to the effect of the gas on a thermometer . Therefore -- assuming that the only observable properties of gases we have correspondence rules for are pressure, volume, and temperature -- it is reasonable to identify (e.g.,) the kinetic theory of gases with its model and to say that the entities (i.e., "molecules") posited by the theory <u>are</u> tiny elastic balls in violent random motion constituting the gas in the container. The reasonableness of the identification is due to an argument by analogy: The theoretical entities are like the model entities in their relations to one another (i.e., the same calculus which represents the relations among the theoretical entities could also represent the relations among the model entities) and to the same observable phenomena; therefore (probably) they are like the model entities in other respects as well -- including in their intrinsic nature.

Now PVn seems <u>prima facie</u> to be a model of the type that, according to Spector, can reasonably be identified with its associated theory (PV), and therefore can lay claim to the evidential support that derives from the predictive and systematizing success of the theory. For PVn and PV (by definition) share the same formal structure; and the

impressions in our minds associated, via correspondence rules, with terms in PV are the very ones associated, again via correspondence rules, with the corresponding terms of PVn. However, I think that PVn fails to satisfy at least two requirements for its identification with PV which are implicit in Spector's argument.

First of all, let us call the set of sentences of a theory T none of the terms of which refer to observables the "hypothesis" of T. And let us distinguish, in a model M for the theory T, between the set of sentences in M which correspond to the hypothesis of T, which we will call the "m-hypothesis" of M, and the set of sentences in M which correspond to the correspondence rules of T, which we will call the "m-correspndence rules" of M. Now in the example of the kinetic theory of gases and its elastic spheres model, the m-correspondence rules were seen to <u>follow from</u>, or to be <u>entailed</u> by, the m-hypothesis plus the laws of classical dynamics. Thus there is a <u>nomological</u> relation between the m-hypothesis -- including both the intrinsic nature and the inter-relation of the model entities -- and the m-correspondence-rules, or between the entities (i.e., the tiny elastic spheres) of the m-hypothesis and the phenomena (i.e., pressure, volume, and "temperature") linked with them via the m-correspondence rules. But the m-correspondence rules of PVn do not follow from the m-hypothesis of PVn (and any

known natural laws). There is no reason, given the m-hypothesis of PVn, to suppose that certain combinations of the impressions (or qualities) in the spatio-temporal framework posited by PVn give rise to certain other impressions outside that framework, as the m-correspondence rules assert. The m-correspondence rules of PVn are appended to the m-hypothesis of PVn in as arbitrary a manner as the correspondence rules of PV are appended to the hypothesis of PV -- in both cases the reason for appending just those correspondence rules (and m-correspondence rules) is simply to be able to derive true statements about impressions in our minds. But it seems that an analogical argument, which is basically of the form 'x is ABC, y is AB, therefore (probably) y is C as well', is acceptable only if there is reason to believe that AB, the respects in which the two analogues are known to be the same or similar, are causally or nomologically relevant to C, the respect in which we want to infer that the two analogues are the same or similar.[32] This requirement is met in the case of the kinetic theory and its model, since the fact that the entities in the container (in the model) are relatively small elastic

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[32]See Mary B. Hesse, Models and Analogies in Science (Notre Dame: University of Notre Dame Press, 1966), pp. 77-78, 81-89.

spheres in random motion (C) is causally relevant to the way they interact with one another (A) and to their giving rise to pressure and "temperature" the way they do (B). But the requirement is not met in the case of PV and PVn; for there is no nomological (and certainly no logical) relation between the posited entities' being impressions (or qualities) of the sort described in PVn (C) and their being related to each other the way they are supposed to be (A) or to their giving rise to impressions in our minds the way they are supposed to (B).

Another requirement, related to the above, that it seems a model would have to satisfy in order to be justifiably identified with its associated theory is that it be the only available model, or at least the best of all available models (i.e., the one most likely to indicate the true nature of what is posited by the theory). Because of the substantive, as well as purely formal, analogy between the kinetic theory and its elastic sphere model (i.e., because the terms linked with theoretical terms in the correspondence rules of the theory and the terms linked with the terms corresponding in the model to those theoretical terms have the same meaning, as well as the model's being representable by the theory's calculus), it seems that, even if there are other models, the elastic sphere model is the one most likely to indicate the true nature of what is asserted by

the theory. (This is just Spector's point put in a comparative way.) But, as was argued in Chapter 5, there are an indefinite number of alternative models for PV, besides PVn, none of which seems in any way more entitled than any other to be identified with PV. Granted, of all the alternative models, it is most natural to suggest PVn; but this "naturalness" does not seem to justify our preferring it to any of the others. Logically they are all on the same footing.

II

So far we have been discussing primarily the role of PVn in rendering PV intelligible. Perhaps the impression has been given that once PV, construed as an abstract theory, is rendered intelligible, its h-d justification is simple and straightforward. However, there is reason to think that there are problems with the justification of PV which have to do with a lack of a certain kind of model, or analogy, for the theory. It may even be argued that Hume himself raised such problems.

I have indicated (in Chapter 2) that Hume's most cogent argument against PV is based on his belief that the only reasonable way to come to a belief of a matter of fact not directly observed (i.e., not given in an impression in the mind) is by causal, or inductive, inference. I have implied that Hume's analysis of factual inference is defective in that he ignored the hypothetico-

deductive (h-d) method. But I admit that it is at least possible that in his arguments for his analysis of factual inference there is an implicit refutation of the view that the h-d method yields justified, or reasonable, inferences.

What makes this possibility somewhat plausible is the fact that in more recent times several philosophers of science have argued that the h-d method must be supplemented by some sort of analogical reasoning in order to yield justified inferences[33]; and the analogical reasoning suggested is in some ways very much like causal, or inductive, reasoning as analyzed by Hume. Thus Hume argued that we can infer, from a given fact e, another fact c which has a certain relation R to e only if we have observed facts like c having relations like R to facts like e. And we now find people arguing that it is not enough for a physical (as opposed to mathematical) theory that it entail the laws governing the phenomena about which it is a theory; a theory requires, in addition, an analogy, or model -- that is, we must know of something else, similar in certain respects to what is posited by the theory and standing to something similar to the associated phenomena in a relation similar to that supposed to hold between the theoretical posits and those phenomena.

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[33]See Campbell, op. cit., Chapter VI, especially pp. 128-130. Relevant sections are reprinted in Brody (ed.), op. cit. See also Hesse, op. cit., pp. 1-56; and Spector's article in Brody (ed.).

Without such a model, it is argued, a theory is not explanatory[34], or not "genuinely predictive"[35], or even "not worthy of the name['theory']."[36] Let us examine these arguments (since, as far as I can tell, Hume himself did not argue specifically that the h-d method must be augmented by analogical or causal reasoning) as they apply to PV.

In accepting PV we are inferring the existence of extra-mental entities which cause impressions of sensation in our minds. According to Hume, in order to be able to make this inference, we must have observed things like the impressions in our minds constantly (or at least more often than not) preceded by things like the entities posited by PV. But it seems that we have not observed any such constant (or frequent) conjunction. All we "observe," in the relevant sense, are the impressions in our minds; we do not observe, conjoined with or preceding these impressions, anything which would satisfy the hypothesis of PV. Therefore, according to Hume, we cannot accept PV. (See my Chapter 2, pp. 22-23).

What about the fact that the hypothesis of PV plus correspondence rules entail (or at least render probable,

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[34]P.W. Bridgman, quoted in Nagel, op. cit., pp. 114-115.

[35]Hesse, op. cit., P. 5.

[36]Campbell, op. cit., P. 129.

or lead us to expect) that we would get in our minds precisely the sorts of impressions that we do in fact get? Doesn't this fact give us a reason to accept PV? Well, according to N.R. Campbell, it does not. Campbell argues that the purely logical requirement that a theory entail the phenomena to be explained by it is not by itself a criterion of acceptability: "Any fool can invent a logically satisfactory theory to explain any law. ... If nothing but this were required we should never lack theories toexplain our laws; a schoolboy in a day's work could solve the problems at which generations have laboured in vain by the most trivial process of trial and error."[37] For a theory to be acceptable it must satisfy an additional requirement: "it must display an analogy. The propositions of the hypothesis must be analogous to some known laws." Again, "analogies are not 'aids' to the establishment of theories; they are an utterly essential part of theories, without which theories would be completely valueless and unworthy of the name."[38]

Campbell does not make clear exactly what he means by 'analogy'. But it is clear that he does not mean a model of the type of which PVn is an example. For he requires that the analogy be to some <u>laws</u>, not merely to some imaginable state of affairs; whereas there do not

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[37] <u>Ibid</u>., pp. 129-130.

[38] <u>Ibid</u>., P. 129.

seem to be any laws which would link the qualities posited by PVn with each other or with impressions in our minds in the manner described in PVn. Furthermore he seems to say that the analogy should suggest correspondence rules[39], thus indicating that what he had in mind was closer to the type of model that Spector claims is to be identified with its associated theory (See above, pp.193-199) than to PVn. (It may also be significant that Campbell's prime example of an analogy is the same as Spector's example of a model identifiable with its theory -- namely the elastic sphere model of the kinetic, or "dynamical" theory of gases.)

To expedite describing what sort of analogy I think Campbell requires of a theory, let us develop a little further the vocabulary (if one may call four terms a vocabulary) introduced earlier, in discussing Spector's proposal (P.196). First of all, let us say that a model M of a theory T is a description, in terms whose meanings we understand, of an observable state of affairs such that there is a one-to-one correspondence between the (meaningful) terms of M and the terms of T and between the (meaningful) sentences of M and the sentences of T. (See above, P. 165, footnote 8 and correlative text.) Now we distinguished in T between the <u>hypothesis</u> of T and the

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[39] <u>Ibid</u>., pp. 128, 133.

correspondence rules (what Campbell calls the "dictionary") of T; and we called the set of sentences in M corresponding to the hypothesis of T the "m-hypothesis" of M, and the sentences of M corresponding to the correspondence rules of T the "m-correspondence rules" of M. (P.196). Let us now call the terms of T that refer to observables the "observational terms" of T, and the corresponding terms of M the "m-observational terms" of M; and let us call the terms of T referring to unobservables the "hypothetical terms" of T, and the corresponding terms of M the "m-hypothetical terms" of M. Finally, let us call the sentences (in m-observational terms) corresponding to sentences in observational terms entailed by T the set of "m-laws" of M. The m-correspondence rules link the m-hypothesis to the m-laws, just as the correspondence rules of T link its hypothesis to the phenomena explained by T.

What Campbell seems to require of a theory T is that it have a model M such that (1) the sentences of the m-hypothesis are (or are deducible from) empirical laws, (2) the m-correspondence rules follow from, or are entailed by, the m-hypothesis plus other empirical laws (see P.196), and (3) the meanings of the m-observational terms are the same as or similar to the meanings of the corresponding (observational) terms of T (see P.194). (Clauses (2) and (3) allow for the model's suggesting correspondence rules for the theory -- via analogical reasoning.)

If this is indeed what Campbell requires, then it is almost the same thing that Hume requires. For if the m-correspondence rules are to follow, via known empirical laws, from the m-hypothesis, then -- assuming a Humean, or "regularity" view of laws -- we must have observed a constant conjunction of states of affairs of the type described in the m-hypothesis with states of affairs of the type described in the m-laws (or at least we must have observed constant conjunctions that together entail the above constant conjunction); which means that we must have observed a constant conjunction of states of affairs which are (structurally, or formally) like those described in the hypothesis of the theory with states of affairs like those described in the sentences in observational terms entailed by the theory. Again, the point of this comparison of Campbell and Hume here is that Campbell, unlike Hume, explicitly argues that the h-d method alone, without an analogy of the requisite kind, cannot render a theory acceptable. The question is: Is his argument sound?

The argument, which we mentioned above, is as follows: The purpose of a theory is to explain a law[40] or set of laws. This purpose it achieves by means of a hypothesis -- that is, a set of propositions couched in terms

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[40]By 'law' Campbell means basically a general statement all the substantive terms of which refer to observables, and which therefore can be established by simple induction from observations; see op. cit., Chapter 5.

"characteristic of the theory" but not generally found outside the context of the theory (at least not before the theory is invented). The referents of the hypothesis' characteristic terms are not observable. But a dictionary (or set of correspondence rules) relates propositions from the hypothesis to propositions about observables. The hypothesis and dictionary, which constitute the theory, together must entail a law L. However, there is never any shortage, for any law L, of hypotheses cum dictionaries which entail L; so if this entailment is all that is needed for a theory to explain L, then there would seem to be an indefinite number of easy-to-construct theories to explain any law. Yet nearly all of these theories would be intuitively "valueless and unworthy of the name"; they would not really be explanations of L.[41] What renders a theory valuable? What makes it an explanation of a law it entails? To answer this question Campbell invents a hypothesis plus dictionary that entails a law but which is intuitively unsatisfactory as a theory to explain that law.[42] He then compares it to the intuitively

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[41]It is not always clear whether Campbell is claiming that a theory without an analogy does not explain at all or only that it does not explain as well, or as satisfactorily, as one with a n analogy. See op. cit., P. 157 for a basis of the latter interpretation. See Campbell, What Is Science?(New York: Dover, 1953) (originally published in 1921), P. 83 for a basis of the former interpretation. But even if Campbell himself admitted that a valuable explanatory theory need not have an analogy, it is worthwhile to examine arguments of his that seem to lead to the opposite conclusion.

[42]Campbell, Foundations of Science, pp. 123-124.

acceptable dynamical theory of gases.[43] He concludes that the difference between the two, by virtue of which the former is not but the latter is an explanatory theory, is that the latter is and the former is not analogous to some set of already known laws, and that in general it is necessary[44] for a theory that it display an analogy to some known law or set of laws if it is to be acceptable.

Hempel has argued against Campbell, claiming that analogies are inessential -- indeed irrelevant -- to the explanatory value of theories.[45] In keeping with his overall thesis that explanation is subsumption under general laws, Hempel claims that the difference between valuable theories and the indefinite number of hypotheses cum dictionaries which entail laws -- and in particular between the kinetic theory of gases and Campbell's invented "theory" -- is not a matter of analogy. Rather

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[43] Ibid., pp. 126-128.

[44] The general outline of Campbell's argument for the importance of analogies -- i.e., his comparison of valueless and valuable theories -- seems to indicate that he holds that it is both necessary and sufficient, in order for a theory which entails a law to be valuable, that it display an analogy (though there may be degrees of value, so that we need not accept every theory that displays the requisite type of analogy). However, when he states his position, he seems to assert only that displaying an analogy is a necessary condition of value, or acceptability. See John Losee, A Historical Introduction to the Philosophy of Science (Oxford: Oxford University Press, 1972), bottom of P. 141.

[45] See the title essay of his Aspects of Scientific Explanation, pp. 433-447, especially pp. 442-445.

> What is wrong with [Campbell's invented] theory, so it seems to me, is that it has no empirically testable consequences other than the law in question (and whatever is logically implied by it alone); whereas a worthwhile scientific theory explains an empirical law by exhibiting it as one aspect of more comprehensive underlying regularities, which have a variety of other testable aspects as well, i.e., which also imply various other empirical laws. Such a theory thus provides a systematically unified account of many different empirical laws. [46]

Now if it is true that all and only acceptable explanatory theories share this property of implying a number of different empirical laws, then Campbell's argument for the necessity of an analogy is invalid; for then the presence or absence of an analogy would not be the only difference between valuable and valueless theories, and would therefore not (for all Campbell has shown) be proven to be necessary for an acceptable explanatory theory. Let us examine, therefore, whether it is true that all and only acceptable explanatory theories entail a number of different empirical laws.

Is it <u>only</u> acceptable explanatory theories that have the property of implying a number of different empirical laws? Well, Campbell's example of an unacceptable theory certainly lacks this property. And if we add, to the requirement that a theory entail a law, that it must also entail a number of various other laws, then it is no longer true that "any fool" or "a schoolboy in a day's work" could

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[46] <u>Ibid</u>., P. 444.

come up with a theory to adequately explain any law. Thus it seems likely that the indefinite number of defective "theories" all lack the property in question.

Do all acceptable explanatory theories entail a number of different empirical laws? Well, one consideration that points to a positive answer is the following: It has been argued[47] that in any explanation it must not be the case that the only grounds for believing, or accepting, an explanatory statement (i.e., a statement in an explanans) is the very thing to be explained (i.e., the explanandum). E.g., there would be something wrong with explaining a vase's lying broken on the floor by reference to Johnny's carelessness if the only evidence for Johnny's carelessness is the vase's lying broken on the floor. Now suppose we want to explain a law L by means of a theory T. What grounds could there be for accepting T besides the fact that it entails L? Such grounds would have to be some true observable implications of T other than L. When enough different implications of T have been found to be true, T will be viewed as well-confirmed; and only then will it be deemed worthy of acceptance as an explanation of L. By the same token, T will also be acceptable as an explanation of each of its other true implications. It turns out, then, that T must provide "a systematically unified account of many

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[47] E.g., by Nagel in The Structure of Science, pp. 43-44.

different empirical laws" (I take it for granted that implications of a theory alone will always be general laws, rather than particular facts) if it is to explain even a single law.

Unfortunately, Hempel himself does not agree with the above stricture on explanatory statements, for he says that some acceptable explanations are "self-evidencing" -- i.e., we believe part of the explanans only because we believe the explanandum.[48] E.g., we could explain why my bicycle tire looks flat by reference to the air's having escaped from it, when our only reason to suspect that the air has escaped from the tire is its looking flat. If self-evidencing explanations are acceptable, then my argument above to show that every acceptable explanatory theory entails a number of different laws is unsound.

Hempel does, however, admit that "an explanation that is self-evidencing may for that reason rest on a poorly supported explanans and may therefore have no strong claim to empirical soundness."[49] That is, as we argued above (following Nagel), a single true implication of a theory, or the fact that a statement, if true, would provide an explanation of one law or particular fact is hardly an overwhelming confirmation of that theory or statement. "But," Hempel continues,

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[48] Hempel, Aspects of Scientific Explanation, pp.370-374.

[49] Ibid., P. 373.

> even this is not inevitable. In the case of the absorption spectrum of a star, for example, the previously accepted background information, including the relevant theories, may indicate that the dark lines observed occur only if the specified elements are present in the star's atmosphere; and then the explanandum [i.e., the spectral lines], in conjunction with the background information, lends very strong support to the crucial explanatory hypothesis [i.e., the presence of the specified elements in the star's atmosphere]. 50

Thus a self-evidencing explanation would be well supported if, given the rest of the explanans and other relevant "background information," not only does "the crucial explanatory hypothesis" h entail (or render probable) the explanandum e, but e entails (or renders probable) h as well. Furthermore, it seems that a self-evidencing explanation is well supported only if this is the case; for if, given all the background information, e did not entail (or render probable) h, we would need some further evidence for h, which could only be some further true implications of h (plus background information).

We are concerned with cases in which h is a theory. The question is, then, Can a theory which entails one and only one law be rendered probable by that one law? What sort of "background information" could entail that e, the law to be explained by means of h, entails (or renders probable) the theory h? One possibility is that h is a theory of a certain type H of which there are

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50 Ibid., P. 373.

several other theories which we have already accepted, and e is a law of a certain type E of which we are familiar with several others, and all (or most) laws of type E that we know of are explained by us by means of theories of type H. We would then have inductive evidence that h is indeed the proper theory to explain e. But then it is not clear whether, or to what extent, h would be a theory explaining e, as opposed to just a part or aspect or particular application of a theory which is a generalization of the theories of type H explaining the class of laws of type E. It seems that the more theories of type H have in common and the more laws of type E have in common, the better the evidence that h is the correct explanatory theory for e; but then also the more likely it is that h will be viewed as a mere part or aspect or special application of a single, more general theory.

Another possibility is that some super-theory governing phenomena which include e states that h must be the theory to explain e. But in such a case it is even more likely than in the above that h will be considered not a theory in its own right, but only a particular application of the super-theory.

It might be suggested that a third possibility is that h display an analogy to some known laws or set of laws (that is, an analogy of the type Spector and Campbell

were talking about). For if the analogue of the hypothesis of h (i.e., the m-hypothesis) is linked with the analogue of e (i.e., the m-laws) by laws (i.e., the m-correspondence rules) formally analogous to the correspondence rules of h, then given that e is true (and since the analogue of e represents something the same as, or similar to, what is represented by e), we have an analogical argument for the truth of h. E.g., let h be the kinetic theory of gases, and let e be the gas law -- $PV=k_1T$; the m-hypothesis is a description (in the terms of classical mechanics) of a swarm of perfectly elastic point-masses filling a container; the m-law is the "law," deducible from the laws of classical mechanics, that the pressure P' exerted on the walls of the container by the point-masses is inversely proportional to the volume V' of the container and directly proportional to the height of the analogue of a column of mercury in a thermometer T'; now since the m-law -- $P'V'=k_2T'$ -- results, according to the laws of mechanics, from the m-hypothesis, is it not probable that e results in like manner from h? One might be tempted to go further and claim that what we have in this example is not just an analogical argument, but a causal argument: The only known underlying mechanism of a regularity expressible as 'PV=kT' (where 'P' represents pressure of some kind, 'V' volume, and 'T' some analogue of temperature) is a swarm of elastic particles; therefore the

regularity expressed by the gas law must be a manifestation of swarms of elastic particles constituting gases. Thus it seems that a theory T with the right kind of analogy or model -- let us call such a model an "analogical model" -- could be acceptable as an explanation of a law even if that law were the only one entailed by T. But let us examine whether this is in fact so by looking more closely at the nature of the causal and/or analogical arguments involved.

First of all, the supposed causal argument is not as straightforward as it may appear. For even if it is the case that the only other (i.e., besides gases in closed containers) systems which exhibit the relevant regularity are swarms of elastic particles, it does not seem to follow that gases too must be, appearances to the contrary, swarms of elastic particles. All that seems to follow is that gases behave just like, or as if they were, swarms of elastic particles with respect to pressure, volume, and temperature. Before we would accept the kinetic theory as a true description of the microstructure of gases and as an explanation of the gas law, we would have to have some further, independent evidence -- e.g., the discovery that marks are left on plates just outside slits in containers of gas, signifying that molecules escape through the slits. The point is that where a theory exhibiting an analogy to known laws is proposed

as an explanation of some aspect of a system originally not thought to be governed by those (or analogous) laws, there can be no straightforward causal argument for the theory. For we are not inferring, from all observed E's being C's, that this E too is a C; rather we are inferring, from all observed E's being either C_1 (swarms of particles) or C_2 (gases) that C_2 is really C_1. And it seems that we need some further evidence to make this identification.

However, this latter form of argument may be viewed as a kind of argument by analogy, which, though not as strong as a causal argument, nevertheless has some force. How much force depends on the extent of the known similarity between the phenomena to be explained and the analogous phenomena (as well as the relevance of the aspects in which the two phenomena are known to be similar to the aspects in which we want to infer that they are similar -- see above, P.196n.and correlative text). But the extent of the similarity seems to be directly proportional to the number of different laws and facts about the explanandum system which are analogous (or isomorphic) to the corresponding laws and facts (i.e., m-laws) about the analogous system; which in turn seems to be proportional to the number of different laws and facts about the explanandum system entailed by the theory. So it seems that the stronger the analogical argument for accepting a theory, the greater the extent to which the theory will explain a

number of different laws about the phenomena about which it is a theory. So even if a theory has an analogical model, it still must entail a number of different empirical laws.

These considerations indicate that every acceptable theory (except, perhaps, "theories" which are merely particular applications of larger theories) entails a number of different laws, thus supporting the contention that Hempel has shown that Campbell's argument for the necessity of an analogy is not sound. For an implicit premise in Campbell's argument is that the only relevant difference between valuable and valueless theories is the presence or absence of analogies; and Hempel has pointed out another relevant difference.

One might (if he is not afraid of non-extensionsl contexts) claim that all I have shown is that all and only acceptable theories <u>in fact</u> entail, and therefore explain, a number of different laws; but I have not shown that this variety of explananda is <u>what makes</u> a theory an acceptable explanatory one; it might still be the case that it is the analogy to known laws that renders a theory acceptable and explanatory. However, I was not arguing against this claim; all I have been trying to do, up to now, is to argue that Hempel has shown that Campbell's argument fails to prove that analogies are necessary for

the acceptability of theories because displaying an analogy is not the only way in which acceptable, truly explanatory theories differ from the "worthless" variety.

Now I will deal with the question of whether it is the analogy or the variety of explananda that renders a proposed explanatory theory acceptable. First of all, let us ask whether displaying an analogy to known laws is indeed common and peculiar to valuable theories, as Campbell apparently thought. (But see above, footnotes 41 and 44, pp.206 and 207 .) Hempel gives an example of a hypothesis plus dictionary which is intuitively unacceptable but which does display an analogy to a known law; what makes it unacceptable as a theory is, he claims, its entailing only one law.[51] Thus it seems that displaying an analogy is not peculiar to acceptable theories. Of course, this means that Hempel's example shows only that displaying an analogy is not sufficient to render a proposed theory acceptable; it does not show that it is not necessary.[52] Nevertheless, if analogy is not both necessary and sufficient for (i.e., both common and peculiar to) acceptable theories, while variety of explananda is both, then it seems more likely that it is the latter, not the former, that makes theories acceptable.

However, the analogy in Hempel's example is not of the type we have interpreted Campbell to require. (See

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[51]Hempel, Aspects of Scientific Explanation, pp.444-445.

[52]See John Losee, op. cit., P. 141.

above, pp.202-204). The analogy is a purely formal one between the equations of the hypothesis and the law -- i.e., it is simply that they are all of the form 'xy = kz'. There is nothing in the analogy to suggest the correspondence rules; it is not at all clear that the "theory," by means of the analogy, effects a "substitution of the familiar for the unfamiliar," which according to Campbell is effected by a theory's analogy[53]; there is no apparent similarity between the observational terms of the "theory" and the m-observational terms of the analogous laws. So Hempel's example fails to show that an analogy of the type required by Campbell is either not necessary of not sufficient to render a theory acceptable.[54]

It would seem that quantum mechanics is a counter-example to the proposition that all acceptable theories display analogies to known laws: "It is usually claimed that, at least on the so-called Copenhagen view, quantum theory is an example of an accepted and useful theory in which models [or analogies] have been abandoned."[55] "The fact that here the mathematical formalism may sometimes be usefully interpreted in terms of [i.e., viewed as analogous to the laws governing] waves and sometimes in terms of particles, and that these models contradict each other although the formalism is self-consistent, shows that models

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[53]See Campbell, Foundations of Science, pp. 113-114 in conjunction with the last paragraph on P. 118, and P.146. See also Campbell's What is Science?, pp. 77-78, 84.

[54]John Losee seems to have had something like this point in mind in op. cit., pp. 142-143.

[55]Mary B. Hesse, Models and Analogies in Science, P.48.

cannot be essential to the logic of the theory."[56] A "Campbellian" could answer by saying that quantum theory, far from lacking any analogy to known laws, actually displays analogies to two different sets of laws. That is, in some respects, quantum theory is analogous to the laws governing the motion of particles, in others it is analogous to the laws governing the propogation of waves; and it is just where one analogy fails that the other holds.[57] In fact, scientists often use one or the other of the analogies to suggest emendations of quantum theory and new correspondence rules.[58] Does the fact that there is no single, consistent set of laws to which quantum theory, in all cases and in all respects is analogous refute Campbell's view that every valuable theory must display an analogy? Well, according to Campbell, what an analogy does that gives value to a theory is to effect "a reduction to more familiar notions"[59] of the laws being explained by the theory. Now certainly nothing with which we are familiar partakes of the nature of both waves and particles in the way the posits of quantum theory seem to. Nevertheless, it does seem to be a reduction to the familiar to analyze the various laws entailed by quantum theory as being manifestations of processes that are in some respects like the propagation of waves

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[56] Ibid., P. 52.

[57] Ibid., P. 53. (She explains her terminology earlier in the book, pp. 8-10.)

[58] Ibid., pp. 48-52. See pp. 49-52 for examples.

[59] Campbell, Foundations of Science, P. 157. See above, footnote no. 53.

and in others like the motion of particles. So I do not think quantum theory provides a clear-cut counter-example to Campbell's thesis. (Campbell considered quantum theory as a refutation of the view, or a frustration of the hope, that all valuable theories must be analogous to some known mechanical laws -- i.e., laws governing the motion of particles. He seems also to have held that quantum theory displays no analogy at all, and that it derives all its value from its simplicity and generality.[60] But this latter view is hard to reconcile with the view that analogies are essential, which he apparently held and argued for earlier in the same chapter.)

Campbell himself discusses a class of theories which are acceptable despite their not displaying the type of analogy illustrated by that of the kinetic theory of gases. Theories of this class he calls "mathematical theories," because they are basically mathematical formulas generalizing, or extrapolating, experimental laws to cases which are not experimentally realizable.[61] Since they are just generalizations, or extrapolations, of the laws for which they are theories, there are operational, or experimental, procedures for measuring every quantity referred to in their equations; that is, every term of the hypothesis of a mathematical theory is related to observables via

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[60] Ibid., pp. 157-158.

[61] Ibid., pp. 140-144.

correspondence rules. It would seem, then, that "mathematical theories" are just empirical laws -- not really theories at all -- and that therefore their not displaying an analogy is no counter-example to the view that theories require analogies. However, Campbell argues that mathematical theories should be considered theories, not just empirical laws, since some of the numerical values which terms of the equations of the theory may take are not experimentally determinable (e.g., they may be infinitely small or infinitely large), so that some statements in the hypothesis of the theory are not linked via correspondence rules with any observable state of affairs.[62]

How does Campbell deal with the apparent counter-examples to his view that analogies are essential provided by mathematical theories? First of all, he says that these theories do display an analogy -- to the very laws which they are theories of.[63] But he admits that "analogy of the kind characteristic of the theory of gases does play a very much less important part in Fourier's Theory [Campbell's example of a mathematical theory] than in the theory of gases."[64] What gives mathematical theories their value is their simplicity.

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[62] Ibid., pp. 141-142.
[63] Ibid., pp. 142-143.
[64] Ibid., P. 143.

Of all the mathematical theories that could be constructed to entail a given law (or laws), the one most worthy of acceptance is the simplest.[65] Now providing a precise account of what simplicity is, or of degrees of simplicity, is a difficult and as yet unsolved problem.[66] Campbell apparently held that such an account is impossible because simplicity is, so to speak, in the eye of the beholder: "a proposition is simple or complex according to the effect it produces on our minds, and that effect will be the same whatever the effect on the minds of others."[67] He seems to have felt that this subjectivity of simplicity somehow vindicated him in his insistence on the necessity of analogies; for he also said that judgements of the appropriateness of an analogy are subjective, possibly varying from one person to the next. It appears, therefore, that his considered view on the question of whether analogies are necessary is that they are not; rather a theory must either display an analogy or be judged the simplest of the alternatives. He was

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[65] Ibid., P. 144.

[66] See, e.g., Nelson Goodman, Problems and Projects (Indianapolis: Bobbs-Merrill, 1972), pp. 277-278. See also Hempel, Philosophy of Natural Science, section 4.4, pp.40-45. A notable recent attempt at a formal treatment of simplicity is Elliott Sober, Simplicity (Oxford: Clarendon Press, 1975). But even a generally sympathetic reviewer, Robert Ackerman in Journal of Philosophy 74 (1977), says that "Sober depends on so many potentially controversial background assumptions that his simplicity concept is highly relativized and is likely to seem provocative rather than convincing to many readers." (P. 492).

[67] Campbell, Foundations of Science, P. 144.

concerned only to show that it is not enough for a hypothesis plus dictionary to satisfy the "logical" criterion of entailing a law; it is necessary that it satisfy some subjective criterion as well -- either by displaying an analogy which people find appropriate or by being simple in people's judgement.[68]

However, from our point of view, Campbell's considered view amounts to a complete capitulation to Hempel; for he does not, after all, claim that analogies are necessary for theories -- he admits that a theory may be valuable on account of its simplicity, even if it does not have an analogy. (He even seems to admit that the generality of a theory -- its systematically connecting a number of different laws -- can give it value in the absence of an analogy. See above, footnote number 60 and correlative text.) Besides, we have, following the lead of Marshall Spector, given objective criteria of the appropriateness of analogies (See above, pp.193-199), so that the alleged subjectivity of judgements of simplicity is no vindication at all of the view that theories require analogies.

However, we cannot let the matter rest here; for Campbell goes on to say, of mathematical theories, that in an important sense, they do not explain the laws they entail:

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[68] Ibid., P. 144.

> there is a marked difference in the explanations provided by the two types of theories [i.e., "mechanical theories," which derive their value from analogy, and mathematical theories], and this difference is usually recognised by not speaking of "explanation" in connection with theories of the second type [i.e., the mathematical]; it would not be usually said that Fourier's Theory explained the laws of thermal conduction -- that phrase would be reserved rather for the electronic theory, if it were successful -- but only that it was a generalisation of those laws. [69]

Now I think that Campbell is right in this, in the sense that mathematical theories do not provide the peculiar kind of theoretical explanation that mechanical theories (i.e., those whose value derives from an analogy) characteristically provide. A mechanical theory posits unobservable entities and processes of which the laws explained by it are exhibited as the manifestations; therefore when we accept a mechanical theory, we feel we understand why the laws in question are what they are. Mathematical theories, on the other hand, do not posit any underlying processes of which the laws in question are manifestations. One who sets out to construct a mathematical theory for a set of laws is merely trying to find the simplest expression of that set of laws. Once such a satisfactory expression is found, it may be that it applies to cases which are not experimentally realizable; and if you want to call it a "theory" on that account, fine.

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[69] Ibid., P. 146.

But it must be admitted that it explains the laws in question, if at all, in a way different from (and perhaps inferior to) that in which mechanical theories explain laws; and that the "unobservability" involved in mathematical theories, by virtue of which Campbell classifies them as theories rather than as laws, is of a different order from that involved in mechanical theories. Therefore, mathematical theories do not provide a refutation of the view that at least theories which explain laws by exhibiting them as manifestations of posited underlying processes require analogies. So it seems that -- at least for theories (such as PV) which explain by positing underlying processes of which the explanandum is a manifestation -- bearing an analogy to known laws, as well as entailing a number of different laws, is common and peculiar to acceptable explanatory theories. So we still do not know whether it is the analogy or the variety of entailed laws that makes a hypothesis-cum-dictionary a valuable theory.

Perhaps we can get a clearer picture of the role of analogies in explanatory theories by first examining the nature of explanation. Hempel, of course, is one of the foremost exponents of the view that explanation is subsumption of the explanadum under more general "covering laws" -- i.e., the view that an explanation consists of general laws (more general than the explanandum, in the sense of applying to the explanandum phenomena and to other

phenomena as well) which, along with particular or less general statements (i.e., statements of the same generality as the explanandum) entail or render probable the explanandum.[70] Hence his insistence that the explanation of a law L is effected by means of a theory T only if L is entailed or rendered probable by T and T is more general than L in that it entails or renders probable a number of other laws as well.

Campbell says that subsumption under more general principles is one kind of explanation. But he claims that the explanation of laws by theories is usually of another kind -- namely, "the substitution of the familiar for the unfamiliar." (See above, footnote number 53.) Thus, in his example of the dynamical theory of gases, we substitute the familiar laws governing the motion of particles for the less familiar gas laws.[71] However, it is possible to see substitution of the familiar for the unfamiliar of the kind effected by the dynamical theory of gases as itself a kind of subsumption of the explanandum under more general principles. For suppose a law L, governing a certain range of phenomena A, asserts that certain aspects

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[70]See, e.g., the title essay of Hempel, Aspects of Scientific Explanation, especially Chapters 1-3. The words in parentheses above represent my own specifications; I do not know whether Hempel (or anyone else) would go along with them.

[71]See Campbell, Foundations of Science, P. 146.

of A, a and b, have a certain relation R to one another -- i.e., L is 'R(a,b)'. And suppose that a theory T is proposed to explain L, where the hypothesis of T asserts a relation r to hold among certain posited theoretical entities x,y, and z -- i.e., r(x,y,z) -- and the correspondence rules (or dictionary) of T assert certain functional relationships between x,y, and z on the one hand and a and b on the other -- i.e., $f_1(x,y,z) = a$, and $f_2(x,y,z) = b$. Suppose further that there is a law L', governing another range of phenomena B, which asserts that r holds among the (observable) aspects of B c,d, and e -- i.e., L' is 'r(c,d,e)' -- and another law L", which states that the (observable) aspects of B, a' and b', which are similar respectively to a and b, have relation R to each other -- i.e., L" is 'R(a',b')'; and suppose it is also a law (or is entailed by laws) that $f_1(c,d,e) = a'$ and $f_2(c,d,e) = b'$. (I assume that if f_1 and f_2 are mathematical functions, R and r are mathematical relations, a,b,a',b',c,d, and e are measurable quantities, and 'x', 'y', and 'z' take numerical values, then it is possible for f_1, f_2, and r to figure both in the empirical laws and in the hypothesis and correspondence rules of the theory.) Obviously, L', L", and the statements connecting c,d, and e with a' and b' constitute an analogy, or model, for T of the kind that Campbell (on our interpretation) requires and that Spector claims is to be identified

with its associated theory. We therefore have reason to identify x, y, and z with c, d, and e (or at least to consider x, y, and z similar respectively to c, d, and e in respects other than and in addition to those captured by T and L'). (See above, pp.193-199.) Thus, assuming the concepts involved in L' and L" are more familiar than those involved in L, we have in T the substitution of the more for the less familiar -- i.e., what is referred to in L is at bottom just what is referred to in L' and L". But then also T extends the range of L' to cover a and b of A so that L is subsumed under the now more general (since it applies to A as well as B) L'.

Of course, we may require further evidence that what underlies L is L', or something analogous to L', before we can accept T as having effected a substitution of L' and L" for L, or the extension of the scope of L' and the subsumption of L under it. That is, we may say that T has shown only that the phenomena governed by L behave <u>as if</u> they were constituted of systems governed by L', but we would need independent evidence of the truth of T before we could accept T as an explanation of L. And of course this independent evidence would consist of laws other than L, but governing the same range of phenomena A, entailed (or rendered probable) by T (plus appropriate correspondence rules, which would probably be suggested by the analogy to the phenomena B governed by L'). But though T would then entail (or render probable) a number

of different laws about A, nevertheless it seems to be the analogy of T to L' that would make T, once accepted as true, an explanation of L; for it is the subsumption of L under the law (or implication of laws) that if r(c,d,e) then $R(f_1(c,d,e),f_2(c,d,e))$ that counts here. It is this sort of substmption under general principles via analogies that Nagel is talking about when he says that, in addition to the "heuristic" and "suggestive" role that analogies, or models, may play,

> models also contribute to the achievement of inclusive systems of explanation. A theory that is articulated in the light of a familiar model resembles in important ways the laws or theories which are assumed to hold for the model itself; and in consequence the new theory is not only assimilated to what is already familiar but can often be viewed as an extension and generalization of an older theory which had a more limited scope. [72]

But suppose we know a number of laws about A. And suppose a theory T', which posits entities and processes underlying A, entails all those known laws[73], and in addition entails a number of new laws about A, but T' does not display an analogy to any known laws (How anyone could invent such an analogy-less theory is not our concern. The question is: Given such a theory, does it explain the laws about A?), although we could construct

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[72] Structure of Science, P. 114.

[73] Or, what is more usual for theories, T' entails laws which approximate the "known laws" and which are subsequently found to accord better with experiment.

a "conceptual" model for T (i.e., a model of the kind that PVn is for PV -- See above, P.164). I think that, in the absence of any better theory for A, T' would be accepted as true and moreover would be regarded as explaining the laws of A. The explanation offered by T' would not be as satisfying as the one effected by T and its analogy to L', because the latter makes the whole system of laws governing A just one instance of a more general regularity. But though T' does not explain the whole system of laws of A, nevertheless in unifying those laws by entailing them all, it explains each law by making it one aspect of a more general principle. (The hypothesis of T' would function as the general principle under which a law of A is subsumed, and the correspondence rules needed to deduce the law from the hypothesis would function as the statements in the explanans of the level of generality of the explanandum.)

Thus it seems that, at least from the point of view of the "covering law" model of explanation, an explanatory theory can be acceptable even without an analogy, although the presence of an analogy may allow for a more dramatic subsumption of phenomena under general laws -- what might be called reduction of one set of laws to another.[74] It

[74] I have in mind Nagel's concept of reduction, in op. cit., Chapter 11. The analogy allows us to "connect" each term in the laws being explained with the corresponding term in the m-laws of the model, and to identify the theoretical posits with the corresponding model-entities. Then we can "derive" the laws from the m-hypothesis and m-correspondence rules via the connections

therefore seems that the primary requirement for an acceptable explanatory theory is that it entail a number of different laws; for meeting this requirement is both necessary in order to confirm the theory (i.e., to justify us in believing it true) and sufficient to render it explanatory of each of the entailed laws.

But before accepting this conclusion, perhaps we should discuss the arguments for the importance of models, or analogies, given by Mary Hesse[75] and Marshall Spector.[76] First of all, Hesse seems to admit that "models are not logically essential" for theories.[77] However, she contends that without a model, a theory is not "genuinely predictive,"[78] or not "predictive or falsifiable in the strong sense."[79] What she has in mind is apparently

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between the terms of the m-laws and the terms of the laws. We will thus have satisfied Nagel's logical requirements for reducing the laws in question to the laws (principally the m-hypothesis, but also the m-correspondence rules and m-laws) governing the analogical model. It may be argued that Nagel's requirements are too strict, i.e., what scientists call "reduction" rarely meet those requirements -- see, e.g., John G. Kemeny and Paul Oppenheim, "On Reduction," in Philosophical Studies (1967), reprinted in Brody (ed.), op. cit. But if they are met, as they are in cases where theories bear analogies to known laws, then it seems a reduction has been effected.

[75] In her Models and Analogies in Science.

[76] In Brody (ed.), op. cit.

[77] Hesse, op. cit., P. 54. See also pp. 24-25.

[78] Ibid., P. 5.

[79] Ibid., P. 43. See also pp. 35-42.

that a model suggests new correspondence rules for a theory, thus allowing the theory to entail more, and more diverse, laws; whereas without a model any such extension of the theory via new correspondence rules would be mostly a matter of luck, or of trial and error.[80]

Now we have seen that the number and diversity of the laws entailed by a theory are important from the point of view of both the theory's confirmation and its explanatory value. But if, as has happened[81], such extensions are made without the help of suggestive models, and the resulting set of laws entailed by the theory is rich enough, then the theory would be perfectly acceptable.[82] (We need not go into the difficult problem of how "rich" the set of entailed laws has to be -- whatever degree of richness is required, it seems that it can, at least in principle, be achieved without, as well as with, a model.)

But Hesse makes the point that only if a new correspondence rule was suggested by (analogical reasoning from) a model would empirical falsification of the implications of the theory based on that correspondence rule affect the theory as a whole; for otherwise the correspondence would have been, "introduced arbitrarily," and therefore the empirical falsification would affect only

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[80] Ibid., pp. 43-46.

[81] See Ibid., pp. 43-44.

[82] Ibid., P. 47.

the proposed new correspondence rule itself -- it would have to be replaced, or just rejected, while the rest of the theory could still stand unrefuted.[83] (I guess this is what Hesse has in mind in calling a model-less theory "not falsifiable in the strongest sense.") Now if we add to this point the premise that scientists do (and should) reject theories because of empirical falsification of implications based on proposed new correspondence rules, then it would seem to follow that a theory must have a model (of the type that suggests new correspondence rules) if it is to be the kind of theory which scientists generally deal with.

However, it seems that scientists' practice of regarding failed attempts to extend a theory by means of new correspondence rules as a refutation of the theory (to the extent that scientists do, and should, engage in this practice) can be justified without recourse to models or to analogical reasoning. For at least one aim of science is to systematize, or unify, a large number of varied phenomena within the framework of a general theory. This aim is accomplished by extending a theory originally meant to apply to one set of phenomena to other, different phenomena. If a theory cannot be so extended, if its range of application seems to be inevitably limited, then

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[83] Ibid., pp. 39-40.

scientists should reject it as not "fruitful" should a more fruitful alternative theory become available. (It should perhaps be mentioned that a limited theory may nevertheless be accepted if it is itself subsumed under a more general theory. But this would be another case where the limited "theory" is only a part of a larger theory.) A fruitful theory -- one which effects the systematic unification of a number of varied phenomena -- is perfectly acceptable, no matter how extensions of it are made or suggested.

We have already discussed Spector's argument that theories can be identified with their models. But he seems to argue also that a theory must be so identified, and that therefore an acceptable theory must have an analogical model (i.e., a model that it can be identified with -- see above, pp. 193-199 and 202-204). (Spector, adopting the terminology of Carnap's Foundations of Logic and Mathematics, expresses his conclusions thus: (1) "we can give semantical rules for theoretical terms" and (2) "theoretical terms must be given semantical rules."[84] Thus what I have been calling "identifying a theory and its model" amounts to giving semantical rules for the theoretical, or hypothetical, terms -- i.e., giving the semantical rules for the terms in the m-hypothesis of the

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[84]Brody (ed.), op. cit., P. 290.

model to the corresponding terms in the hypothesis of the theory.) His argument is basically to point out some untoward consequences of the view that the meanings of theoretical terms are given by the theory's correspondence rules rather than by its model.[85] One of these consequences is that every time we add a correspondence rule to a theory we change the meanings of the theoretical terms involved in the new correspondence rule. Thus, e.g., when new correspondence rules were added to the kinetic theory of gases to link the theoretical terms such as 'molecule' with the blackening of a sheet of film outside a slit in a wall of the container of gas in the "molecular beam experiment,"[86] the meaning of 'molecule' was changed. But intuitively it seems that the meaning of 'molecule' did not change just because of the changes involved in testing the kinetic theory by means of the molecular beam experiment. Of course, if the meaning of 'molecule' is given by the model, then our intuitions are vindicated. Another consequence of the view that the meanings of theoretical terms is determined by correspondence rules is that it "declares certain types of reasoning which are paradigms of physical genius to be (fortunate) logical mistakes of a very basic sort."[87]

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[85] See Brody (ed.), op. cit., pp. 288-289.

[86] See Ibid., P. 285, and footnote number 20. Also P.288.

[87] Ibid., P. 289.

E.g., Niels Bohr's explanation of the spectrum of hydrogen was based on considering his theory of the atom as a detailing of the kinetic theory of gases, so that such terms as 'atom' and 'mass' would have the same meaning in both theories; but since the correspondence rules of one theory differ from those of the other, the meanings of those terms should be different, thus making Bohr's arguments fallacious in that they involve equivocation. Again, if we consider the meanings of theoretical terms to be given by the model, the terms involved do have the same meaning in both theories, and Bohr's argument comes out valid. A third consequence is that

> We could not even describe the kinetic theory of gases as a reduction of thermodynamics to (statistical) dynamics. We could only notice that there is a formal similarity between some of the primitive formulas of the kinetic theory and the laws of classical dynamics. But these laws would not have the same meaning in each case. In the kinetic theory, the meanings of the terms force, mass, momentum, etc., would have to be analysed in terms of the observation terms pressure, temperature, and volume. 88

And of course, if we identify the theoretical terms with the corresponding terms of the model, as Spector suggests, then we have a reduction in that the processes underlying the behavior of gases are seen to be dynamical processes.

There are several things to be said about Spector's argument. First of all, it is not clear that denying that

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88 Ibid., P. 289.

models give meaning to theoretical terms commits one to the view that adding new correspondence rules to a theory changes the meanings of its theoretical terms. Nagel, for example, seems to hold that the meanings of theoretical terms are given primarily by the theory's hypothesis, which "implicitly defines" them. Thus he says,

> By contrast with alterations in the postulates of a theory, which in effect constitute a modification of the implicit definitions of the theoretical notions, the introduction of new correspondence rules does not change either the formal structure or the intended meaning of the theory, though new rules may enlarge the theory's range of application.[89]

However, Spector might say that despite Nagel's correct intuition, expressed above, that additions to a theory do not change meanings, nevertheless in accepting the thesis that theoretical terms cannot be explicitly defined (via models) but must be "partially" defined by the hypothesis and correspondence rules, he is committed to the denial of this intuition because every new correspondence rule counts toward the definition of the theoretical terms. Nor is it plausible to say that it is only the hypothesis, not the correspondence rules, that gives the meanings of the theoretical terms; for those terms are not correctly applicable to systems of objects to which only the hypothesis but not the correspondence rules applies (e.g., a swarm of billiard balls in a box would

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[89] Structure of Science, P. 102.

not be called "gas molecules" even if their behavior was expressible by the hypothesis of the kinetic theory of gases).

It might be suggested that we simply accept the allegedly "untoward" consequence that additions to the correspondence rules of a theory that lacks an analogical model change the meanings of the theoretical terms. It is not at all clear that our intuitions would be outraged by this result. For we can admit that theoretical terms in theories which have analogical models get their meanings from those models, so additions to the correspondence rules of such theories do not alter those meanings. And perhaps the consequence that new correspondence rules change the meanings of theoretical terms only seemed counter-intuitive because we were thinking primarily of theories of the latter sort (e.g., the kinetic theory of gases, which we had been using as an example). Besides, the changes of meaning involved in the addition of a new correspondence rule are very slight, and might be viewed more as further specifications, rather than as changes, of the meanings of the terms involved.

However, there is another approach to the meaning of theoretical terms which completely avoids the first of the "untoward consequences" (listed above) of not assigning meanings via analogical models: I have been claiming, especially with respect to PV, that the meanings of theoretical terms do not figure in the inference of observable consequences from the theory -- the same observable consequences follow if we replace every theoretical term by

an existentially quantified variable, a la Ramsey; so whatever information would be conveyed by those meanings is not confirmed by the truth of the observable consequences alone. Therefore if a theory which lacks an analogical model is expressed using theoretical terms (i.e., predicate constants), as opposed to existentially quantified variables, then we should say that we simply do not know the meanings of those terms and that the hypothesis and correspondence rules of the theory only fix their reference by telling us facts about their referents, whatever they might be. The introduction of new terms and sentences into the correspondence rules and/or hypothesis of the theory therefore in no way affects the meanings (whatever they may be) of the theoretical terms. On this approach to theoretical terms, the first of the alleged untoward consequences of denying that theoretical terms must get their meanings through analogical models is not really a consequence.

As for the second of the allegedly untoward consequences, the most Spector has shown is that reasoning such as Bohr's is valid only if the meanings of the theoretical terms is given <u>via</u> analogical models. He has not shown that every acceptable theory must allow for such reasoning. So he has not shown that every theory must have an analogical model. The example of Bohr's reasoning is embarrassing only for someone who denies

that analogical models ever give meanings to theoretical terms, not for someone who admits that models may give meaning to theoretical terms but denies that they need always do so.

Similarly for the third consequence: The kinetic theory of gases does effect a reduction of thermodynamics to statistical dynamics because of its mechanical (or dynamical) analogical model. But not every theory need effect a reduction of one science, or set of laws, to another; and so it does not follow that every acceptable theory must have an analogical model.

Thus, contrary to what Spector says[90], he has not shown that "theoretical terms must be given semantical rules" through analogical models (though I think he has shown that they can be given semantical rules in this way).

In this chapter we have construed PV as a theory, along traditional lines. We were able to distinguish in a rather straightforward way between observable and unobservable, or theoretical, entities, and to divide PV accordingly into hypothesis and correspondence rules. And PVn was seen to be a model for PV in the sense of a description, in terms all of which are understood, of a state of affairs that could be represented, in abstract

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[90]Brody (ed.), P. 290.

terms, by the hypothesis and correspondence rules of PV.

This construal of PV led to two major problems, one concerning the intelligibility of PV, the other concerning its justification, or confirmation. The problem of intelligibility was solved in the following way: We could get some idea of what is asserted by PV by thinking of PVn. And to keep our thinking abstract, or general -- so that we would not unjustifiedly reason from irrelevant aspects of PVn -- we could construct a formal system which would embody the definition of PV and which would include an explicit statement of rules of formation and transformation allowing all and only sentences and inferences that would be licit in any possible version of PV.

As for the problem of justification, we found that PVn is not a model of the type which some philosophers of science have argued that an acceptable theory must have. So we examined these philosophers' arguments, and concluded that they do not prove that such models are necessary. What is necessary for a theory to be acceptable is that it unify a number of otherwise independent, diverse phenomena by showing them all to be derivable from the same few general principles. And, as we have seen (in Chapter 3), PV does this on a large scale. Therefore we are justified in believing PV.

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