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SCIENTIFIC VIRTUES: AN INTRODUCTION TO DIACHRONIC REALISM

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SCIENTIFIC VIRTUES: AN INTRODUCTION TO DIACHRONIC REALISM

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CHAPTER I

REALISM

Introduction

Imagine that human-like beings inhabit a world similar to earth except that it is enclosed by a gigantic cube consisting of sheets of translucent cloth-like material.[1] Outside of this cube flocks of geese fly across the top from north to south and back again but only their shadows, projected on the translucent cloth, are visible to the inhabitants of this world. Furthermore, a system of mirrors is set up outside the translucent cube in such a way that these shadows are also cast upon one of the vertical walls of the cube, for example, the northern wall.[2] Finally, it is physically impossible for inhabitants of this world to penetrate the translucent sheets so that they cannot (in principle, if you like) see the geese directly, hear their squawkings, collect their feathers, etc. Anything the inhabitants conjecture about the shadows is therefore forever limited to evidence that can be gathered from within the cube.

Assuming that human-like creatures, given enough time, will almost certainly attempt to take stock of their environment in a more or less systematic way, various cosmologies will be developed that will somehow try to incorporate the visible shadows into a consistent 'cube-picture'. Perhaps the most primitive cosmologies will see at first in the irregular shadowy display the caprice of the gods, or the haphazardness of the slings and arrows of outrageous fortune. Others may interpret them as entities governed by laws and consisting of materials totally different from any known on this other world. Doubtless, over time it will be noted that there are correlations between the upper and the northern shadows and, perhaps, if the inhabitants develop telescopes, more of the detail of the shadows will be discerned. At this point, some of the more primitive capricious cosmologies are likely to lose (though perhaps not without a struggle) whatever credibility they might have once had, at least for the more informed members of the population. Still, there will be a wide range of acceptable cosmologies, any of which can continue to save the phenomena.

Among the more sophisticated inhabitants this underdetermination of current theories by the available evidence will lead to an important methodological debate. Some will maintain that the differences between rival cosmologies are merely conventional or pragmatic, that as long as the phenomena are equally saved there are no further facts of the matter to which one can appeal. Any system that mathematically accounts for the data will be as good as any other. Choices of which system to use will be based on questions of convenience, personal or social taste, or, in short, sociological and psychological considerations together with, perhaps, a more or less objective standard of computational ease. It may even be argued that this is all that a'science' of the shadows is or should be, and a cubical Ptolemy may pick and choose at will from among the various mathematically adequate systems without feeling that he (or she) has violated any physical principles.

Others will not be satisfied with this sort of account of science and will attempt to provide explanations as to the nature of the shadows, what causes their movements, and so forth. Crystalline cubes, invisible strings, and various other theoretical models will be advanced as the 'true' or 'real' account of the observed phenomena. This group will likely criticize the other for surrendering the quest for truth for purely formal, descriptive correlations. In turn they will be labeled as 'armchair metaphysicians', criticized for their inability to move beyond visual or imaginable aids in their account of the phenomena.

Trying to decide at this point which group was more rational or more scientific would be to jump into the traditional debates between scientific realists and various forms of anti-realism. This would be to take a stand on what science is and should be, and which of the above approaches is more representative and conducive of whatever these are taken to be. It would also be to assume that these issues can be decided once and for all, for all theories. Roughly, a decision could be made to support the first group and claim that if descriptive and predictive success and relative ease of computation does not decide between alternative accounts, there is no rational choice between them. Or it could be claimed with the second group that predictive and descriptive success should be (somehow) correlated with questions concerning the truth of the accounts. Is there an objective way to choose between these two views?

At the stage we left our cubical science, I think the more rational view would be to side with the anti-realist approach. All that the available evidence supports is that there is a correlation between the two visible sets of shadows. Whether there are two distinct realms, crystalline cubes, invisible strings, 'epicubes', or whatever, is, at present, totally underdetermined by the available evidence. Arguing for one theory over another when this is the case seems to be less rational than remaining neutral as to the truth of the accounts and stressing their relative usefulness, computational ease, etc.

Rather than leaving the story at this point, however, it may be instructive to provide more and varied information for the cubical scientists. If, for example, there are also geese within the cube, similarities between their observed behavior and the behavior of the shadows may eventually be noticed. Scientific developments in other fields also may begin to separate the competing theories into more and less plausible alternatives. Advances in optics, for example, might enable the scientists to determine that the observed phenomena <u>are</u> shadows, rather than aethereal substances. Subsequently, theories that had not utilized entities beyond the cube, or worse, had argued against them, would be in serious trouble.

At this point, one would expect a great deal of new interest and research concerning what the external entities are like. There would also be more impetus to link information provided by separate fields when this is available, so that advances in each one may throw light on problems within the others. If, for example, certain types of biological regularities become established concerning cubical geese, the observed similarities between them and the shadows can be increased or decreased. The scientists begin to expect more and more effects to be predictable from the various remaining theories. Each one becomes committed to more and more as other regularities are established or refuted. It does not seem implausible that enough convergence between and among various theories and the available evidence could lead to the acceptance of the theory that the visible shadows are caused by the very situation described at the beginning of the above parable.

Perhaps enough had been learned about biological entities, and birds in particular, that it began to look very probable that the shadows were caused by a particular kind of entity. Similarly, the shape of and the correlation between the shadows might eventually point to the existence of mirrors, and so forth. While the vagueness of this fanciful development of cubical science undoubtedly stacks the deck, I hope that it at least shows the plausibility of the view that a realist position might become more rational as evidence accumulates for and against various theories. At a certain conceivable level of development (detailed commitments being borne out, a great deal of convergence among theories in different fields, etc.) the level of confidence in the truth of a particular theory can rationally increase.

If this is so, it appears that one should not interpret realist/anti-realist debates as atemporal or as global, but rather as being about particular theories at particular stages in their development.[3] Furthermore, the relative rationality of each position can change as the ratio between the evidence for a theory and the empirical and conceptual problems within this theory fluctuates.[4] The purpose of this dissertation is not only to argue that this diachronic approach to realist/anti-realist debates is superior to the traditional synchronically interpreted dichotomy, but also to argue that using such an approach can support a particular form of scientific realism, at least for some theories, at particular stages of their development. This will also involve, of course, an elucidation of what this form of realism amounts to, what kind (as well as what amount) of evidence can support it, and enough historical data to help make the resulting position plausible. The remainder of this work will be devoted to providing such an elucidation and defense.

Some Types of Scientific Realism

There are, of course, many different versions of scientific realism, as there are many different versions of anti-realism. One of the more traditional controversies, beginning with debates between Plato and Aristotle, concerns the referential status of abstract terms such as properties and relational terms. In medieval parlance part of this debate focused on the problem of the existence of universals. As such, it finds part of its current formulation in the widespread debates concerning the reality of numbers, or whether 'physical magnitude' terms such as 'length' are best understood as functions which map objects into real numbers, or as properties of these objects (or, in Kyburg's happy phrase, the distinction between 'length' being a property of an object or "the result of doing something to the object").[5] Also relevant, of course, are some of the recent debates concerning whether or not to quantify over (or be ontologically committed to) classes or sets, physical properties, modalities (such as necessity) and so forth.[6] Despite their long philosophical history and their importance for philosophy of science (particularly as some of the above mentioned issues relate to the problems of measurement and the reference of physical magnitude terms), however, these debates are not the ones that most often occur among realist and anti-realist philosophers and historians, nor are they the ones that are usually found in historical cases of disagreement among scientists.

Similarly, though scientific realist claims are sometimes formulated in terms of whether statements containing theoretical terms have truth values (as opposed to, for example, being mere 'inference tickets', or being reducible to non-theoretical, observational statements),[7] this is also not the usual focus in historical cases of realist/anti-realist debates. It has long been maintained that the 'meanings' of theoretical statements are not reducible to the meanings of observational statements.[8] Furthermore, important current anti-realist positions, such as that of Bas C. Van Fraassen (discussed at length in chapter three of this work), are no longer committed to a strict observational/theoretical distinction, nor to the view that the empirical meaningfulness of theoretical claims is totally contained in the observational consequences of a theory. In fact, much of the motivation behind recent realist positions is the utter failure of early positivist 'reductionist' programs to succeed in showing that theoretical statements do not have independent meaning and scientific (especially explanatory) importance.[9]

Finally, while the claim that scientific theories (or the conjunction of statements within a scientific theory) <u>have</u> truth values may once have been considered important, it has never been the main focus of actual realist/anti-realist debates in science. Rather, the focal point of historical cases of disagreement has been over whether there was sufficient evidence available at the time to justify postulating 'gravity', or 'atoms', or 'electrons', or 'phlogiston', i.e., whether scientists working in the relevant field could be warranted in believing in such theoretical entities. Consequently, whether theories or their component parts have truth values (whether we can discover them or not) misses this important epistemological component of whether the evidence warrants assigning them the value 'true' or 'approximately true'. Similarly, I will argue in chapter two that the recent interest in formulating theories of reference for theoretical terms is, while a necessary ingredient for realist positions, also beside the point as far as this central epistemic component is concerned. Briefly, even if a theory of reference succeeds in accounting for how theoretical terms can be referential, whether particular ones are cannot be decided without considering the evidential support for these terms available at a particular time for a particular theory.

Much more relevant and widespread (given the tendency to adopt holistic interpretations of scientific theories in the aftermath of positivist observational-reductionist frenzy)[10] is what I will call a pragmatic approach to realism. In such an approach one often finds the realist claim that some theoretical terms have the same existential status and support as medium-sized physical objects linked with the view that both 'molecules' and 'chairs' are postulated to make sense of our total experience. W.V.O. Quine, perhaps the most familiar adherent to such a view, expresses this claim as follows.

> Considered relative to our surface irritations, which exhaust our clues to an external world, the molecules and their extraordinary ilk are thus much on a par with the most ordinary physical objects. The positing of those extraordinary things is just a vivid analogue of the positing or acknowledging of ordinary things.[11]

What places both ordinary objects and theoretical entities 'on a par' is the desideratum that our overall theory (the set of all our beliefs from the 'sitability' of chairs to the complementarity of electrons) remain as consistent and as simple as possible, while being able to successfully guide our behavior in the sense of enabling us to make accurate predictions concerning future experience.[12] Unlike the realist positions discussed above, this view does emphasize the relation between theoretical (and ordinary) objects and the evidence which is used to support belief in them. Also, unlike these other forms of realism, the pragmatic approach (or something like it) recently has become quite widespread in philosophy of science, and does (or may) have direct relevance for interpreting historical cases of realist/anti-realist debates. For all of these reasons, I will consider it in some detail before I proceed to develop the type of scientific realism that I will be defending in this work.

First of all, the pragmatic approach does represent a type of scientific realism, both in the sense that it emphasizes the evidential support for postulated entities, and because it does not interpret the 'existence' of theoretical entities differently from that of ordinary physical objects.[13] Still, there is something substantially indeterminate about pragmatic realism in that it places too much emphasis on epistemological concerns. Quine often claims that when predictions fail what we are to change in our total system of beliefs to correct the resulting tension is largely underdetermined. He often uses an analogy (which he attributes to Neurath) of attempting to repair a leaking boat while at sea.

> Our boat stays afloat because at each alteration we keep the bulk of it intact as a going concern. Our words continue to make passable sense because of continuity of change of theory: we warp usage gradually enough to avoid rupture.[14]

What we change to bring recalcitrant experiences into our system depends on what would do the least overall damage to our existing system. This depends, of course, on all of our beliefs, as well as upon all of the varied interconnections between them (or at least upon all of some subset of our belief system, since finding that some swans are black need not create tension within axiomatic set theory or quantum mechanics). This is why Quine's position can be classified as holistic, and why such a position cannot adequately assess what precisely to blame when predictions go awry, nor what precisely to praise when they are successful. Since scientists have almost always behaved as if they could single out particular parts of a theory to modify or abandon when predictions fail, holistic interpretations of scientific theories cannot represent how scientists interpret scientific practice.[15]

The situation is worse for pragmatic realism. Quine has repeatedly argued that our ontological commitments (often put in terms of what we should be willing to quantify over) should be based on our best physical theories, and that new commitments should be considered in terms of whether they add to the scope and accuracy of existing theory.[16] As an <u>epistemic</u> guideline, this represents sound advice. On the other hand, from Quine's pragmatic-holistic perspective, this claim also takes on ontological trappings, concerning not only what we have warrant to believe there is, but also concerning what there is. It is one thing to insist that our only basis for believing or disbelieving in theoretical claims involves their relation to available evidence, and quite another to claim that this evidential support is all there is to claims of existence or truth. To decide on the existence of, for example, electrons because of the ability of the physical theories that postulate them to organize our experience and make predictions about our future experience is a different matter from claiming that this is what existence claims amount to (or that this is all we can legitimately _mean_ when we say that statements involving electrons are true).

Embracing the latter is to slip into one or another form of verificationism, which has already been battered and bruised sufficiently to eliminate the necessity of attacking it again in this work. Quine's position is, of course, more subtle than some earlier forms of verificationism, and was in fact formulated partially in response to early positivist accounts. Still, interpreting notions of 'truth' or 'existence' in terms of some system of beliefs (or according to 'our best physical theories') again amounts to identifying evidential and ontological concerns (even though this identification may be less direct in pragmatic realism than in earlier forms of verificationism). How we _connect_ theoretical claims to 'physical reality' is a difficult and as yet unanswered (to everyone's satisfaction at least) question. Still, it is this connection that is operative in our most widespread notions of 'truth' and 'existence'. Consequently, reducing physical reality to well supported observational and theoretical claims abandons the usual notion of objective truth rather than elucidating it. The evidence I will provide if questioned about my ontological commitments is simply not what these commitments commit me _to_ (i.e., that there _is_ a connection between my statements and something (perhaps vaguely) referred to as 'physical reality').

Recently claims about the connection between our statements and a theory-independent external reality have been widely attacked as 'metaphysical' by a number of philosophers (some of them realists) who are interested in philosophy of science. Primarily, these attacks have been based on an interpretation of Tarski's definition of 'truth' for formalized languages. It is claimed that it is a consequence of Tarski's approach that truth (even a 'correspondence' theory of truth) is to be formulated <u>within</u> a theory, and that the connection between terms (or statements) and 'things' is to be understood internally in terms of the formal notion of 'satisfaction'. Hilary Putnam has popularizes this approach in his presidential address to the Eastern meeting of the American Philosophical Association in 1976.[17] While Putnam's approach is in many respects different from Quine's, and Putnam would undoubtedly resist the label of 'pragmatic realism', I will try to show that the position he outlined in "Realism and Reason" is committed to the same muddling of epistemological and ontological concerns outlined above. Putnam considers his realism to be many sided, and supported in different ways by a number of his works.[18] Still, his Tarskian attacks on what he calls 'metaphysical realism' (MR) can be initially separated from other aspects of his overall realist position. Consequently, I will here focus primarily on "Realism and Reason", and consider his general epistemic account of realism in chapter four, and his theory of the reference of theoretical terms in chapter two.

Putnam formulates the essential ingredients of MR as follows.

> The most important consequence of metaphysical realism is that <u>truth</u> is supposed to be <u>radically non-epistemic</u>--we might be 'brains in

> a vat' and so the theory that is 'ideal' from the point of view of operational utility, inner beauty and elegance, 'plausibility', simplicity, 'conservatism', etc., might be false. 'Verified' (in any operational sense) does not imply 'true', on the metaphysical realist picture, even in the ideal limit.[19]

This certainly depicts the notion that 'truth' and 'existence' depend in a crucial way on some connection between statements and an independent physical reality that I opted for above. Putnam claims that this view is not simply wrong or naive, but incoherent. This commits him to something stronger than holding that empirical evidence does not support MR, or that the 'connection' between words and independent reality that it is based on is not sufficiently articulated. Claims that a position is 'incoherent', like claims that a statement is 'meaningless', imply that there is either some out and out contradiction within the position, or at least that it systematically misuses central terms (in this case, 'truth', 'existence', or 'connection'). Thus, though Putnam explicitly rejects a verificationist theory of meaning, it is precisely over meanings that he disagrees with MR positions. I have already maintained that what most of us mean when we claim that, for example, electrons carry a single negative unit charge of electricity, is that there is a correspondence between what we say and the way electrons 'really' behave (independently of our best theory concerning them, which may be false). I think that this is the common informed view of 'truth', 'existence', and the like, so that the burden of proof is almost certainly on Putnam. How, then, does he try to justify his claim that MR is 'incoherent'?

He begins his attack on MR by appealing to an imaginary ideal theory that he characterizes as follows.

> Let T_1 be an ideal theory, by our lights. Lifting restrictions on

> our actual all-too-finite powers, we can imagine T_1 to have every property <u>except</u> <u>objective</u> <u>truth</u>--which is left open--that we like. E.g., T_1 can be imagined complete, consistent, to predict correctly all observational sentences (as far as we can tell), to meet whatever 'operational constraints' there are (if these are 'fuzzy', let T_1 seem <u>clearly</u> to meet them), to be 'beautiful', 'simple', 'plausible', etc. The supposition under consideration is that T_1 might be all of this <u>and</u> <u>still</u> <u>be</u> (in reality) <u>false</u>.[20]

It is this claim of MR that Putnam argues is incoherent, for reasons I will soon provide. At this point, however, it will be worthwhile to elucidate what might be incoherent about such a claim, and how this shows that Putnam also muddles epistemological and ontological considerations. Before his formal objections are articulated, we can become intuitively clearer concerning what his motivations are for claiming that MR positions are incoherent by trying to formulate some incoherence claims that might be generated from the above quote.

First, it might be argued that, given such an ideal theory, we should surely believe in its truth (if we are willing to believe in anything). This would amount to a claim that this model of an ideal theory completely satisfies all we could possibly mean by evidence warranting belief in a theory. Hence, if MR claims that there is something more or different involved in <u>justifying</u> theoretical claims, it would be 'incoherent' because of some misuse of the term 'justification' (or 'warrant', or 'evidence'). I agree. In fact, a large part of chapter three will be devoted to arguing that when a theory achieves a certain level of 'virtues' (to be elucidated later—roughly corresponding to some of the 'operational constraints' that Putnam lists), the most appropriate epistemic attitude towards it (or the conjunction of its theoretical claims) would be belief in its truth. Still, MR certainly need not be committed to this type of incoherence, and I know of no actual MR positions that would deny that, as far as justification is concerned, the ideal theory postulated by Putnam could serve as an exemplar.

'Justification', however, is an epistemic concept, concerned with our right to claim to know or believe certain statements. As such, whether or not such epistemic attitudes can be said to be justified does not, in itself, say anything about whether such warranted beliefs are true. I think, for example, that Aristotle was justified, given the evidence available to him, in believing (or even claiming to know) that the earth is stationary at the center of our solar system. Still, of course, he was wrong, and later evidence began to accumulate against his belief so that we can be said to justifiably believe (or know, if you like) that the earth is not stationary at the center of our solar system. Granted, this example does not approach the evidence converging on Putnam's ideal theory (though I will later argue that cases like that of Aristotle do pose problems for Putnam's position). Nevertheless, even in the ideal case, the theory could be wrong. I would argue that this would approach the miraculous (for reasons to be given in chapter three) and that we could have no explanation of how a theory could be so incredibly confirmed and yet be false. Still, if truth is indeed linked with the notion of some connection between statements and an independent reality, it is not impossible that a theory that is 'super confirmed' could be false. We would have no reason to believe that it was false, and no way of ever finding out that it was false (assuming that no new evidence indicates otherwise), but, alas, we and our theories do not have the final say in such matters.

Second (and this is what Putnam clearly intends), it might be argued that the incoherence within MR is precisely this notion that there is something more to 'truth' than what is contained in super-confirmed statements. This would be to deny that our notion of 'truth' entails any commitment to a connection with some independent reality (i.e., to deny that 'truth' is a non-epistemic notion). I think, as I've said, that this is just false. Even if articulating what this connection is between words and things is difficult (or even impossible), it is not incoherent (though it may be naive or unjustified) to postulate such a connection. Denying that truth is non-epistemic is, I think, precisely to argue that completely confirmed statements are what we mean by 'truth', i.e., to adopt a form of verificationism.[21] This does not, in itself, show that verificationist claims are wrong, but it does show that MR is only incoherent if we treat 'meaning' (at least of terms such as 'truth' and 'existence') in a verificationist manner. Incoherence from-the-perspective-of-verificationism is not incoherence per se. A theory of meaning has slipped into the argument, and, in fact, one that some eighty years of philosophical argumentation give us good reasons to reject. Intuitively, then, I think that Putnam's claims for the incoherence of MR are suspect. Now we need to consider the arguments he actually gave for this claim to ensure that important questions haven't been begged or ignored.

Putnam's actual argument for the incoherence of MR positions is, as I've said, closely linked to model-theoretic considerations tied to Tarski's definition of 'truth' for formalized languages. The following is the main passage in which these model-theoretic considerations are supposed to show that theory-independent notions of truth are incoherent.

> I assume that THE WORLD has (or can be broken into) infinitely many pieces. I also assume T_1 says there are infinitely many things (so in this respect T_1 is 'objectively right' about THE WORLD). Now T_1 is consistent (by hypothesis) and has (only) infinite models. So by the completeness theorem (in its model-theoretic form), T_1 has a model of every infinite cardinality. Pick a model M of the same cardinality as THE WORLD. Map the individuals of M one-to-one into pieces of THE WORLD, and use the mapping to define relations of M directly in THE WORLD. The result is a satisfaction relation SAT—a 'correspondence' between terms of L and the sets of pieces of THE WORLD—such that the theory T_1 comes out true—true of THE WORLD—provided we just interpret 'true' as TRUE(SAT). So what becomes of the claim that even the ideal theory T_1 might really be false?[22]

This is a nice piece of argumentation, based on a widely accepted formal treatment of truth, and on a well-defined notion of 'satisfaction'.[23] Whether it is compelling as opposed to being sophisticated legerdemain remains to be seen.

First, there are some minor problems involved in Putnam's account if it is to be related to a (even ideal) physical theory. While it may be the case that physical theories will someday be capable of formulation within some artificial language, it is at least controversial whether such a program will ever be carried out. If it is not carried out, it is not clear that Tarski's theory can be applied to such a theory. Tarski consistently maintained that his definition of truth was not meant to apply to natural (or any nonformal) languages, both because of the relative vagueness of nonformal languages, and because of certain persistent paradoxes (notably that of the 'liar') if such languages are used.[24] As I said, this is probably a minor problem, both because some ideal scientific theory may be adequately formalized someday, and (more importantly) because there have been attempts to extend Tarski's treatment to nonformal languages.[25] Also, even if the above problem can be ignored, it is not clear that 'incoherence' results from a MR position given Putnam's above account because, as I've claimed, 'incoherence' seems to be a claim about meanings, and Tarski's 'Convention T' is only meant to produce extensionally equivalent statements.[26] Even if (and this is also controversial, as we'll see) the mapping of M into THE WORLD produces TRUE(SAT) statements that are extensionally equivalent to MR true-of-an-external-reality statements, it is still not incoherent to claim that something is missing in M concerning our ordinary notion of 'true'. This point too may be a minor one, but an extension of it is not.

Second, G.H. Merrill has argued that an extension of this last minor point shows that Putnam's construal of TRUE(SAT) does not render MR positions incoherent.[27] The crux of Putnam's claim is that the one-to-one mapping of M into THE WORLD renders the MR 'true' and 'TRUE(SAT) in M' equivalent. What then is 'left over' in the MR concept of truth? Merrill argues that Putnam's argument against MR is a non sequitur in that it only applies to a MR position that is committed to the claim that THE WORLD consists of a set of objects (and hence the mapping of M into THE WORLD would be complete).[28] As Merrill points out, it is dubious whether any MR position would accept such a construal.

> Are there any realists who maintain just this position? This is dubious, for realists typically hold that not only are there objectively existing entities (both observable and unobservable) in the world, but also that these entities bear to one another certain objectual relations. And according to this latter view the world must be represented not simply as a set, but as a set together with a class of relations among the members of that set.[29]

The remainder of Merrill's article consists in constructing a model involving what he calls a 'structured domain' (including the relations between the objects) and showing that, given such a model, MR claims do not come out incoherent on a Tarskian construal (though, for Merrill, MR claims are still _false_). This shows, if it is successful, that Putnam's arguments do not formally justify his desired results. Briefly, if Merrill is right, Putnam has attacked a straw man.

Finally, there are also less formal problems with Putnam's claim that MR is incoherent besides the ones I've already presented.[30] First, a formal consideration renders Putnam's argument much too strong for his purposes. Though Putnam's claim is that it is incoherent to maintain that the _ideal_ theory T_1 can be 'super-justified' and yet be false, such a claim can similarly be applied to non-ideal theories that are (or once were) rationally acceptable.[31] The moral of this is that, for example, Aristotle's belief that the earth was stationary at the center of our solar system (or Newton's theory, or phlogiston theory, etc.) will come out TRUE(SAT) for much the same reasons that the model M of T_1 does, which would turn Putnam's account into a version of relativism. Or, if one chooses to restructure TRUE(SAT) so that _only_ 'ideal' theories would qualify (for example, one might argue that no non-ideal theory is _really_ rationally acceptable, or some such), then "there is scarce any difference between Putnam's unmetaphysical realist and the garden-variety sceptic."[32]

Second, and more generally, all of Putnam's arguments for the truth (as opposed to the justification of believes-true claims) of T_1 with M as a model are of the 'what else?' variety. "What else could be required for the truth of the ideal theory than that there be an interpretation which makes it true in the actual world?"[33] Not operational constraints, for they can be assumed to be met in T_1.[34] Furthermore, according to Putnam, it would be illegitimate to claim that it is not enough for the truth of a theory that one of its models can be mapped into THE WORLD, but that it must also be an 'intended' interpretation. How do such 'intentions' link words with things in a manner that is different from the mapping of M into THE WORLD?[35] Causal theories of reference, intended to replace the vague 'intention' talk with real causal relations between words and objects (consisting, perhaps, of physical, social, and psychological circumstances) will not help, according to Putnam, since these theories are still unarticulated concerning what reference is. In short, for Putnam, such theories label the already mysterious relation formulated by MR positions, but do nothing to demystify it.[36] This amounts to a kind of circularity, that "in order to say anything which is informative about what fixes reference, the reference of some of our words and phrases must already be fixed."[37] And, if such a circularity is vicious, it would seem to undermine MR claims.

There are at least two things wrong with this. First, there is no reason to think that the above circularity is vicious unless causal theories of reference are understood as definitions of 'reference.' Rather, they should be viewed as "natural explanations" of aspects of reference,[38] and, as such, the circularity is no more vicious than the familiar belief-reference circularity in radical translation (roughly, we need to know what the native believes to fix the reference of her utterances, and we need to know what the references of her utterances are to figure out what she believes). Second, and more importantly, causal and other accounts of reference, since they are at least partially concerned with explaining the success of linguistic communities by linking these communities' linguistic practices to their (independent) physical environment, are not analyses of the term 'refers'. As I claimed earlier, arguing that the connection between words and things postulated by MR is unarticulated is not the same as arguing that this postulation is incoherent. Again, MR positions may be false or naive (but, if so, a different type of argument must be leveled against them), but that is a different matter from their being incoherent. Given my earlier remarks against Putnam's 'what else?' strategy, plus the arguments of Merrill and Glymour, it seems that the alleged incoherence of MR positions is a *non sequitur*, both formally and intuitively.

Realism and Explanation

One may wonder why so much space has been devoted to criticizing Putnam's attack on MR. First, such a move has become widely accepted, and much effort has been expended recently trying to avoid the 'pitfalls' of MR.[39] Second, and more importantly, a large part of the overall realist position I am going to defend depends on some sort of MR being acceptable. The basic formulation of diachronic realism will be presented in the next section of this chapter. This basic formulation will look like several familiar versions of scientific realism, with the addition of a few qualifications and addenda. It shares with most realist positions the basic intuition that scientific realism offers the best (if not the only) *explanation* of the overall success of science. It will be seen in chapter four, however, that this intuition faces serious difficulties if it is accepted at face value. Briefly, it has recently been argued that 'successful' theories need not be true, and that theories that can be said to have successfully referred and to have been at least 'approximately true' were not always the most successful theories available at any given time. Consequently, the notions of 'truthlikeness', 'benign chauvinism', and 'being on the right track' will later be introduced as addenda to diachronic realism to help counter this objection. The crux of the matter is that these notions, as well as the success of the general intuition that realism explains scientific success, depend on some sort of MR interpretation of 'true', and, hence, the necessity of attempting to refute Putnam's objections to MR in the preceeding section. At present it will suffice to introduce part of my general strategy, and to consider a different objection to realism as explanation, reserving my main line of argumentation until chapter four.

One of the notions I will further elucidate is that of 'benign chauvinism'. Part of this concept (the 'chauvinism' part) consists simply in asserting the truism that we must <u>judge</u> the truth or falsity of theories or theoretical claims, the reference or non-reference of theoretical terms, and the 'success' of past theories in terms of the evidence now available to <u>us</u> (as is often noted, what else besides our criteria and our evidence <u>can</u> we use?). Hence, Aristotle, for example, was right to believe that the earth was stationary at the center of our solar system given his evidence, but nevertheless, given our information, this belief was wrong. Such a claim depends on there being a separation between epistemological (Aristotle was justified in <u>believing</u> x) and ontological (Aristotle was nevertheless <u>wrong</u>) concerns. Furthermore, the 'benign' part of this chauvinism consists partially in the ability to claim that Aristotle and other past theorists were justified in making their claims, in spite of the fact that they were wrong. I agree with

Glymour that a simple extension of Putnam's argument concerning the truth of the ideal theory T_1 would render such past successful theories (or rationally acceptable theories) 'true' as well. The resulting relativism would render either 'chauvinism' bankrupt (Aristotle's claim was true, given Aristotle's 'conceptual scheme')at the expense of a (or so I've maintained) philosophically respectable notion of truth, or the 'benign' part bankrupt (possibly in the form of 'incommensurability' arguments such as "we can't understand, or accept, or appreciate any part of Aristotle's theory, since we are invariably locked in our own different perspective"--so that Aristotle's claims were not even rational, by our lights)at the expense of historical charity and intelligibility. 'Truthlikeness' and 'being on the right track' also depend in crucial ways on the theory-independence of truth, and the resulting epistemological/ontological distinction. This brief introduction to 'benign chauvinism' should suffice for the moment to show why I belabored my critique of Putnam's 'unmetaphysical realism'.

While most of my arguments in defense of realism as explanation will be presented later, there is an independent, general type of defense of this notion that can be presented now. The belief that realist positions explain scientific success has become a cornerstone of virtually all recent realist positions, including Putnam's. The basic intuition is that the best way to account for the success of, for example, atomic theory (consisting, in part, of its crucial role in a number of different sciences, its predictive and explanatory power, its developing more than one way to measure or detect subatomic particles, etc.) is by assuming that there are atoms (or something very much like atoms).

The alternative seems to be to swallow the existence of more or less incredible coincidences in the form of "while there aren't atoms or anything like them, our overall experience is such that the world operates _as if_ there were", or some such. Roughly, it does not seem likely that the postulation of imaginary or arbitrary entities would enable us to achieve the very impressive convergence of evidence for a successful theory that many current scientific theories (and some past theories as well) enjoy. How else are we to account for this _level_ of success (barring Berkeley's God, or Descartes' possibly too clever evil genius)?

In response many anti-realists (and some, notably pragmatic, realists) have claimed that the increasing success of scientific theories can be explained in some sort of evolutionary manner.[40] Most of these accounts, however, remain at best explanatory sketches, as if the details of the evolutionary account were familiar enough to render attempts to elucidate it unnecessary. The most articulated evolutionary account that I know of (though it is primarily concerned with explaining the evolution of ideas, while other evolutionary accounts concern themselves with cultural survival elements) is provided by Pierre Duhem in _The Aim and Structure of Physical Theory_.[41] Consequently, I will briefly consider Duhem's formulation in order to determine whether evolutionary accounts actually provide a rival explanation for the success of science.

As is well known, Duhem's general position regarding the cognitive status of theories and theoretical statements is that they are not assumptions concerning the 'real' nature of material things, but rather that they "have as their sole aim the economical condensation and classification of experimental laws."[42] This means that the theorist enjoys considerable freedom in the formulation of her theories, constrained only by the experimental laws that she is attempting to condense and classify, and by "the opinions of men" (past theories, the prevailing methodological and theoretical framework, etc.). Still, a successful and impressive theory such as Newton's theory of universal gravitation does not spring forth as "the sudden product of a creation" (which would seem miraculous) but is rather "the slow and progressive result of an evolution" (which seems at least less miraculous).[43] Duhem then proceeds to outline the earlier attempts to deal with the physical problem of 'weight', from Aristotle through theories of astrologers and physicians to the systems of Galileo and Newton.[44] What results is a plausible view that we can understand the complexity and high level of success of Newton's theory as a result of a slow evolution roughly spanning two thousand years of attempts to condense and classify experimental and experiential regularities. Given this slow evolution of theoretical systems, converging finally in Newton's theory of universal gravitation, the otherwise implausible view that Newton 'chanced' upon this theory that just 'happened' to work can be somewhat mollified.

From Duhem's perspective, all of the aspects of Newton's theory were available for him to synthesize, and all of these aspects were formulated by previous, less successful, attempts to condense and classify experimental laws that can be seen as representing nearer and nearer 'misses' (not, of course, a la realism, as near misses towards the 'true' account, but rather as near misses towards a more empirically adequate account). If it can be conceded that Aristotle's system was hardly a miraculous scientific achievement, but was rather a more or less common-sense theory concerning the then relatively straightforward experiential 'facts', then showing that Newton's system developed gradually (a piece at a time) from Aristotle's and other relatively simple systems removes the 'miraculous coincidence' aspect of universal gravitation without postulating its 'truth'. In short, Newton's theory was as successful as it was because it represented the gradual synthesis of previously less successful theories. One need not, therefore, be committed to theories approaching the truth in order to account for their increasing success.

Such a view seems quite plausible, and I have no doubt that something like the above account may well explain the formation of successful theories. Still, I maintain, their success has not thereby been explained. Indeed, the fact that Einstein could formulate the special theory of relativity may presuppose that earlier non-relativistic theories at least helped to pose the problems that Einstein addressed. Still, why should any of them have worked as well as they did, if they were not at least partially 'right'? Some evolutionary theories (such as Van Fraassen's) would claim that such theories need only be 'right' about the experimental laws that they condense and classify (or even, for Van Fraassen, 'explain').[45] For some theories (such as phlogiston theory), I will argue later that their ability to organize otherwise unrelated data is all that can be claimed to be 'right' about them. I do not find this plausible, however, for all theories, especially ones that enjoy a particularly high convergence of different types of evidence (which phlogiston theory, concentric sphere astronomical theories, etc., do not). How does the postulation of, for example, 'electrons' account for so much experimental data, and in such a relatively simple and determinate manner, unless there are electrons (or something very much like them)? For a realist this is accounted for by accepting electrons as real entities, some of whose properties we have discovered. For an anti-realist, I maintain, it does not appear that this level of success can be explained at all.

Briefly, an evolutionary anti-realist must claim that success is to be expected given the (evolutionary) necessity that many of our low-level generalizations must be right in order for us to survive (examples: I will fall if I step off of a cliff, bears and armed robbers are dangerous, etc.), and the fact that theories are postulated to aid us in such experiential expectations. From this (if it is correct) it only follows that our overall belief system must be at least largely successful on the pain of our extinction as a species. If one adds to this Duhem's account of the piecemeal evolution of theoretical systems, there may be some sense in which even complicated abstract theoretical systems would also have to be successful (on the pain of their extinction as fruitful coordinating and classifying systems). Still what makes them successful is not included in such an account, while for the realist what makes them successful is their approximation to the truth. I therefore maintain that the best explanation for the success of scientific theories is that the theories themselves (or the conjunction of their theoretical claims) are largely right, and not just concerning the experimental regularities that they classify and predict. In any event, an account of how false or neutral theories could enjoy such an impressive convergence of evidence is not yet provided by evolutionary anti-realists. Consequently, it seems that the realist account of this success is still the only one available. Further elaboration and defense of this brief sketch will, again, be provided in chapter four, when some detailed anti-realist arguments against this claim are considered.

Diachronic Realism

The previous two sections have provided some information concerning what diachronic realism is not, as well as a few hints as to what it is. In partial summary, one can claim that diachronic realism addresses the relation between evidence and theoretical claims in the sense that certain levels of evidential confirmation warrant belief in some theoretical claims. Furthermore, diachronic realism is not committed to the view that all debates between realists and anti-realists should be decided in favor of realism. Briefly, since the evidential support for a theory can change, so does whether or not we are warranted in believing the claims of a particular theory in light of this evidential support. Finally, diachronic realism is committed to some form of what has been termed 'metaphysical realism', and thus is committed to a distinction between: (1) epistemological concerns such as whether, given the available evidence, it would be rational to believe the claims of a particular theory; and (2) ontological concerns such as whether or not the theory is (evidence and theory-independently) true. I will briefly elaborate this notion of diachronic realism in order to separate it from some otherwise similar realist formulations.

As I claimed earlier, the usual focus in historical cases of realist/anti-realist debates in science is whether there is sufficient converging evidence at a particular time to warrant belief in particular theoretical claims. As such diachronic realism is immediately separated from the kind of scientific realism concerned with the general existence or non-existence of abstract entities, or with the kind of scientific realism that claims that scientific theoretical claims merely have truth values.[46] Still, there are many realist positions that share this epistemic component with diachronic realism, and are, therefore, in this respect similar to it. Many of these formulations are based on a claim made some time ago by Wilfrid Sellars. "As I see it, to have good reason for holding a theory is *ipso facto* to have good reason for holding that the entities postulated by the theory exist."[47] Similar formulations of scientific realism can be found in the works of Richard Boyd,[48] Clark Glymour,[49] and (as the position he is attacking) Bas C. Van Fraassen.[50] Diachronic realism differs from this formulation chiefly in two respects. First, such a formulation is too strong in that it does not seem true that having good reasons for holding (or accepting) a theory are necessarily good reasons for believing it (or believing in the entities it postulates). After all, we should hold or accept the best theory currently available that can account for the largest number of data within the relevant field. At one time, Franklin's theory of static electricity held this honor within the (then fledgling) field of electrical physics. Still, as will be shown in chapter four, there were enough problems with this theory at the time to render out and out belief in it (and the 'electrical fluid' it postulated) to be, at best, reckless. In a different context, while 'possible world' semantics may well be the best current approach for accounting for some of our informed intuitions regarding modal logic, I, at least, balk at accepting the existence of 'possible worlds' (particularly if this 'existence' is understood in the same manner as the 'existence' of tables, molecules, and electrons). Still, if a theory is indeed the best theory available, we should accept it, use it, and try to elaborate it. Consequently, reasons for holding a theory are not always strong enough to warrant belief in it.

Second, Sellar's formulation of scientific realism does not explicitly countenance the possibility that a theory might at one time enjoy sufficient supportive evidence to warrant belief, and at other times not enjoy this status. Almost all traditional formulations of realist and anti-realist positions share such an ahistorical bias.[51] As I've already claimed, diachronic realism, on the other hand, explicitly recognizes the need to consider the evidence available for particular theories at particular stages of their development in assessing whether or not this evidence is sufficient to warrant belief. This sympathy for historical change in the theory-evidence relation strikes me as obviously necessary for any view that links warranted belief to the available evidence for a theory. Still, most realist formulations have not explicitly adopted this perspective and, as we shall see in chapters three and four, many anti-realist arguments are formulated precisely because of this ommission.

Finally, though I will explain and justify this notion in chapter three, I will deal with the evidence available for a theory at a particular time in terms of the level of theoretical virtues a given theory has. Roughly, these virtues correspond to what have sometimes been called 'criteria for theory choice', or, in Putnam's terminology, 'operational constraints'. For example, instead of merely considering as evidence for a theory the number of correct predictions it makes, I will also include among scientific virtues <u>constraints</u> on how these predictions can be made. The number and type of commitments a theory has, as well as the number of previously diverse fields it manages to unify, and whether it can accomplish this in a relatively simple manner, also enter into the level of virtues a theory can be said to have at a given time. At present, I only intend to introduce the terminology of 'scientific virtues' so that I can proceed to present my version of diachronic realism.

Given the above considerations, diachronic realism can be formulated as follows:

> When a particular theory can be shown to have achieved a certain level of scientific virtues, we can rationally believe in the truth of its theoretical claims and in the existence of the theoretical entities it postulates at the time this level of virtues is achieved.

This formulation is, again, meant to satisfy the considerations just discussed, as well as to be committed to a version of what Putnam calls 'metaphysical realism'. It will be argued later that this formulation of scientific realism is both historically and conceptually adequate. This adequacy, however, is contingent upon three additional concepts. I will briefly introduce these concepts in this section, together with an outline of the reasons the additions are necessary, and provide an overview to the strategy of the remaining chapters.

One of the three additional concepts is <u>benign chauvinism</u>, which has already been introduced, together with an outline of why it is important for an epistemic approach to scientific realism. The other two concepts are necessary to defend the intuition that realism offers the best explanation for the success of science, in view of the historical fact that not all successful theories were true, nor were all true (or 'approximately true') theories successful at all stages of their development. Roughly, if levels of success are differentiated and explained by a theory's being 'right' about something, it must be possible to separate theories in terms of how much they were right about. Sometimes theories whose central theoretical terms did not refer (and were thereby neither true nor approximately true) were nevertheless 'successful', given the level of evidence then available. Phlogiston theory, again, offers a well-known example of a theory that could account for a variety of chemical phenomena even though there is (and was—benign chauvinism) no such thing as phlogiston. In such cases, all the theory was right about was that this variety of data was, in fact, connected. Hence, even though its 'success' is best explained by its having been right about something, there is no reason to believe in the existence of its postulated entities.

This case differs from some other cases of non-referential theories that achieved a higher level of virtues than phlogiston theory (for example, Franklin's theory of static electricity). Since I want to argue that Franklin's theory achieved a higher level of virtues and thereby warrants more belief than phlogiston theory, and still maintain that Franklin's theory was neither true nor approximately true, I would seem to face a dilemma concerning just how to separate these two non-referential theories. I cannot separate them in terms of the amount of empirical data they covered for two reasons. First, this would amount to accepting van Fraassen's proposal that this is all that theories can be said to be 'right' about (an unhappy move if diachronic realism is to retain its 'realism'). Second, if I wish to separate these theories in terms of different levels of warranted belief in the theoretical components of these theories, then I certainly cannot restrict this belief to the empirical data alone. Roughly, since neither phlogiston theory nor Franklin's electrical theory were successfully referential, and yet there is a difference in the level of virtues in the two cases, and since diachronic realism is committed to this different level of virtues being explained in terms of Franklin's theory being right about more than phlogiston theory was, different notions besides 'true' and 'approximately true' are necessary to account for the difference in the two cases. Consequently, I intend for the notion of truthlikeness to cover the fact that a successful theory is right about something, either experimental regularities or at least partial theoretical correctness. As such, 'truthlikeness' (like 'true') must be interpreted as evidence and theory-independent, and our justification for positing different levels of truthlikeness must be in terms of different levels of scientific virtues. In short, our reasons for believing in the relative truthlikeness of a particular theory are based on evidential considerations, and could be wrong.

Furthermore, since phlogiston theory and Franklin's theory cannot be separated in terms of their truth or approximate truth (since, again, neither successfully referred), and yet Franklin's theory was more theoretically correct than phlogiston theory, there must be a way of articulating partial theoretical success without resorting to notions of 'true' or 'approximately true'. If realism is to provide an explanation of the increased 'success' of Franklin's theory, this articulation must also include some ontological grounds for the difference in epistemic attitudes. To have been partially theoretically correct without its central theoretical terms referring, there must have been something about at least the theoretical approach of Franklin's theory that was 'right', as opposed to the explicit theoretical claims Franklin made. I maintain that Franklin's theoretical approach was on the right track (as opposed to the theoretical approach of phlogiston theory), and hence was partially theoretically correct, because it was a direct forerunner to a later successful theory, or was closely related to such a theory, or was mistated, but with relatively minor amendments could be reconstructed by such a theory, which (from our perspective) is referentially successful, and hence true or approximately true. 'Being on the right track' is supposed to enable diachronic realism to differentiate between levels of truthlikeness in different theories (justified, again, by their different levels of virtues) in terms of a non-referential theory's being pulled up by its theoretical bootstraps by a true or approximately true theory.

While in fact all the examples I will subsequently provide to elucidate 'being on the right track' are justified by later theories which they directly led to, or with which they share important theoretical aspects, I do not intend to imply that it is because the referentially successful theories were later that they were more successful, and could consequently render the earlier theories more truthlike. It could turn out, for example, that Newton was right, and then I would want to argue that it is because relativity theory shares important theoretical aspects with universal gravitation that it is as successful as it is.

There are two important reasons for this non-evolutionary addendum to 'being on the right track'. First, I believe that scientific progress is a fact, but I do not wish to base my epistemological or ontological commitments on this fact (i.e., I think that scientific progress is a *contingent* fact). Consequently, it is quite compatible with diachronic realism that some earlier theories *may* turn out to be closer to the truth than some of their successors.[52] Diachronic realism is thereby committed to *some* scientific theories having achieved sufficient evidential success to warrant belief in these theories, but these theories are not *necessarily* current theories. Second, if 'being on the right track' is interpreted in terms of earlier successful theories leading to and being ontologically supported by later more successful theories (with 'earlier' and 'later' being the operative terms), it becomes *too easy* to historically demonstrate it. Duhem's position, it should be remembered, also shows that Newton's theory, for example, was a direct successor of earlier, less successful theories. On this interpretation the ontological notion of truthlikeness can too easily slide into the historical truism that later scientists have the advantage of being able to formulate their theories on the background established by earlier efforts. Consequently, to maintain its epistemic-ontological role, I intend for 'being on the right track' to remain aloof from evolutionary considerations.

Now that diachronic realism has been introduced, it remains for me to elaborate and defend it in the course of this dissertation. Chapter two concerns the relationship between diachronic realism and recently formulated theories of reference for theoretical terms. In this chapter I counter some anti-realist objections--chiefly, 'incommensurability' arguments based on the alleged meaning-reference shift of theoretical terms when theories change, and I further insist that the epistemic component of diachronic realism is a necessary condition for addressing realist/anti-realist debates; that is, a successful theory of reference for theoretical terms is not enough for a realist position. Chapter three is devoted to elaborating this epistemic component by clarifying the theoretical virtues introduced in this section, and I defend this approach against the most important conceptual anti-realist arguments. Chapter four addresses the most important historical anti-realist arguments, as well as elaborates the notions of 'benign chauvinism', 'truthlikeness', and 'being on the right track', and I use these notions to interpret two important historical examples in some detail. These chapters should provide a suitable elaboration and defense of diachronic realism as briefly presented in this chapter.

NOTES

[1]Many of the structural features of this example were introduced by Hans Reichenbach in order to support an entirely different point. See, Hans Reichenbach, _Experience and Prediction: An Analysis of the Foundations and the Structure of Knowledge_ (Chicago: University of Chicago Press, 1949/1938), pp. 115-29.

[2]An illustration of this situation is provided by Reichenbach. Ibid., p. 117.

[3]This point of view is defended by Michael Gardner in "The 19th Century Atomic Debates," _Philosophy of Science_, vol. 46, no. 1 (1979).

[4]Of course, the mathematical precision implied by 'ratio' is not assumed here.

[5]Henry E. Kyburg, _Philosophy of Science: A Formal Approach_ (New York: Macmillan, 1968), pp. 56-61. See also, Stephen F. Barker, _Philosophy of Mathematics_ (Englewood Cliffs: Prentice Hall, 1964), chapters 2 and 3. Brian Ellis, _Basic Concepts of Measurement_ (Cambridge: Cambridge University Press, 1966), chapter 2. Paul Benacerraf, "What Numbers Could Not Be," _Philosophical Review_, vol. 74, no. 1 (1965). Hartry Field, "Realism and Anti-Realism about Mathematics," _Philosophical Topics_, vol. 13, no. 1 (1982).

[6] For a brief review of some of these debates see, Hilary Putnam, "Mathematics Without Foundations," *Journal of Philosophy*, vol. 64, no. 1 (1967). W. V. O. Quine, "Ontological Reduction and the World of Numbers," and "On Multiplying Entities," both in *The Ways of Paradox and Other Essays* (Cambridge: Harvard University Press, 1977/1966), and the same author's *Word and Object* (Cambridge: MIT Press, 1975/1960), chapter 7. Finally, see W. V. O. Quine, "The Problem of Interpreting Modal Logic," and Ruth Barcan Marcus, "Modalities and Intensional Languages," both in Irving M. Copi and James A. Gould, eds., *Contemporary Readings in Logical Theory* (New York: Macmillan, 1967).

[7] For a discussion of early positivist formulations that adopt one or both of these claims, and their eventual demise, see Frederick Suppe's introduction to Frederick Suppe, ed., *The Structure of Scientific Theories* (Urbana: University of Illinois Press, 1974).

[8] See, for example, Grover Maxwell, "The Ontological Status of Theoretical Entities" in Herbert Feigl and Grover Maxwell, eds., *Scientific Explanation, Space, and Time* (Minneapolis: University of Minnesota Press, 1971/1962).

[9] See J. J. C. Smart, *Between Physics and Philosophy: An Introduction to the Philosophy of Science* (New York: Random House, 1968), pp. 133-51, and *Philosophy and Scientific Realism* (London: Routledge and Kegan Paul, 1971/1963), pp. 28-39. Wilfrid Sellars, "The Language of Theories" in Herbert Feigl and Grover Maxwell, eds., *Current Issues in the Philosophy of Science* (New York: Holt, Rinehart, and Winston, 1961) also appearing as chapter four of Wilfrid Sellars, *Science, Perception, and Reality* (London: Routledge and Kegan Paul, 1968/1963).

Wilfrid Sellars, "Is Scientific Realism Tenable?" in Peter Asquith and Frederick Suppe, eds., Philosophy of Science Association Proceedings, vol. II (Ann Arbor: Edwards Brothers, 1977). Wesley C. Salmon, "Why Ask Why?" Proceedings and Addresses of the American Philosophical Association, vol. 51, no. 6 (1978).

[10]See Clark Glymour, Theory and Evidence (Princeton: Princeton University Press, 1980), pp. 39-62.

[11]Willard Van Orman Quine, Word and Object, p. 22.

[12]See also W. V. O. Quine, "Natural Kinds" in Ontological Relativity and Other Essays (New York: Columbia University Press, 1969), "Posits and Reality," in The Ways of Paradox and Other Essays, and "Facts of the Matter," Southwestern Journal of Philosophy, vol. 9, no. 2 (1978).

[13]J. J. C. Smart interprets Quine's position as a realist position in "Quine's Philosophy of Science" in D. Davidson and J. Hintikka, eds., Words and Objections: Essays on the Work of W. V. Quine (Dordrecht: D. Reidel, 1975/1969), and Quine agrees with this interpretation in the same volume, pp. 292-94. A similar sense of 'realism' concerning the relative 'concreteness' of electrons and tables and the use of the term 'exists' can be found in J. J. C. Smart, Philosophy and Scientific Realism, pp. 33-38.

[14]Word and Object, p. 4.

[15]See, for example, Glymour, Theory and Evidence, for a similar criticism of the hypothetico-deductive model, pp. 29-39, and a criticism of holism, p. 45.

[16]See, again, "Natural Kinds."

[17]See Hilary Putnam, "Realism and Reason," Proceedings and Addresses of the American Philosophical Association, vol. 50, no. 6 (1977), reprinted in Hilary Putnam, Meaning and the Moral Sciences (London: Routledge and Kegan Paul, 1978). All subsequent references will be to this 1978 version of the paper.

[18]See his introduction to Hilary Putnam, Mathematics, Matter, and Method: Philosophical Papers, vol. I (Cambridge: Cambridge University Press, 1980), pp. vii-viii.

[19]"Realism and Reason," p. 125. Emphasis in original.

[20]Ibid., emphasis in original.

[21]See, for example, John McDowell's characterization of Dummett's verificationist position in "Truth Conditions, Bivalence, and Verificationism" in Gareth Evans and John McDowell, eds., Truth and Meaning (Oxford: Clarendon Press, 1976), pp. 47-50.

[22]"Realism and Reason," pp. 125-26. Emphasis in original.

[23]For Tarski's definition of 'truth' as well as his formal treatment of 'satisfaction', see Alfred Tarski, "The Concept of Truth in Formalized Languages" in J. H. Woodger, trans., Logic, Semantics, Metamathematics (Oxford: Clarendon Press, 1956) especially pp. 186-209, and the same author's "The Semantic Conception of Truth," Philosophy and Phenomenological Research, vol. 4, no. 3 (1944), especially pp. 352-55. One of the best secondary sources on Tarski's system, both in terms of formal accuracy and readability, is Mark Platts, Ways of Meaning: An Introduction to a Philosophy of Language (London: Routledge and Kegan Paul, 1979), chapter 1.

[24]See Alfred Tarski, "The Concept of Truth in Formalized

Languages," pp. 157-60, 165, 267, and "The Semantic Conception of Truth," pp. 346-49.

[25]See Donald Davidson, "Truth and Meaning," _Synthese_, vol. 17 (1967), especially pp. 313-17, and "True to the Facts," _Journal of Philosophy_, vol. 66, no. 21 (1969). See also, John McDowell, "Truth Conditions, Bivalence, and Verificationism," J. A. Foster, "Meaning and Truth Theory," and Brian Loar, "Two Theories of Meaning" all in Evans and McDowell, eds., _Truth and Meaning_. Finally, see Mark Platts, _Ways of Meaning_, especially chapters 2, 3, and 9.

[26]"The Semantic Conception of Truth," pp. 342-44.

[27]See G. H. Merrill, "The Model-Theoretic Argument Against Realism," _Philosophy of Science_, vol. 47, no. 1 (1980).

[28]Ibid., p. 71.

[29]Ibid., pp. 71-72. Emphasis in original.

[30]See Clark Glymour, "Conceptual Scheming or Confessions of a Metaphysical Realism," _Synthese_, vol. 51 (1982).

[31]Ibid., pp. 175-76.

[32]Ibid., p. 176.

[33]Ibid.

[34]"Realism and Reason," p. 126.

[35]Ibid., pp. 126-27.

[36]Ibid.

[37]"Conceptual Scheming," pp. 177-78.

[38]Ibid., p. 179.

[39]See, for example, Paul Horwich, "Three Forms of Realism," _Synthese_, vol. 51 (1982).

[40]Quine hints at such an account in "Natural Kinds" as well as in other places, and Van Fraassen appeals to it in The Scientific Image (Oxford: Clarendon Press, 1980), p. 40.

[41]Pierre Duhem, The Aim and Structure of Physical Theory (New York: Athenium, 1977/1905), chapter 7.

[42]Ibid., p. 219.

[43]Ibid., p. 221.

[44]Ibid., pp. 222-52.

[45]For an independent attack on such a position see Nancy Cartwright, "When Explanation Leads to Inference," Philosophical Topics, vol. 13, no. 1 (1982).

[46]And for a number of other realist positions as well. Ernest Nagel, for example, discusses five realist formulations that would also be acceptable to some anti-realists. I am, of course, concerned with a realist formulation that would not be acceptable to anti-realists. See, Ernest Nagel, The Structure of Science: Problems in the Logic of Scientific Explanation (New York: Harcourt, Brace, and World, 1961), pp. 145-52.

[47]Wilfrid Sellars, Science, Perception, and Reality, p. 91.

[48]Richard N. Boyd, "Realism, Underdetermination, and a Causal Theory of Evidence," Nous, vol. 7, no. 1 (1973), p. 1.

[49]Clark Glymour, "To Save the Noumena." Paper presented at the Eastern Division of the American Philosophical Association, 1976. P. 1.

[50]Bas C. Van Fraassen, The Scientific Image, pp. 6-8.

[51]Some exceptions to this general tendency that have strongly influenced my formulation of diachronic realism are Michael Gardner,

"The 19th Century Atomic Debates," and W. H. Newton-Smith, The Rationality of Science (Boston: Routledge and Kegan Paul, 1981), pp. 29, 40.

[52] In this sense especially 'truthlikeness' and 'being on the right track' differ from the otherwise similar notion of 'verisimilitude' as discussed by Newton-Smith, which is also essentially a device for blocking the 'pessimistic induction' that since some past 'successful' theories were false, we have little reason to believe that present successful theories are true. See, The Rationality of Science, especially chapter 8.

CHAPTER II

REFERENCE AND SCIENTIFIC REALISM

Reference Change and Incommensurability

In the middle of the 18th century, experiments conducted with the newly invented Leyden Jar led to the 'one-fluid' theory of (static) electricity. For some historians, this theory marks the beginning of the study of electricity as a legitimate part of natural philosophy.[1] Certainly most philosophers and historians have traditionally considered the rapid progress made during the next 50 years on the basis of this theory as establishing a new branch of science that has progressed more or less continuously to the present day. In fact, many of the phenomena that Franklin and his followers attempted to explain with this theory are still described in more or less the same terms in current introductory science texts.[2] Still, references to 'imponderable fluids' are not to be found in these current texts, while they can be considered to be central to the older theories. It thereby becomes important to try to analyze in what sense contemporary theories are 'about the same thing'.[3] Progress seems to commit one to some sense of continuity, while, of course, allowing for more refined experimental and theoretical perspectives. Both proponents, and recently, opponents of cumulative scientific progress are therefore interested in notions of 'sameness' of meaning and reference with regard to theoretical terms and statements.

Perhaps the best known attempt to undermine cumulative progress in science is found in the work of Thomas Kuhn.[4] His attacks on the traditional notion of cumulative progress are broad and varied. Each succeeding chapter provides an interpretation of historical examples that, if accurate, increasingly undermine a number of classical empiricist claims concerning progress in science. Gradually, the methods, data, and underlying assumptions of succeeding theories are claimed to be incommensurable, and, ultimately, it is argued that there are no straightforward bridges across the theoretical divide. Still, much of the evidence he introduces depends at least implicitly on the narrower topics of meaning and reference. While arguing against the alleged derivability of older theories from the framework of newer, more comprehensive systems, for example, he makes the following claims concerning the specific cases of Newtonian Mechanics and Special Relativity.

> The physical reference of these Einsteinian concepts ['space', 'time', and 'mass'] are by no means identical with those of the Newtonian concepts that bear the same name. (Newtonian mass is conserved; Einsteinian mass is convertible with energy. Only at low relative velocities may the two be measured in the same way, and even then they must not be conceived to be the same.)[5]

From the non-identity of the respective concepts, Kuhn argues that the alleged derivations, even at low relative velocities, are bankrupt, i.e., claims about Newtonian 'mass' are not, and cannot be, derivable from the different theoretical framework of Einstein's theory.

What is it exactly that Kuhn thinks is changed in this example? The first sentence in the quoted passage clearly states that it is the shift in 'physical reference' that constitutes the problem. He then immediately goes on to justify this claim by citing examples of different statements believed to be true of 'mass' in the two theories. Specifically, Kuhn lists two differences that may very well not be legitimately handled in the same way. First, he says, Newtonian 'mass' is conserved while Einsteinian 'mass' is convertible with energy at high velocities with respect to the speed of light. This might, without too much oversimplification, be analyzed as a difference in the properties of 'mass', or at least as a difference in the way the variable 'mass' will behave mathematically with appropriate changes in reference frames and the other variables involved. The second claim is that either the measuring operations for determining the value of 'mass' are not the same in the two theories, or that the results obtained via measuring in the two theories will not always correspond. If statements believed to be true about a concept comprise at least part of what is often called the _meaning_[6] of that concept, and if Kuhn accepts the (quasi) Fregean claim that meaning determines reference, then it is possible to understand his insistence that the term 'mass' underwent a change in "physical reference"--i.e., Newton and Einstein are not talking about the same thing. After all, the _only_ reasons Kuhn provides for the alleged reference shift of 'mass' in Newton's and Einstein's theories is that the two theories provide different theoretical accounts of 'mass'.

While Kuhn is concerned with much broader topics, the above seems to be a fair representation of his implicit views concerning the meaning and reference of theoretical terms. Before these views are expanded and criticized, I will try to flush out what such views might have to do with cumulative scientific progress, and why this topic is important for the purposes of this dissertation. First, it seems clear that if 'mass', for example, is a central term in both Newton's and Einstein's theories, and if its 'physical reference' is in fact different, then the two theories are in an important sense not 'about the same thing'. While it may be true that the very notion of the 'physical reference' of concepts like 'mass' is problematical, _if_ these problems can be resolved, and _if_ the reference can be shown to have changed, cross-theoretical statements using it would necessarily be about different things and thus at cross-purposes. Hence, Kuhn's arguments against derivability would appear to hold. A logical analogy could be made concerning a predicate that was given two different extensions. Surely logical validity would not result if the predicates with different extensions were used interchangeably in a proof. Further, it seems clear that 'theories' about the two different predicates (or even _using_ the two different predicates) would not be likely to interconnect, at least not in the simple way that pro-cumulative theorists imagine.

It is something like this simplified case that Kuhn seems to be mustering against the notion of cumulative progress in science. Combined with arguments concerning the theory-ladenness of experimental results, and the non-translatability of methodological perspectives, reference change seems to eliminate all reasonable candidates for different theories having the same subject matter. Simply put, if scientists within different traditions are not talking about the same things, how can later theories be said to increase our understanding of the topics covered by their predecessors? If we do not increase our knowledge about (at least some) of the same topics, what is left of the claim that successive theories often represent cumulative progress?

Of course, problems with scientific progress transcend the realist/anti-realist debates with which I am primarily concerned. An anti-realist can certainly be just as concerned with preserving progress as a realist.[7] While it may not neatly divide into realist/anti-realist considerations, however, scientific progress is a desideratum of the realist view I'm defending. As will be shown later, some notion of progress is central to the notion of 'being on the right track'. Furthermore, much of the literature concerning scientific progress approaches the topic from the perspective of a more explicit treatment of meaning and reference than is found in Kuhn. Theories of reference in particular have recently engendered a great deal of new interest in scientific realism. Also because I want to offer an alternative approach to realism, this chapter will consider the topics of the meaning and reference of scientific terms and statements in some detail.

Kuhn, as was argued above, seems committed to a view that meaning change engenders reference change. At least, if this is not his view it is not clear that he has given even implicit arguments in favor of the alleged reference change in the Newtonian/Einsteinian example. Furthermore, he is not the only anti-realist who has at least implicitly adopted such a view. Perhaps second in influence only to Kuhn as an opponent of cumulative scientific progress is Paul Feyerabend, who seems to argue in much the same way.

> In classical, prerelativistic physics, the concept of mass (and, for that matter, the concept of length and the concept of time duration) was absolute in the sense that the mass of a system was not influenced (except, perhaps, causally) by its motion in the coordinate system chosen. Within relativity, however, mass has

> become a relational concept whose specification is incomplete without indication of the coordinate system to which the spatiotemporal descriptions are all to be referred. Of course, the values obtained on measurement of the classical mass and of the relativistic mass will agree in the domain 'D', in which the classical concepts were first found to be useful. This does not mean that what is measured is the same in both cases: what is measured in the classical case is an intrinsic property of the system under consideration; what is measured in the case of relativity is a relation between the system and certain characteristics of 'D'. It is also impossible to define the exact classical concepts in relativistic terms as to relate them with the help of an empirical generalization. . . . It is therefore again necessary to abandon completely the classical conceptual scheme once the theory of relativity has been introduced.[8]

That it is the change in the meanings of scientific terms that he believes engenders "abandoning completely the classical conceptual scheme" is made more explicit in Feyerabend's account. He argues that the point of the above passage is to show that:

> The postulate of meaning invariance is incompatible with actual scientific practice. . . . in most cases it is impossible to relate successive scientific theories in such a manner that the key terms they provide for the description of a domain 'D', where they overlap and are empirically adequate, either possess the same meanings or can at least be connected by experimental generalizations.[9]

Both Kuhn and Feyerabend, then, base part of their attacks on cumulative scientific progress on considerations of meaning and reference. They also (at least implicitly) seem committed to the view that changes in the meaning of scientific terms (in the rough sense described earlier) constitute changes in the reference (if any) of these terms. It is now time to investigate these claims in more detail.

First, there is an inherent vagueness in the claim that meaning determines reference, especially since explicit accounts of meaning and reference are not to be found in either of the cited authors. Does any change in statements-held-true concerning a theoretical concept constitute a meaning change, and, hence, a shift in reference? Does the status of the statement-held-true within the two theories determine whether or not the reference of the concept has changed? Does the notion of 'meaning' encompass the notion of 'reference', or are they distinct concepts? Since neither Kuhn nor Feyerabend address such issues, it will be necessary to reconstruct their positions in such a way that their probable position concerning these questions can be ascertained. This task will be aided somewhat by the fact that their implicit view of the meaning and reference of theoretical terms is likely to be borrowed from the views they are criticizing. Since both authors are concerned primarily with refuting *empiricist* notions of cumulative progress, derivability of the older theory from the perspective of the newer, etc., we can hope to get clearer on what *type* of meaning determines reference by analyzing such topics within the 20th-century empiricist tradition.

Classical Empiricist Accounts of Reference

The earliest versions of 20th-century empiricism were, of course, the various forms of logical positivism advocated by the Vienna Circle and philosophers and scientists influenced by this group. While many disagreements arose concerning a sense-data foundation or a physicalist foundation for empirical knowledge, this school was virtually unanimous concerning the 'verifiability' theory of meaning. Simply put, this theory of meaning was an attempt to separate cognitively significant (testable) statements from metaphysical flights of fancy.

> We say that a sentence is factually significant to any given person, if, and only if, he knows how to verify the proposition which it purports to express--that is--if he knows what observations would lead him, under certain conditions, to accept the proposition as being true, or reject it as being false.[10]

The verifiability criterion, construed as it is above or in any of its many formulations, was not generally accepted as a theory of (semantic) meaning. Still, many remained sympathetic to it construed as at least a necessary condition for the meaningfulness of empirical statements, at least in science. So construed, it could be interpreted as a proviso that a statement must be verifiable (in principle at least) in order to be acceptable as a part of empirical science (i.e., meaningful scientific statements must be testable). This insistence on empirically significant scientific statements was at least as important a motivation for logical empiricists as any explicit interest in (semantic) meaning or reference. It will be worthwhile to keep this aspect of their enterprise in mind as we attempt to uncover their position on meaning and reference.

An important influence on the empiricists, published a year before Carnap's paradigmatic Logical Structure of the World, was P. W. Bridgman's The Logic of Modern Physics,[11] in which Bridgman developed his notion of 'operational definitions'. One of the main motivations for this work was the 'revolutions' caused in modern physics by Einstein's theories and by recent work in the quantum domain. Bridgman thought that the real basis of Einstein's conceptual revolution was his critical attitude towards scientific concepts, particularly towards the relation between theoretical concepts and means of testing them (briefly discussed in the preceding paragraph).

> It was a great shock to discover that classical concepts, accepted unquestioningly, were inadequate to meet the actual situation, and the shock of this discovery has resulted in a critical attitude toward our whole conceptual structure which must at least in part be permanent. Reflection on the situation after the event shows that it should not have needed the new experimental facts which led to relativity to convince us of the inadequacy of our previous concepts, but that a sufficiently shrewd analysis should have prepared us for at least the possibility of what Einstein did.[12]

This 'shrewd analysis' would have revealed that the change in attitude that Einstein initiated would not have been necessary if the concepts of empirical science had not been established in such a way that new experience would show them to be metaphysical fetters,[13] and if "our present experience did not exact hostages of the future."[14] The main aspect of classical physics that led to such an impass was the tendency to define physical concepts in terms of their properties.[15] Under such circumstances, even if a definition of a concept should be wholly adequate with respect to all current experience, there is no guarantee that it will remain so as this experience is expanded as a result of new experiments. If, however, these concepts had been defined in terms of physical operations used to measure or detect these concepts, such conceptual fetters would never have resulted.[16] If new physical operations would be adopted in the future, a new concept should be introduced corresponding to them.[17] If there are no unambiguous operations presently available for a proposed physical concept, that concept should be considered empirically meaningless. In either case, we would not have found ourselves stymied with classical concepts inadequate for dealing with the new experimental data.

Given that the main motivation for operational definitions was not a concern to clarify (semantic) meaning, what sort of view of meaning and reference of theoretical terms can be found in Bridgman's work? The meaning of theoretical terms is explicitly determined by a unique set of operations used to detect or measure that term. To return to the example used by Kuhn, Einsteinian 'mass' will correspond to Newtonian 'mass' if and only if this concept is measured in the same way by the two theories, and this measurement technique achieves the same or sufficiently similar results. In chemistry, the mass of a substance is often measured by weighing samples of it before and after some chemical change. It is on such a basis that the law of the conservation of mass was first formulated (rather, from Bridgman's point of view this is how it _should_ have been formulated, historically; Lavoisier probably assumed it). Neither Newton nor Einstein, however, has anything to say about _this_ scientific treatment of the concept 'mass'. Both are rather concerned (in the case used by Kuhn at least) with the mass of bodies in motion, or their _inertial_ mass. If mass is conceived in classical physics as a _property_ of bodies, and some correlation between mass measured by weighing and the amount of force necessary to accelerate it a certain amount has been established, perhaps the operations used to determine mass in chemistry might also be used to determine the mass of a body before it is moved (if it is also thought to be conserved). There is no hope, however, of carrying out such operations concerning the sub-atomic particles that provided the experimental tests for Einstein's theory.[18] Consequently, if this operational definition of mass is used, the two theories should strictly use different terms according to Bridgman ('mass' cannot mean the same thing).

This, however, is not how Bridgman or Kuhn interpret the concept 'mass' in the two theories. Rather, Bridgman treats 'mass' as determined by the other quantities in Newton's second law, or Einstein's Lorentzian transformation of this in the case of the kinetic energy of a slowly accelerated electron.[19]

> We are eventually able to give to each rigid material body a numerical tag characteristic of the body, such that the product of

> this number and the acceleration it receives under the action of any given force applied to it by a spring balance is numerically equal to the force, . . .[20]

If 'mass' is operationally defined by the equations of the Newtonian and Einsteinian theories, however, it is, again, not the same concept in the two theories, since these equations are not the same. Consequently, on any of the above readings of 'mass', it is not the same concept in Newton and Einstein, and, hence, Newton and Einstein aren't really contradicting one another.

This conclusion, which seems to follow from Bridgman's operational treatment of theoretical concepts, is precisely the view attributed to Kuhn and Feyerabend earlier. Bridgman, of course, does not develop the consequences that the avowedly meaning-change theorists do, and this is probably due to his different motivations for developing his view. (He was, after all, trying to found an empiricist philosophy of science, while Kuhn and Feyerabend were arguing against such an enterprise.) It would seem, however, that if operational definitions provide the 'meaning' of theoretical constructs, and if change in the operations for measuring or detecting a concept entails changes in these concepts, Bridgman's position, if spelled out, is implicitly advocating a meaning-change view as well. Does the physical reference of a concept change if the meaning changes as it did in Kuhn? This is even harder to establish in the case of Bridgman. He, too, fails to specifically address the notions of meaning and reference that we've come to accept as terms of art (at least since Kripke). Worse, in another context, he makes claims about the cumulative progress of atomic theory leading to the 'physical reality' of atoms that seems very strange given his earlier strict operationalist perspective.

> The atom was invented to explain constant combining weights in chemistry. For a long time there was no other experimental evidence of its existence, and it remained a pure invention, without physical reality, useful in discussing a certain group of phenomena. It is one of the most fascinating things in physics to trace the accumulation of independent new physical information all pointing to the atom, until now we are as convinced of its physical reality as of our hands and feet.[21]

How could we achieve independent new physical information concerning the atom if a unique set of operations must define it as he argued earlier? How could evidence accumulate at all if new operations entail new concepts, and new concepts entail new theoretical entities?

Such arguments in Bridgman are indeed puzzling, and seem to be contradictory. Perhaps he was simply inconsistent, but, again, a more charitable construal may be available if we remember once again his main motivation for developing his position. He was primarily concerned with articulating how theoretical scientific concepts could be assured of having empirical content. Given this emphasis, he would not be overly careful about implicit consequences his view might have concerning (semantic) meaning and reference. If we've trying to sort this problem out, we can only be puzzled by his above treatment of 'atom'. Just when it appeared that he held a strict meaning-change view, we find him claiming a physical reality for the theoretical concept 'atom', based on the very cumulative progress within atomic theory that a meaning-change view would often deny. It is little wonder that an explicit treatment of meaning and reference is not to be found in Kuhn and Feyerabend, since the position that they are attacking also failed to articulate an unambiguous view concerning these topics. Hence, though the incommensurability arguments found in Kuhn and Feyerabend seem much too strong, and even naive, to current theorists interested in semantics, they are fairly representative of the level of semantic sophistication achieved by some of their main opponents.

Perhaps the situation is clearer in some of the other empiricists. It would seem that if any of these philosophers of science were also concerned with strictly semantic issues, that philosopher would be Rudolf Carnap. Does he present a view of the meaning and reference of theoretical terms that would be more accessible to more recent philosophers? First, it might be useful to elucidate Carnap's position with respect to Bridgman's. Operational definitions in Bridgman's sense can be construed as a special case of explicit definitions. The logical form of explicit definitions, according to Carnap, is the following: '$(x)(Tx \equiv (O_2x \rightarrow O_1x))$', where 'T' stands for the theoretical construct being defined, 'O_2' stands for a certain set of test conditions or operations, and 'O_1' stands for a positive outcome of these tests.[22] Given this formulation, the theoretical term 'T' is defined in terms of certain experimental tests and outcomes, rather than in terms of 'T's' properties (hence, being close to Bridgman's position outlined earlier). If we also stipulate that every time we establish new test conditions, the definition of 'T' changes, the above formulation amounts to the same thing as Bridgman's treatment of explicit definitions. Since the implications of operational definitions for the meaning and reference of theoretical terms has already been discussed, we can illustrate Carnap's position by considering his criticisms of this form.

Carnap's criticism of the explicit definition approach for the introduction of theoretical terms begins with a discussion concerning their adequacy for handling dispositional concepts in science. Consider, for example, the following model for the logical form of explicit definitions. Let 'T' stand for the dispositional concept 'soluble in water', 'O_2' for the test condition of placing a substance in water, and 'O_1' for the positive test result that the substance dissolves within a certain amount of time. Given the usual interpretations of the logical connectives in '$(x)(Tx \equiv (O_2x \rightarrow O_1x))$', 'T' will properly apply to any substance not placed in water, since a sentence of the form '$\phi \equiv \psi$' is true when both 'ϕ' and 'ψ' have the same truth value and '$\phi \rightarrow \psi$' is true whenever 'ϕ' is false. Hence, any substance not placed in water would render 'O_2x' false, and therefore '$O_2x \rightarrow O_1x$' true, and consequently 'Tx' true (i.e., this substance would be 'soluble'). It is for this reason that Carnap rejects explicit definitions at this point in his career, and introduces another approach for the introduction of theoretical terms, that of 'reduction sentences'.[23]

Instead of explicitly defining a theoretical term, Carnap introduces it into the language of science by a pair of sentences which determine cases where the term applies to an experimental result and where it doesn't. If 'Q_1' and 'Q_4' represent experimental conditions (test situations), and 'Q_2' and 'Q_5' represent positive results of these test conditions, and 'Q_3' represents the ascription of the theoretical concept, then '$(x)(Q_1x \rightarrow (Q_2x \rightarrow Q_3x))$' and '$(x)(Q_4x \rightarrow (Q_5x \rightarrow \sim Q_3x))$' logically depict a case where the theoretical term applies and a case where it does not, and are called 'reduction pairs' for the introduction of that theoretical concept.[24] In the special case where 'Q_4' coincides with 'Q_1', and 'Q_5' with 'Q_2', the reduction pair can be reduced to a single 'bilateral reduction sentence' of the form '$(x)(Q_1x \rightarrow (Q_3x \equiv Q_2x))$'.[25] Sets of reduction pairs or bilateral reduction sentences do not explicitly define the theoretical construct, but rather give it a 'meaning' only in cases where the test conditions are fulfilled.[26] For example, an object that is never placed in water (using our earlier example) is not under the extension of the theoretical construct, and should not be considered as having or not having the theoretical property (or as belonging or not belonging to the class that it represents, etc.). Carnap believes that such an approach is better suited to handle disposition terms than explicit definitions, since it is not subject to the unhappy consequence that the theoretical term will apply to all objects that are not placed under the test conditions.

It is only after the problems of disposition terms have been discussed that Carnap mentions implications of the reduction sentence approach that may be more important for the meaning-change views we've been considering. First, he states that reduction sentences allow for a theoretical concept to be measured (or operationally defined) in more than one way, in contrast to Bridgman's strict formulation considered earlier.

> If a property or physical magnitude can be determined by different methods then we may state one reduction pair or one bilateral reduction sentence for each method. The intensity of an electric current can be measured for instance by measuring the heat produced in the conductor, or the deviation of a magnetic needle, or the quantity of silver separated out of a solution, or the quantity of hydrogen separated out of water, etc. We may state a set of bilateral reduction sentences, one corresponding to each of these methods.[27]

Second, he believes that such an approach is advantageous for scientific progress, in that new experimental progress need not disrupt past theoretical commitments, or, again, in Bridgman's terms, "our present experience need not exact hostages of the future." Specifically, since reduction sentences only confer meaning on theoretical terms when test conditions are fulfilled, there is always a 'region of indeterminateness' or a 'third class' besides those of falling under the extension of a concept or not falling under it; i.e., the cases in which the test conditions are not met.

> The scientist wishes neither to determine all the cases of the third class positively, nor all of them negatively; he wishes to leave these questions open until the results of further investigations suggest the statement of a new reduction pair; thereby some of the cases so far undetermined become determined positively and some negatively. If we now were to state a definition, we should have to revoke it at such a new stage of the development of science, and to state a new definition, incompatible with the first one. If, on the other hand, we were now to state a reduction pair, we should merely have to add one or more reduction pairs at the new stage; and these pairs will be compatible with the first one. In this latter case we do not correct the determinations laid down in the previous stage but simply supplement them.[28]

This is, in a sense, precisely the motivation that we found earlier in Bridgman, i.e., to avoid drastic conceptual revision every time new experimental conditions are applied to existing theoretical concepts. Carnap's approach also seems to avoid the meaning change consequences that were shown to be implicit in Bridgman's account. Does his view at this stage really offer an alternative to meaning-change views?

Many philosophers of science interpret Carnap's approach in this way.[29] In a recent paper, however, Jane English has argued persuasively that this is not the case, and that Carnap's position concerning the meaning of theoretical terms in his 1936-37 paper (as well as later formulations) itself represents an implicit meaning-change view.[30] To support this position, English considers a simple case of theory change, formalized in a manner compatible with Carnap's reduction sentence approach. Assume that theory 'A' consists solely of one bilateral reduction sentence, partially defining a theoretical concept 'T_1' in terms of test conditions 'O_1' and observational results of these tests 'O_2', so that the theory can be formally represented by the logical statement '$(x)(O_1x \rightarrow (T_1x \equiv O_2x))$'. If 'a' is an object that has successfully undergone the experimental tests, it would then be a commitment of theory 'A' that this object has the theoretical property 'T_1' (i.e., 'T_1a').[31] Assume also that theory 'B' shares this bilateral reduction sentence with theory 'A', but that further research has reduced the region of indeterminacy of the term 'T_1' by ruling out certain objects that satisfy this reduction sentence, but also satisfy a new test condition that is meant to depict other properties, now incompatible with 'T_1' (i.e., a new reduction sentence is added to theory 'B': '$(x)(O_3x \rightarrow (O_4x \rightarrow \sim T_1x))$'). If the object 'a' that satisfied the original bilateral reduction sentence also satisfies this second reduction sentence, theory 'B' would be committed to the statement '$\sim T_1a$'.[32] Would these two theories contradict one another? If not, English has constructed an example of theoretical term introduction via chains of reduction sentences that leads to a meaning-change view.

First, it should be noted that the two theories do not contradict one another observationally. The observational consequences of theory 'B' are not incompatible with the observational consequences of theory 'A', since the only way they could contradict one another would be as a result of test condition 'O_3', which is not even contained in theory 'A'. It does seem, however, that the two theories contradict one another theoretically, since a commitment of theory 'A' concerning object 'a' is 'T_1a', which seems formally incompatible with the commitment '$\sim T_1a$' of theory 'B'. One theory seems to be committed to the <u>same</u> object having a property that the other theory denies to it. On closer inspection, however, this does not have to be the case. Theory 'B's' commitment to '$\sim T_1a$' is based on the assumption '$(x)((O_3x \cdot O_4x) \rightarrow \sim T_x)$', which is, again, not even covered by theory 'A'. The two theories would contradict one another theoretically only if '$\sim T_1a \cdot T_1a$' is impossible, but, for theory 'A', '$\sim T_1a$', being based on different test conditions, is within the region of indeterminateness for 'T_1'. Consequently, it <u>could</u> be given the value false within 'A', or, more in line with Carnap's intentions, be simply indeterminate within 'A'. In neither case does '$T_1a \cdot \sim T_1a$' represent a straightforward contradiction between the two theories (for the same reason that the two theories do not contradict one another observationally).[33] It seems, then, that meaning-change examples can be generated via chains of reduction sentences.

To show that the above example is not only a formal consideration, consider a future state of biology, hoped for by many contemporary essentialists regarding natural kind terms. Perhaps the best operational treatment of 'species' currently available to biological taxonomists involves the interbreeding of two groups of animals that produces fertile offspring. This, of course, could easily be symbolized precisely as was theory 'A' above, with 'T_1' representing the theoretical concept 'belongs to the same species x'. Assume, however, that some future stage of biology succeeds in determining that microstructures of the animals belonging to species 'T_1' can be theoretically used in organizing data, making predictions, etc., precisely analogously to the role that microstructures now play in physics. At that point, assuming that there will be test conditions determining when this microstructure is present or absent, a new definition utilizing these tests may play a role precisely like the additional reduction sentence in theory 'B' above. It is then possible that a pair of animals that could interbreed and produce fertile offspring may both not have this underlying microstructure. According to theory 'A', of course, these animals would be members of the same species, while according to theory 'B' they would not. Again, however, the two theories would not contradict one another, since the test conditions that lead to the negative assertion in theory 'B' are not available in theory 'A'. <u>Any</u> change of classification that resulted in tightening the conditions of class membership, if the claim is partially defined in terms of reduction sentences, could lead to such a result. That is, since the two theories use different test conditions for establishing class membership, the classes are different, and attributing incompatible properties to <u>different</u> classes does not involve a contradiction.

English concludes that any sets of reduction sentences that do not contradict one another observationally do not contradict theoretically on Carnap's view.[34] His earlier example of the intensity of an electric current was a fortunate one in that the addition of new reduction sentences did not further restrict the extension of 'T_1', but rather, extended it. In cases like the above, however, the theories are not 'talking about the same thing'. Perhaps this result is due to the still very strong operational-observational basis of Carnap's views in

1936-37. He certainly liberalized his conditions with the passage of time.[35] English argues, however, that as his views liberalized, they became more contextualist (holistic) and, subsequently even more open to meaning-change interpretations.[36]

As was the case with Bridgman, however, the precise relation between the procedures for establishing the empirical significance of theoretical terms and the implicit meaning-change views these procedures might entail is never explicitly drawn by Carnap. In fact, the problem we've been investigating in this chapter was probably not explicitly addressed until the work of Kuhn, which was avowedly anti-empiricist. Even there, as we've seen, the treatment of semantic notions of meaning and reference is implicit at best, so that it remains difficult to determine precisely what changes when the meanings of theoretical terms undergo revisions. This is, I think, largely due to the fact that 'meaning' and 'reference' were not taken as terms of art until post-Kripkean philosophy, at least by philosophers of science (Frege, Searle, and Strawson, for example, did not have much influence on philosophy of science). Neither Frege's nor Carnap's extensive use of 'reference' and 'sense', 'intension' and 'extension', etc., have the tight applicability to our problem that one might wish. To give one example to solidify this point, Carnap, in a relatively late article, offers the following view of increasing intensional precision in science that is both strikingly similar, and yet worlds apart from corresponding treatments of necessity, scientific progress, and the relation between theoretical terms and their corresponding properties found in, for example, Kripke and Putnam.

> In the oldest book on chemistry, . . . there were a great number of statements describing the properties of a given substance, say water or sulphuric acid, including its reactions with other substances. There was no clear indication as to which of these numerous properties were to be taken as essential or definatory for the substance. Therefore, at least on the basis of the book above, we cannot determine which of the statements made in the book were analytic and which synthetic for its author. . . . But in chemistry there was an early development from the state described to states of greater and greater intensional precision. On the basis of the theory of chemical elements, slowly with increasing explicitness certain properties were selected as essential. . . . For the elementary substances, first certain experimental properties were more and more clearly selected as definatory, for example, the atomic weight, later the position in Mendeleyev's system. Still later, with a differentiation of the various isotopes, the nuclear composition was regarded as definatory, say characterized by the number of protons (atomic number) and the number of neutrons.[37]

As in Kripke, we find in this quotation a belief that science progresses by replacing accidental properties with necessary properties of a substance. Unlike Kripke, this necessity is interpreted in terms of definitions of the terms and analyticity. Corresponding near-misses are also found in earlier empiricist work on designation, meaning, extension, etc. Not surprisingly, it is difficult to extract clear commitments on these issues in writers like Kuhn, who were arguing against the empiricist position. Consequently, it is not clear, as we discussed earlier, what changes for Kuhn with a change of theory, nor how much meaning change is necessary for a complete breakdown of communication between competing theories.

While I have possibly belabored the lack of clear concepts of meaning and reference within classical empiricism, it is important to realize that these concepts have only recently played crucial roles in philosophy of science. Consequently, Kuhn and Feyerabend were not being foolish or unfair when they took their incommensurability arguments to be telling against contemporary views of scientific progress. Furthermore, to adequately assess the importance of their incommensurability arguments, one must determine how their implicit interpretations of meaning and reference fare when they are unpacked in terms of more recent, clearer semantic accounts. It will therefore be necessary to discuss how theories of reference in particular have been formulated recently, and to use these new formulations to address scientific examples. Only then can it be decided whether there is reason for a realist (or any philosopher of science who is committed to scientific progress) to be worried by incommensurability claims.

Current Theories of Reference Applied to Scientific Examples

Without too much oversimplification, I think there are three major approaches to meaning and reference expressible in current terminology: the strict descriptivist view (SD), the cluster view (CD), and the causal view (CT). For certain kinds of terms (theoretical concepts being an example), some combination of these may be the most appropriate approach. Still, it will be useful to treat each one separately (as an 'ideal type') at the beginning of our analysis, and discuss possible complications for certain kinds of terms later.

The strict descriptivist (SD) view of reference holds that the reference of a term is determined by the meaning of that term in the sense that "the meaning of a word is a set of characters that are necessary and sufficient for it to apply,"[38] or, "concepts or meanings associated with general terms and names determine the set of things to which they apply or refer."[39] The term 'mass', for example, refers to whatever satisfies the set of concepts or meanings associated with it in a given theory (examples, $m = f/a$, mass $\propto$ weight, $m = \frac{E}{c^2}$, etc.). If different theories associate different concepts with this term, they are necessarily referring to different entities or sets of entities. As in a strict reading of Bridgman's concept of operational definitions, if _any_ of the concepts associated with a theoretical term change, a new term should (strictly) be introduced. That is, the set of concepts associated with a theoretical term _define_ that term, and determine its extension (if any) via necessary and sufficient conditions for class membership. An (SD) view of reference is distinguished by this insistence that if _any_ of the concepts or meanings associated with a theoretical term change, there is a corresponding change of reference. According to this view, then, Einsteinian and Newtonian 'mass' must have different references, since the two theories certainly associate (some) different meanings to the term (for example, the mass of an object is invariant for Newton, but not for Einstein). Similarly, Lavoisier's 'Oxygen' cannot pick out the same gas as Priestley's 'de-phlogisticated air', since part of the meaning of Priestley's air involves "being capable of taking more phlogiston from nitrous air,"[40] a formulation which Lavoisier, of course, rejects along with phlogiston theory. For that matter, Lavoisier's 'oxygen' cannot pick out the same gas as our 'oxygen', since this term was chosen by him on the view that oxygen is an 'acidifying principle'.[41] In general, no term will retain its reference through a theoretical change unless the term is not at all affected by the different theories (e.g., 'weight' and 'length' in classical, but not in relativistic theory changes).[42]

The only position that we've seen that comes close to the (SD)'s view concerning reference change on <u>any</u> meaning change is, again, Bridgman's. If the meaning of a theoretical concept is completely determined by the set of operations used to measure or detect it, and different operations should (strictly) introduce different concepts, then <u>any</u> change in meaning (so construed) would seem to lead to a reference change. Since Bridgman explicitly rejects the old property approach to the meaning of scientific terms, his position is limited to changes in measurement. That is, he would apparently reject treating the meaning of 'dephlogistigated air' as having to do with phlogiston as metaphysical, and would instead define it via the set of operations which detect or measure it. In that case it would seem that Priestley and Lavoisier <u>could</u> be referring to the same gas, regardless of their respective theories, provided that they used the same measuring procedures for detecting it. In many cases, of course, they did use identical or similar procedures.[43]

On the other hand, there are many examples concerning similar seemingly basic and unproblematical concepts such as time, where Einstein's operations would differ from all previous treatments. Hence, <u>some</u> cases of meaning change according to Kuhn and Feyerabend might not be such cases according to Bridgman, and vice versa. Though Kuhn, for example, allows for cumulative progress within <u>normal science</u>, it is evident that there are cases of changes of measurement procedures <u>within</u> a given theory that, for Bridgman, would change the meaning and reference of the relevant concepts. Worse, Bridgman allows for a variety of evidence to point to the existence of atoms, which does not fit well with his earlier claims if they are interpreted on a strict (SD) view.

Carnap, too, at times seems to be committed to something like an (SD) view. In Meaning and Necessity, for example, he argues that 'extensions' can be reduced to 'intensions'.[44] Again, however, the terms here used are connected to the notion of 'analyticity'. That is, the long quotation analyzed earlier concerning essential properties as definatory, is much like his treatment of extension and intension in Meaning and Necessity, and not much like the Kripkean analysis of meaning and reference. In another late work, "Empiricism, Semantics, and Ontology," he makes a distinction between 'internal' and 'external' questions concerning the existence or reality of abstract entities, 'internal' and 'external' applying to the linguistic framework in which the abstract concepts occur.[45] Such a relativity of questions to a particular linguistic framework might be construed as applying to the meaning and reference of terms being relative to different theories. In fact, as was stated above, English interprets all of Carnap's later, contextualist, views as meaning change views. Still, it remains dangerous to accept such an extrapolation, since, again, these were not the issues that he was primarily concerned with in this work. It was seen that on his earlier position of the introduction of theoretical terms via reduction sentences, he might be committed to a meaning change view in certain types of cases. One of my purposes in discussing reduction sentences at such length, however, is to show that this was not a commitment that he recognized. Remember, his example of the strength of an electric current was to illustrate the possibility of scientific progress on the reduction sentence approach, as opposed to an explicit definition account.

Perhaps only Feyerabend (sometimes) straightforwardly accepts a strict (SD) view.[46] Even here, however, the emphasis is upon rejecting empiricist positions, especially the claim that the observation language is immune from meaning change. Again, if we cannot pin down the empiricist position concerning the (semantic) meaning and reference of scientific terms in straightforward current terminology, we must be careful in so interpreting a position that is constructed to refute empiricist claims. It is quite possible, in other words, that <u>no one</u> would want to support a strict (SD) view, if such a position was spelled out as it is in Kripke or Putnam. Still, such a view is often interpreted as being a position against which the causal theory of reference (CT) begins.[47] As such, it constitutes an important view concerning the meaning and reference of scientific terms, regardless of the difficulties in finding anyone who unambiguously accepts it. Clearly, if this view is accepted, scientific progress in any normal sense becomes all but impossible. Even Kuhn's modest claims for progress within 'normal science' are problematical if <u>any</u> change in the meaning of a term (however construed) constitutes a change of reference. In short, if this is the way we decide to handle the meaning and reference of theoretical terms in science, Kuhn's and Feyerabend's objections to cumulative scientific progress follow with a vengeance. So much of a vengeance, in fact, that it is not clear that <u>they</u> would accept a strict interpretation of this view.[48] Given this, and the implausibility of interpreting just <u>any</u> change of meaning of a theoretical term entailing a change of reference, the (SD) interpretation of meaning and reference need not seriously distress anyone who believes in scientific progress.

First someone would have to unambiguously propose it and second, it does not do justice to semantics or to actual scientific practice.

The cluster theory of reference (CD) agrees with the (SD) view in the sense that the reference of a term is fixed by the concepts or meanings associated with it, but allows for earlier concepts _not_ associated with the meaning or definition of the term to change without a corresponding shift in reference. That is, on a (CD) account, only some statements believed true of a theoretical term are really associated with its _meaning_ (only 'core' concepts provide necessary and sufficient conditions for class membership). _Some_ disjunction of (possibly weighted) concepts and meanings must remain intact for the reference of the term to be maintained, but not all of them. In the case of proper names, John R. Searle supports a (CD) view by claiming that 'Aristotle' refers to a certain man even if some of the identifying descriptions associated with him are false, as long as some of these descriptions remain true (the 'core' concepts).[49] If this treatment is extended as it was with the (SD) view to include general terms as well as proper names, it is a candidate for a way to interpret the meaning and reference of theoretical terms in science. On a (CD) view, Priestley and Lavoisier may or may not have been referring to the same gas within their respective theories. The ambiguity is due to the fact that they agreed concerning some of the meanings associated with the gas, and disagreed concerning others. They agreed that the gas could be isolated (more or less) in a number of ways, and that it supported combustion and respiration better than atmospheric air, etc. On the other hand, they disagreed concerning whether or not it was given off or added to a metal during the process of calcination (oxidation), whether or not it was an air usually bereft of phlogiston, etc. Whether or not they were referring to the same gas depends, of course, on how one chooses to weight these and other relevant aspects of their meaning. If it is decided that the connection with the experiments that isolated the gas are core concepts, or the presence of a number of similar qualitative properties, then the reference could be the same. If, on the other hand, it is decided that phlogiston is a crucial element in the meaning of 'de-phlogisticated air', (i.e., that it is one of the necessary and sufficient conditions for a substance to be de-phlogisticated air), then the reference would be different in the two theories. Similarly, if it is decided that the property of being an 'acidifying principle' is a core concept in Lavoisier's theory of oxygen, then he could not be referring to our oxygen.

How are such decisions made? The question of the respective weight to be placed on various meanings assigned to a term is at least partially relative to one's philosophical perspective. Carnap, for example (in footnote #34 cited in the text) argues that chemistry underwent a progression of 'intensional precision', in that after the theory(ies) of chemical elements, various properties were increasingly taken as being 'essential' to an element. 'Essential' is, again, taken as being connected with 'analyticity' for Carnap, understood as having to do with 'definatory' properties of an element. Consequently, properties such as 'having the atomic number 8' would now be taken as essential to oxygen, and earlier defining properties (e.g., atomic weight) would now be considered accidental (or, changeable without change of reference, e.g., isotopes). Of course, anyone who does not accept the notion of analyticity need not feel obliged to agree with this particular weighting of the properties. Worse, since neither Lavoisier nor Priestley had any knowledge of such properties, some would argue that Carnap's position does little to help us decide whether they were referring to the same gas. Of course, we can (and, I think should) make such a decision on the basis of present knowledge, but, as long as decisions are necessary for determining reference, no-one is compelled to accept such a view. Besides, such a decision may well constitute an arbitrary assumption as far as Kuhn is concerned. He would agree that later theories interpret their predecessors from their own subsequent perspectives, but this is, for him, to commit the terminal fallacy historically. Because there is as yet no transhistorical sense of the essential properties associated with a term, it should not simply be legislated that such and such properties are to count as determining the 'same reference' for terms across the theoretical divide.

Still, a (CD) view offers many advantages over an (SD) view. It is at least possible in many cases to account for sameness of reference across the theoretical divide from this perspective, even if some individual cases remain controversial. The weighting of associated meanings seems to be crucial to the success of the (CD) approach, however, and such weighting may well turn out to be relative to a given theory. Past decisions were due to the properties that contemporary theories associated with a given term, present decisions are due to our best theories, and future decisions will be based on future theories. As long as such decisions remain relative to contemporary perspectives, whether or not a given term refers to the same entity can receive different answers.[50] On the other hand, at least conditions have been laid down according to the (CD) view which make it possible that reference can be preserved. Even this much makes Kuhn's 'arguments' against sameness of reference deficient. If one cannot assume that reference is preserved because of the relativity of essential properties to theoretical frameworks, neither can one assume that it is _not_, due to the same relativity. Hence, his assertion that the physical reference of 'mass' is not the same in Newton and Einstein, _because_ some of the properties associated with this term are different, is not well founded. He too must deal with the problem of the weighting of relevant properties, which he has so far failed to mention. Again, of course, such an explicit treatment of meaning and reference as found in the (CD) view was not articulated by Kuhn _or_ the empiricists he was criticizing. Still, from our perspective, if he cannot maintain some of his arguments under an (SD) view, he cannot maintain others under the (CD) view. A strict (SD) view, again, would commit Kuhn to claiming that _any_ change in the properties associated with a theoretical term would result in that term's changing reference, and, consequently, it is not clear what would become of his concept of 'normal science'. On the other hand, a (CD) view simply does not allow one to assume that the physical reference of a theoretical term has changed _because_ of a change in some of the properties associated with the term, which is Kuhn's only 'argument' for the change in reference of the term 'mass' in Newton and Einstein. The strict meaning-change view is thereby weakening as the notions of meaning and reference are clarified. Already, reference change can be shown to be different from meaning change, and not a simple consequence of meaning change.

The causal theory of reference (CT) is a relatively recent addition to the literature, and is usually attributed to the work of Kripke and Putnam. It differs from both the (SD) and the (CD) accounts in that it denies the necessity of _any_ associated meanings being involved in the reference of a proper name, or general terms for classes and kinds, or, at least, it drastically changes the role these meanings play in determining the reference of a term.[51] It was seen earlier that it is a necessary consequence of the (CD) view that an object or class must have at least _some_ of the properties or meanings usually associated with it for someone to refer to it. In Searle's version, "any individual not having at least some of these properties could not be Aristotle."[52] Similarly, if a certain gas does not have _any_ of the properties (or a properly weighted subset of properties) associated with it by Priestley or Lavoisier, they could not have been referring to _that_ gas. Paradoxically, if some still undiscovered gas that they never even encountered _does_ have these properties, they were referring to _it_. If reference is determined by a set of properties associated with a term, then reference fails if these properties do not apply to any object, and is misplaced if they apply to some other, unknown, object. This seems to be a consequence of any view that claims these properties or meanings are anything like necessary and sufficient conditions for class membership.

According to Kripke, neither the (SD) nor the (CD) views adequately depict actual cases of the reference of a term, at least for proper names and natural kind terms. As long as there is an object or kind that is originally isolated via ostension and/or experimentation, subsequent expressions utilizing the name that was used to designate this object or kind refer to that object or that kind, regardless of the properties originally or subsequently associated with it. For a proper name, Kripke's version of the (CT) is presented as follows:

> Someone, let's say a baby, is born; his parents call him by a certain name. They talk about him to their friends. Other people meet him. Through various sorts of talk the name is spread from link to link as if by a chain. A speaker who is at the far end of this chain, who has heard about, say Richard Feymann, in the market place or elsewhere, may be referring to Richard Feymann even though he can't remember from whom he first heard of Feymann or from whom he ever heard of Feymann. . . . He doesn't know what a Feymann diagram is, he doesn't know what the Feymann theory of pair production and annihilation is. Not only that: he'd have trouble distinguishing between Gell-Mann and Feymann. So he doesn't have to know these things, but, instead, a chain of communication going back to Feymann himself has been established by virtue of his membership in a community which passed the name on from link to link; . . .[53]

Once such a 'baptism'[54] takes place between a name and an object, a 'causal chain' is formed between the actual object and the term(s) 'linked' to it via chains of communication. The properties associated with the object, even essential properties that the object could not lack, are not needed to determine which object is being referred to.[55]

This account of reference is, again, supposed to apply at least to proper names and natural kind terms such as 'tiger', 'water', and 'gold'; in short, any name of a thing or class whose corresponding substance is accessible to ostensive definitions. The (CT) view is founded in direct disagreement with both the (SD) and (CD) views concerning how a reference is 'picked out'. Both the (CD) and (SD) views base successful reference on what is known (or believed) about the object or class named. As described by Kripke:

> Whatever we know about them (the objects or classes) determines the referent of the name as the unique thing satisfying those properties. For example, if I use the name 'Napoleon', and someone asks 'To whom are you referring?', I will answer something like, 'Napoleon was the emperor of the French in the early part of the 19th century; he was eventually defeated at Waterloo', thus giving a uniquely identifying description to determine the referent of the name.[56]

Against this, and the (CD) view, Kripke proposes the (CT) view depicted briefly above. Introducing the notion of a 'rigid designator', as a term which refers to the same object in all possible worlds,[57] Kripke argues that neither all of the properties associated with the designated object, nor a properly weighted subset of them, determine anything like the 'meaning' of the designator. Referring to Searle's version of the (CD) view discussed in the last section, Kripke argues that the reference (designation) of 'Aristotle' is not affected by denying that he had the properties normally associated with him.

> Most of the things commonly attributed to Aristotle are things that Aristotle might not have done at all. In a situation in which he didn't do them, we would describe that as a situation in which <u>Aristotle</u> didn't do them. . . . Not only is it true <u>of</u> the man Aristotle that he might not have gone into pegagogy (and hence, not have been the 'teacher of Alexander'); it is also true that we use the term 'Aristotle' in such a way that, in thinking of a counterfactual situation in which Aristotle didn't go into any of the fields and do any of the achievements we commonly attribute to him, still we would say that was a situation in which <u>Aristotle</u> did not do these things.[58]

Similarly, if 'gold' or 'water' rigidly designates certain classes of objects, then theorists who are properly connected to these objects, directly or via a causal chain, formulate their theories about <u>those</u> objects, however different their respective theoretical accounts.

How would the (CT) view deal with the air isolated by Priestley and Lavoisier? Since the gas, once isolated, can be treated as a natural kind much like water or gold (i.e., it can be contained in a vessel, pointed to, experimented on, etc.), Priestley and Lavoisier have different theories about the same gas if they are both causally related to it. Since Lavoisier basically repeated Priestley's experiments (at least at the early stages), there can be little doubt that the gas he isolated was the same that had "astonished" Priestley. Even a (CD) view might come to this conclusion, depending, again, on how one chooses to weight the properties. A (CT) view, of course, has no problems with ascribing the same designation to 'de-phlogisticated air' and 'oxygen', given the causal relation between the two theorists and a certain 'stuff'.

Kuhn argues that it is quite impossible to clearly establish who discovered the gas, or when it was discovered. Part of his claim is based on the rather vague concept 'discover'. Are we to say that someone discovered 'x' if they do not realize what they discovered? Or should the credit for a discovery go only to someone who recognizes something approximating the real significance of what he discovered? These questions do not affect the reference of a term, at least according to a (CT) view. Kuhn's implicit commitment to one of the other views is once again evident in the following passage.

> Priestley's claim to the discovery of oxygen is based upon his priority in isolating a gas that was later recognized as a distinct species. But Priestley's sample was not pure, and, if holding impure oxygen in one's hands is to discover it, that had been done by everyone who ever bottled atmospheric air. Besides, if Priestley was the discoverer, when was the discovery made? In 1774 he thought he had obtained nitrous oxide, a species he already knew; in 1775 he saw the gas as de-phlogisticated air, which is still not oxygen or even, for phlogistic chemists, a quite unexpected sort of gas.[59]

Some of these claims involve what is meant by 'discover' (as opposed to reference), some, as we'll see, are nonsense, and the last implies that the isolated gas was not oxygen, precisely because Priestley thought it was de-phlogisticated air.

The problems with 'discover' do not concern us here. The nonsensical part of the above quote is Kuhn's claim that anyone who bottled atmospheric air would have an equal claim with Priestley to having discovered oxygen. If this is changed to having referred to what we call oxygen, no one who merely captured atmospheric air in a bottle was causally related to oxygen (any more than the discoverer of any medium sized physical object could be said to be referring to its molecules). By mid-March of 1775, Priestley was quite aware that the gas he had isolated was 'purer' than atmospheric air. After a nitrous air test, and after observing the behavior of a mouse placed in the isolated sample, he was fully satisfied that the sample "was much better than common air."[60] Finally, why in the world would Kuhn argue that the isolated air, 'seen' as dephlogisticated air, was not oxygen, unless he thinks that statements-believed-true about a substance determine the reference of the term used to designate that substance? Clearly, he is advocating at least a (CD) view here, and such a position has already been shown to render his 'arguments' concerning physical reference worthless. In any event, it seems much more natural to hold that if Priestley and Lavoisier isolated the same gas, their respective statements concerning their experiments are statements about it, which is unproblematical on the (CT) view.

In a recent article, Philip Kitcher uses what he calls (along with Donnellan) a 'historical explanation theory' of reference, which includes most of the salient features of a (CT) view, to argue against

Kuhn's and Feyerabend's conceptual relativism, emphasizing the case of Priestley.[61] He is concerned in this article with developing a methodology, through the concept of reference, that would enable philosophers and historians to determine "what Priestley was talking about, and how much of what he said is true."[62] The problem that Kitcher sees as central to Kuhn's and Feyerabend's positions is how to reconcile two apparently inconsistent philosophical intuitions. On the one hand it seems that if there is no such thing as phlogiston, then there is no such thing as 'de-phlogisticated air', and statements made by Priestley using this term would then fail to refer.[63] On the other hand, "we are also tempted to suppose that Priestley and Cavendish used the terms 'de-phlogisticated air' and 'phlogisticated air' to refer, and that they made some true utterances using these terms."[64] These latter would include the various statements of Priestley containing 'de-phlogisticated air' that described his experimental results, and agree with propositions that Lavoisier (and we for that matter) rightly ascribe to oxygen (e.g., de-phlogisticated air supports combustion better than atmospheric air). The resolution of these apparently contradictory intuitions according to Kitcher is to "allow that different tokens of 'de-phlogisticated air' refer differently."[65]

To accomplish this, an account of reference and translation is necessary which allows for different tokens of the same term to have different referential status. A 'historical explanatory theory' of reference can, according to Kitcher, provide such an account. The following quotation should begin to clarify how it accomplishes this, and what some of the similarities and differences are that it has with the (CT) view outlined above.

> The general theory of reference which I espoused above proposes that the referent of a token is the entity which figures in an appropriate way in a historical explanation of the production of that token. An explanation of the production of a token will consist in a description of a sequence of events whose first member is either an event in which the referent of the token is causally involved, or an event which involves the singling out, by description, of the referent of the token. . . . Priestley's early utterances of 'de-phlogisticated air' were initiated by an event in which Stahl specified phlogiston as the substance emitted in combustion. After Priestley had isolated oxygen and misidentified it, things changed. His later utterances could be initiated either by the events in which Stahl fixed the reference of 'phlogiston' or by events of quite a different sort, to wit, encounters with oxygen. Thus we can answer the question of how different tokens of a scientific term can refer to different entities by supposing that the production of different tokens can be initiated by different events.[66]

In other words, when Priestley uttered statements concerning 'de-phlogisticated air' that merely espoused Stahl's (fallacious) theory, these statements failed to refer. On the other hand, when he uttered statements concerning 'de-phlogisticated air' that were initiated by his own experiments with the new 'factitious air' (oxygen), they referred to <u>that</u> gas, and were right or wrong depending on what he claimed about <u>it</u>.

The influence of the (CT) view on Kitcher's account of how Priestley sometimes referred to oxygen, and said true things about it, should be clear. He was, in these instances, directly causally related to what we call oxygen. Other instances of the use of the term 'de-phlogisticated air' by Priestley, however, require a different referential account. There he was related via a causal chain to Stahl's <u>use</u> of 'phlogiston', and, since <u>it</u> failed to refer, so did Priestley's derivative from it. This account of the reference of individual tokens thereby shares the (CT) view's machinery of direct causal relations to an object, and indirect relations through a causal chain to earlier 'baptisms'. It is different from the (CT) view outlined above, however, in two respects. First, and less important (for now), this is the first treatment we've considered of an initial 'baptism' of a non-existent object. Some philosophers argue that a (CT) view is still the correct theory of references for such cases, and others disagree. In any event, since it is at least more difficult to account for the isolation of or ostensive relation to a non-existent object, it will be correspondingly more difficult to elucidate a (CT) view in such cases. Second, and more important (for now), the necessity of considering the context of individual tokens of a term illuminates an entirely different problem concerning Kitcher's treatment of the historical explanatory theory of (semantic) reference.

In a work which is often taken as one of the precursors of the (CT) view, Keith Donnellan argued that definite descriptions have two different functions for determining the referent of a term, one 'attributive' and one 'referential'.

> A speaker who uses a definite description attributively in an assertion states something about whoever or whatever is the so-and-so. A speaker who uses a definite description referentially in an assertion, on the other hand, uses the description to enable his audience to pick out whom or what he is talking about and states something about that person or thing.[67]

An example Donnellan uses to illustrate this difference is 'Smith's murderer is insane'. On the one hand, due to the grisly nature of the crime, a police chief might use this description to refer to whoever committed this terrible deed. On the other hand, a juror after the trial of Shlomo, convicted of the crime, may say 'Smith's murderer is insane' to his wife, since he often observed Shlomo drooling and rolling his eyes during his trial, and thereby use the description 'Smith's murderer' to pick out Shlomo (even though, let's say, Shlomo was framed). In the first case, for Donnellan, the description refers to whoever satisfies the description, in the second it picks out Shlomo, whether the description is satisfied by him or not. As with Kitcher's account, we must know something about the context of the utterance (or token) to tell which is the case (i.e., who it is that the term denotes). We could not, for example, decide whether the description, printed on a piece of paper, was used referentially or attributively. If reference is taken to be a semantic notion, can such relativity to individual contexts be tolerated?

Kripke argues, rightly I think, that such relativity is not appropriate for semantic treatments of reference.

> In a given idiolect, the semantic referent of a designator (without indexicals) is given by a general intention of the speaker to refer to a certain object whenever the designator is used. The speaker's referent is given by a specific intention, on a given occasion, to refer to a certain object. If the speaker believes that the object he wants to talk about, on a given occasion, fulfills the conditions for being the semantic referent, then he believes that there is no clash between his general intentions and his specific intentions. My hypothesis is that Donnellan's referential-attributive distinction should be generalized in this light.[68]

This 'speaker's referent' belongs not to semantics, but to the field of linguistic studies generally called 'pragmatics'. Cases like Donnellan's referential use of descriptions, or any case of misdescribing that may be said to (via the principle of charity) still pick out a certain person or object, depend upon the relevant context for the reference to work. Semantically, on the other hand, the reference must fail in all cases of misnaming, since the truth of the uttered sentence should not depend on individual contexts. From this point of view Priestley may well have been talking about oxygen, since the (CT) view depends upon the causal relation between the act of naming and the object named, which he seems to have fulfilled via his experiments. Still, against Kitcher's position, none of his statements using 'de-phlogisticated air' were (semantically) true, since 'de-phlogisticated air' is semantically related to a non-existent entity, phlogiston. That is, the possibility of making a charitable translation of a past statement into currently respectable parlance is not sufficient to render the original statement true. If I, for example, utter the statement 'Bill seems to be an awfully jealous man' after having talked to Frank and (mistakenly) taken him to be Bill, witnesses may very well charitably construe my utterance as being (really) about Frank. Still, this does not render 'Bill seems to be an awfully jealous man' true. Semantically, the truth of statements needs to be established via the statement type, while, pragmatically, tokens can be gerrymandered via the principle of charitable translation. In other words, while Kitcher's account certainly succeeds in breaking down Kuhn's and Feyerabend's objections to different theories being about the same thing, it is not a theory of semantic reference. This does no damage to the notion of scientific progress made possible according to such a view, but it does necessitate a long overdue clarification of terminology.

In a recent, unpublished paper, Lawrence Poncinie uses a distinction between what a name denotes and what a speaker refers to[69] that strikes me as being important for unravelling the semantics/pragmatics muddle engendered by much of the current literature. If the difference between meaning and reference is now usually understood through the works of Kripke and Putnam, the notion of reference remains ambiguous. It is not just a matter of how one can determine which object is picked out by a name or a description, but also a matter of how general the referential relation depicted by a statement is. If Priestley can be judged to have said true things about oxygen, this is only because of a charitable translation of phlogiston talk into oxygen talk, not because of the referential status of 'de-phlogisticated air' itself. It would probably greatly aid current discussions of meaning and reference change in science if such a distinction between semantic reference and denotation was kept separate from speaker's reference and practical concerns over what a historical figure meant to say, or, given his or her terminology, how best to translate the historical figure's statements into current terminology. For a historian, the latter enterprise may be all that really matters. For a philosopher, however, whose tools of the trade include notions like semantics and pragmatics, such a distinction seems to be crucial. Extreme meaning-change views are in jeopardy regardless of which treatment is followed, but questions in philosophy of language remain obscure unless some kind of demarcation is clearly and consistently adhered to.

If a (CT) view seems to be able to answer Kuhn's and Feyerabend's claims about reference change of scientific concepts as long as the concepts behave like proper names or natural kind terms, what about strictly theoretical terms? Here it is a bit more difficult to determine how the causal theory can be of help, at least as it has been outlined so far in this chapter. Kripke provides very little detail concerning how such terms are handled by his theory of reference. When he discusses such concepts as 'electromagnetic waves', or 'streams of photons' he does so in the context of discussing such scientific identity statements as "light is a stream of photons, that water is H_2O, that lightning is an electrical discharge, that gold is the element with the atomic number 79."[70] We are never told how 'photons' or 'atomic numbers' are isolated and picked out in analogy to the objects denoted by proper names or natural kind terms.[71] Instead, Kripke is concerned with ascertaining whether such identity statements, if true, are necessary or contingent. He argues that they are necessary, even though they are empirically discovered, and this claim has produced a great deal of current interest in physical necessity and scientific realism. Still, whether or not current 'atomic theory' (if there is one such theory) is an improved successor of Dalton's, or even Bohr's, 'atomic theory', seems problematical if we don't have a referential (denotational) account of the theoretical concept of an atom. On the face of it, it does not appear that anything like an initial 'baptism' is possible in such cases, if there is no ostensible <u>thing</u> to name. Worse, can reference or denotation be as theory-independent in such cases as it appears to be on the (CT) view with proper names and natural kind terms? Do we have anything like 'photons' picked out except against a large background of theoretical commitments? If not, are not properties and meanings believed true of the class of photons necessary for referring to or denoting that class? In short, if a child is born, or a type of medium sized physical object is discovered, the 'baptism' machinery discussed by Kripke seems to be adequate to initiate the 'causal chain' necessary for the (CT) view. A theoretical entity, on the other hand, is usually introduced as a postulate, or as a possible explanation of certain experimental data subject to further verification. It is thus not clear how the baptism machinery functions in the case of a theoretical postulate.

Putnam seems to consider the referential status of such terms. Initially, such terms are treated exactly like names or natural kinds on a (CT) view.

> Bohr would have been referring to electrons when he used the word 'electron', notwithstanding the fact that some of his beliefs about electrons were mistaken, and we are referring to those same particles notwithstanding the fact that some of our beliefs--even beliefs included in our scientific 'definition' of the term 'electron'--may very likely turn out to be equally mistaken.[72]

The question is, of course, how are we referring to the same thing in this case? Well, for Putnam, if I were standing next to Benjamin Franklin when he conducted one of his experiments on electrical phenomena (e.g., his famous 'kite' experiment), I could be causally linked to the event (his experiment), and hence acquire the term 'electricity'.

> It is clear that each of my later uses will be causally connected to this introducing event, as long as those uses exemplify the ability I acquired in that introducing event. . . . If I teach the word to someone else by telling him that the word 'electricity' is the name of a physical magnitude, and by telling him certain facts about it which do not constitute a causal description--for example, I might tell him that like charges repel and unlike charges attract, and that atoms consist of a nucleus with one kind of charge surrounded by satellite electrons with the opposite kind of charge--even if the facts I tell him do not constitute a definite description of any kind, let alone a causal description--still, the word's being in his vocabulary will be causally linked to its being in my vocabulary, and hence, ultimately to an introducing event.[73]

The above, of course, is exactly parallel to the linking via a causal chain of later uses of a name to an initial 'baptism'. What, however, is picked out by the introducing event in this case if my 'facts about it' are not necessary and sufficient conditions for membership in the class of electrical phenomena? For Putnam, there seem to be <u>some</u> sort of meanings associated with 'physical magnitude terms' like 'electricity' or 'mass', even if these do not comprise necessary and sufficient conditions for class membership. There is, in other words, some 'minimal linguistic information'[74] associated with the terms, and, together with the causal connections in a community's use of the term, this minimal information enables one to ascribe the same referent to the term. The 'minimal linguistic information' that Putnam thinks is necessary involves <u>causal</u> descriptions "because physical magnitudes are normally discussed through their effects."[75] In other words, 'electricity' is picked out if, together with the right causal connection to an original 'baptism' (or some such), it is viewed as the cause of certain experimental effects, however the current theory may otherwise describe the nature of electricity ('flow' of electrons as opposed to an 'imponderable fluid', etc.). A similar account holds for other types of theoretical terms in science.

Still, problems remain for the (CT) view in such cases. For one thing, we still are not told <u>what</u> the introducing event introduces, except a <u>use</u> of the term under certain circumstances. Franklin conducted an experiment, for example, and called the cause of the experimental results 'electricity'. Still, the 'cause' was not isolated, nor is it in any normal sense ostensive. Also, if the causal chain links successive speakers to a <u>use</u> of a term, instead of an object, we get a causal account of the same <u>use</u>, but not necessarily of the same reference (this is related to, but not identical with the pragmatic/semantic problem in Kitcher's account of Priestley). Kripke, at least, does not seem to want to be committed to this kind of causal account. Nor does he seem to want to be committed to any minimal linguistic information being necessary for physical reference. One could be totally wrong about, for example, whether Aristotle is male, or a teacher, or a Greek, and still denote him for Kripke. If theoretical terms operate differently, it is not clear how they are different, or whether a causal account is appropriate for them.

If we can be totally wrong about theoretical entities, yet still refer to them, the situation may well prove to be even worse for a causal account. Consider an example Kripke uses in a different context:

> Neptune was hypothesized as the planet which caused such and such discrepancies in the orbit of certain other planets. If Leverrier indeed gave the name 'Neptune' to the planet before it was ever seen, then he fixed the reference of "Neptune" by means of the description just mentioned.[76]

Kripke goes on to say that the statement "if such and such perturbations are caused by a planet, they are caused by Neptune" becomes, in this case of fixing a reference via a description, an apriori truth. Still, he argues, it is not a necessary truth, since various counterfactual situations would have changed the description (i.e., it was not a definition).[77] But what about the supposed planet 'Vulcan' that was supposed to account for a similar deficiency in Newtonian planetary theory?[78] If the cause of the advance in the perihelion of Mercury had to be a planet, then 'Vulcan' failed to refer, but, then it seems that at least the minimal linguistic information that Vulcan is a planet is necessary for determining what, if anything, 'Vulcan' referred to. On the other hand, if 'Vulcan' stands indifferently for 'the cause of the perturbation of Mercury's orbit', then Leverrier might be taken to have been referring (unknowingly, of course) to some aspect of General Relativity! It does not seem that the (CT) view can have it both ways. Either some added information is needed for the reference of theoretical terms, or they are simply causally related to the results of experiments (i.e., 'Vulcan' is _whatever_ causes the advance in the perihelion of Mercury, etc.). If the first horn is accepted, then some amendments will need to be tacked on to the (CT) view when theoretical terms are involved (what _kinds_ of minimal linguistic information?), and it will then have to be separated from a (CD) view by some other criteria than the 'baptism' machinery. If the second horn is accepted, the (CT) view will run into serious conflict with our intuitions. Just as the (SD) view seemed suspect partially because of its anamolous results (I not only am not talking about Aristotle if my descriptions do not fit _him_, I _am_ talking about whomever they _do_ fit), the (CT) view would, on this reading, have past theorists talking about current (or even future) explanatory causal mechanisms. It seems that the first horn is much more acceptable, so what would be needed to make the (CT) view viable for all scientific terms?

Michael E. Levin has argued that questions about meaning change and reference change must be kept separate, and, contra Kuhn and Feyerabend, the untranslatability of a term does not lead to a difference in physical reference.[79] As an example, he uses the Greek term 'atomoi' (ατομοι), and considers how they could be _wrong_ about the very things later theorists called 'atoms'. He concludes that a (CT) view can account for the two terms, which are not, perhaps, intertranslatable, still referring to the same things. "According to the CTR (CT), the Greeks were using 'atomos' to refer to whatever explains the facts that explain the observations that explain the Greek's coinage of 'atomos'."[80] In other words, some of the same phenomena that led the Greeks to postulate ατομοι, led modern theorists to postulate atoms, and, hence, the two terms can refer to the same thing. It is important to use 'refer to' here, instead of 'denote', because Levin's account creates the same problems for semantic denotation as did Kitcher's. Again, I would like to reserve the technical semantic concept 'denote' for cases involving sentence <u>types</u>, <u>not</u> contingent upon contextual information involving individual <u>tokens</u>. When causal chains are used to unpack <u>uses</u> of terms, they lose their claim to being a semantic tool. Worse, Levin's account does not explain how a (CT) view can account for <u>what</u> is picked out by 'ατομοι' or 'atoms'. Since neither refers to (<u>or</u> denotes) ostensive entities, we are back to the problem outlined in the last paragraph.

Berent Enc has attempted to amend the (CT) view to deal with the problem of non-ostensive scientific terms.[81] Enc claims, as I have above, that the referential apparatus for 'gold' or 'cat' is not like that for 'caloric' or 'magnetic field', precisely because the objects named by the latter terms are not ostensive.[82] Rather than accepting the use of descriptions to fix a reference for <u>whatever</u> causes the effect x, Enc thinks that the referential apparatus for non-ostensive terms must include the 'kind-constituting properties' attributed to the object, and the 'explanatory mechanism' developed in the relevant theory or theories.[83] Non-ostensive terms, in other words, lack the indexical element the (CT) view utilizes to determine the referent for proper names and natural kind terms, and instead require some 'set of beliefs' to determine their reference (if any).[84] To determine the referent for a non-ostensive term, we must know something about the kind of thing the theory postulates this referent to be, as well as how such a kind of thing could explain the effects for which it was introduced.

This, I think, is to restrict the conditions of the reference of theoretical terms too drastically, especially by adding the condition of the causal account of <u>how</u> the theoretical kind brings about its effects. Such a restriction would, for example, entail that Franklin's 'positive' and 'negative' electricity would not be the same as ours, though we use the same terms, and, in many cases, simple experiments are explained in similar ways. It is, of course, <u>arguable</u> that his theoretical kinds are not the same as ours, but this should not <u>follow</u> from a given treatment of reference. Generally, it does not seem proper that a theory of reference determine our intuitions about sameness of reference, but should, rather, match these intuitions once they have been established. More importantly, theoretical terms are often introduced (or, if pre-existent, incorporated into a new theory) without detailed causal analyses of <u>how</u> they accomplish their effects (e.g., 'gravity' in the <u>Principia</u>). This should not, in itself, render such terms non-referential, or even referentially vague. Still, <u>some</u> kind of theoretical associations do seem to be necessary for determining the reference of theoretical kinds. Enc's restrictions simply seem to be too strong.

From a different perspective, Hartry Field has argued that referential semantics, whether from a (CT) or some other perspective, are not sufficient for establishing the truth or falsity of statements within a theory. Some of the statements in Newton's theory, for example, can be assigned truth values even though there is no unambiguous determination of what some of the theoretical terms denote.[85] Different current physics texts, for example, contain treatments of 'mass' that, depending on the way it is formulated, has or does not have the same value in all reference frames ('relativistic mass' = total energy/c^2, 'proper mass' = nonkinetic energy/c^2).[86] If current texts allow for different formulations of 'mass', which one, if either, can Newton be said to have denoted? Field argues that this question is undecidable, that pre-relativistic uses of 'mass' were 'referentially indeterminate'.

> Before relativity theory was discovered . . . the word 'mass' was referentially indeterminate: it did not lack denotation, in any straightforward sense; on the contrary, there are two physical quantities that each satisfy the normal criteria for being the denotation of the term.[87]

This is not to be construed as a failure on our part to determine which of these pre-relativistic 'mass' is denoted, but rather, there is no fact of the matter.

According to Newton, momentum can be determined as the product of mass and velocity, and this 'mass' has the same value with respect to different frames of reference. After relativity theory, both of these statements cannot be true. Either the conservation of momentum is violated if velocity is held constant, or the invariability of mass is violated if the conservation of momentum is maintained.[88] If 'mass' is taken as relativistic mass then momentum is conserved while mass no longer remains constant, and vice versa if 'proper mass' is used.[89] Hence, for Field, we know that the conjunction of the two claims concerning 'mass' in Newton is false (i.e., has a determinate truth value), but, we know neither which conjunct is false, nor which current 'mass' Newton denoted. The principle of Charity for translation also won't help according to Field, since both types of 'mass' play about equally important roles in relativity theory (as did the two claims concerning 'mass' in Newton's theory). Nor will factors such as 'simplicity' help determine our translation, since different equations will be simpler, depending on which type of 'mass' is employed.[90] Nor, is it compatible with the most reasonable assignment of truth values to Newton's statements to assume that Newton wasn't denoting anything with his use of 'mass'. It seems true, for example, that it takes more force to move a body with more mass, and how could this statement be true if 'mass' is like Santa Claus or phlogiston?[91]

Field offers a solution by distinguishing between 'denoting' and 'partially denoting'. Newton's term 'mass', for Field, partially denoted both 'proper mass' and 'relativistic mass', and didn't fully denote either. The consequence of this distinction is to allow us to accept the part of meaning-change views that claims that we can't always equate the same term in two different theories, while still rejecting the claim that the theories are incommensurable. If we allow for a previous term to partially denote later treatments of the same term, we can determine (some) truth values across the conceptual divide (e.g., it does take more force to move an object with more mass in both theories), without claiming that there must be total determinacy of denotation in the earlier case.[92] In short, we can translate truth values of some statements, without being able to strictly determine the denotation of their component theoretical terms, as long as we can assign at least partial denotation to them in terms of later theories. Hence, theories of reference (note: no particular theory was employed by Field) may not be crucial for establishing the truth of theoretical statements (i.e., scientific realism) as has often been assumed.

This is, I think, the most important insight in Field's article, regardless of the adequacy of his treatment of the particular case of 'mass'. Partial denotation, of course, cannot replace denotation in semantic theory, being parasitic on the prior notion.[93] Still, the tight link in semantic theory between assignment of truth value and the denotation of terms seems to be partially sundered on Field's account. This is, I think, as it should be, especially concerning science. Theories of reference cannot establish the truth or falsity of theoretical statements, or whether theoretical terms have a denotation. Rather, they should be compatible with whatever reasons we already have for believing or not believing theoretical claims. Edwin Berk has argued that (CT) views, for example, while supporting some realist desiderata, undermine others.[94] Some realists stress the referential (denotational) aspect of terms in a mature science, and the (CT) view can help to show how this can be defended (although, as has been shown, amendments are needed for theoretical terms). (CT) views, as has already been shown (as well as (CD) views) allow for the transtheoretical reference of scientific terms. On the other hand, insisting that the properties associated with a theoretical term have no real bearing on the denotation of that term can actually undermine a realist position stressing the importance of evidence for a given theory (the position defended in this dissertation). For Kripke, of course, essential properties of a natural kind term cannot be lacking if an object is to belong to that kind. Still, epistemologically, what these essential properties are, and what our evidence for them is, is not only not a denotational matter, it is a theoretical matter. By apparently downplaying the role of theories in determining the extension of theoretical kinds (at least), the (CT) view may actually undermine our chances of making realist claims concerning them. Hence, the amendments needed for the (CT) view to adequately handle theoretical kinds briefly addressed above have more than technical 'mopping up' importance.

Peter Smith uses some of these considerations to argue that an amended (CD) view would be more germane to scientific realism and scientific progress than an amended (CT) view.[95] While I think that a yet-to-be-formulated amendment of the (CT) view is more promising (it does, after all, nicely handle natural kind terms in science), the real lesson of the above discussion is that an adequate defense of scientific realism as here formulated is not to be found in any denotational approach.

Briefly, while there are more than one amended theory of denotation that can undermine anti-progress, anti-realist objections as found in writers such as Kuhn and Feyerabend, establishing that we should believe any theory, past or present, involves our evidence that the relevant theoretical terms _are_ referential. This is, of course, not reasonably to be expected from theories of denotation, but from theories of scientific verification and theory testing. Whether we should believe that theoretical statements are true, _including_ whether or not their component terms at least partially denote, has to do with what amount and type of _evidence_ we have for the theory in question _at a particular time_. The next chapter will introduce my account of how the 'virtues' of scientific theories can provide the framework for addressing these issues.

NOTES

[1] I. Bernard Cohen, Franklin and Newton: An Inquiry into Speculative Newtonian Experimental Science and Franklin's Work in Electricity as an Example Thereof (Philadelphia: The American Philosophical Society, 1956), pp. 285-286. Thomas S. Kuhn, The Structure of Scientific Revolutions (Chicago: University of Chicago Press, 1970, 2nd ed.), pp. 13-15.

[2] Later I will argue that this constitutes one of the important senses in which Franklin's theory was 'on the right track', and I will provide a number of examples of the parallel terminology at that point.

[3] This way of putting the problem is, at this point, purposefully vague. Since the terminology of current theories of reference will only be introduced later in this chapter, the earlier discussions of the problem will incorporate the vagueness of the earlier treatments. I will usually incorporate the terminology of whatever author I am presently discussing, until I introduce the current terminology. As will be shown later, this very vagueness (due to differences of orientation) plays a rather important historical role.

[4] Especially in the extremely influential Structure of Scientific Revolutions.

[5]Ibid., p. 102.

[6]Again, while I would want to separate semantical questions such as 'meaning' from contextual (pragmatic) questions such as 'belief', I will use the less precise treatment at present, to be supplemented later.

[7]Larry Laudan, for example, attempts to formulate scientific 'progress' in terms of problems solved, avoiding some pitfalls allegedly contained in traditional empiricist approaches. _Progress and Its Problems: Towards a Theory of Scientific Growth_ (Berkeley: University of California Press, 1978). For that matter, even Kuhn maintains a sense of 'progress' through revolutions, which, however, is not as clear as one would like. Cf. Kuhn, _op. cit._, pp. 160-73, and the 1969 Postscript added to the second edition, pp. 198-204.

[8]Paul K. Feyerabend, "Explanation, Reduction, and Empiricism" in Herbert Feigl and Grover Maxwell, eds., _Scientific Explanation, Space, and Time_ (Minneapolis: University of Minnesota Press, 1971/1962), pp. 80-81.

[9]Ibid., p. 81.

[10]A. J. Ayer, _Language, Truth, and Logic_ (New York: Dover, 1952), p. 35.

[11]P. W. Bridgman, _The Logic of Modern Physics_ (New York: MacMillan, 1961/1927).

[12]Ibid., p. 1.

[13]"The attitude of the physicist must therefore be one of pure empiricism. He recognizes no _a priori_ principles which determine or limit the possibilities of new experience." Ibid., p. 3.

[14] Ibid., p. 4.

[15] In this sense Bridgman is already at variance with what many current authors take to be the crux of a 'traditional theory of reference'. If such a theory is supposed to maintain that the properties associated with a term provide necessary and sufficient conditions for membership in the class which the term denotes, then denying that scientific terms should be defined via properties is at least a partial rejection of this view. On the other hand, as will be shown, there is a reading of Bridgman's treatment of the meaning of theoretical concepts that places him within the spirit, if not the letter, of this traditional view.

[16] The Logic of Modern Physics, p. 5.

[17] "We must demand that the set of operations equivalent to any concept be a unique set, for otherwise there are possibilities of ambiguity in practical applications which we cannot admit," ibid., p. 6. "If we have more than one set of operations, we have more than one concept, and strictly there should be a separate name to correspond to each different set of operations," ibid., p. 10. "To say that a certain star is 10^5 light years distant is actually and conceptually an entire different kind of thing from saying that a certain goal post is 100 meters distant," ibid., p. 18.

[18] Cf. the footnote by Robert W. Lawson in Albert Einstein, Relativity: The Special and General Theory (New York: Crown, 1961/1916), p. 48. Clearly the bombardment of elements by alpha-particles used to confirm the Einsteinian equation '$E=mc^2$' is not accessible to the weighing of these particles before the bombardment takes place.

[19]Albert Einstein, "The Electrodynamics of Moving Bodies" in Albert Einstein, H. A. Lorentz, H. Weyl, and H. Minkowski, _The Principle of Relativity: A Collection of Original Memoirs on the Special and General Theory of Relativity_ (New York: Dover, 1952/1923, Einstein's paper first published in 1905), p. 63.

[20]_The Logic of Modern Physics_, p. 103.

[21]Ibid., p. 59.

[22]Rudolf Carnap, "Testability and Meaning," _Philosophy of Science_, vol. 3, no. 4, Dec. 1936, and vol. 4, no. 1, Jan. 1937. Reprinted in Herbert Feigl and May Brodbeck, eds., _Readings in the Philosophy of Science_ (New York: Appleton-Century-Crofts, 1953), p. 52. References that follow will be from this reprint. I have added the quantifier since this form will be more familiar to most readers.

[23]Ibid., pp. 53-56.

[24]Ibid., pp. 53-54.

[25]Ibid., p. 54.

[26]Ibid., p. 56.

[27]Ibid.

[28]Ibid., p. 59.

[29]See, for example, Carl G. Hempel, "Implications of Carnap's Work for the Philosophy of Science," in Paul A. Schilpp, ed., _The Philosophy of Rudolf Carnap_ (Lasalle: Open Court, 1963), esp. pp. 689-91.

[30]Jane English, "Partial Interpretation and Meaning Change," _Journal of Philosophy_, vol. 75, no. 2, Feb. 1978.

[31]Ibid., p. 61.

[32]Ibid.

[33] Ibid., pp. 62-63.

[34] Ibid., p. 62.

[35] See, for example, Rudolf Carnap, "The Methodological Character of Theoretical Concepts," in Herbert Feigl and Michael Scriven, eds., The Foundations of Science and The Concepts of Psychology and Psychoanalysis (Minneapolis: University of Minnesota Press, 1976/1956). "The principle of operationism, which was first proposed in physics by Bridgman and then applied also in other fields of science, including psychology, had on the whole a healthy effect on the procedures of concept formation used by scientists. The principle has contributed to the clarification of many concepts and has helped to eliminate unclear or even unscientific concepts. On the other hand, we must realize today that the principle is too narrow," p. 65. "Today I think, in agreement with most empiricists, that the connection between observation terms and the terms of theoretical science is much more indirect and weak than it was conceived either in my earlier formulations or in those of operationism," p. 53.

[36] English analyzes these later trends in the latter parts of her paper, esp. pp. 64-74. That this contextualist move was general for empiricists is made clear in Carl G. Hempel, "Empiricist Criteria of Cognitive Significance: Problems and Changes," in Aspects of Scientific Explanation: and Other Essays in the Philosophy of Science (New York: Free Press, 1965), pp. 113-18.

[37] Rudolf Carnap, "Meaning and Synonymy in Natural Language," Philosophical Studies, vol. 6, no. 3, April, 1955, p. 41. Emphasis added.

[38]Jerrold J. Katz, "The Neo-Classical Theory of Reference," in Peter A. French, Theodore E. Uehling, Jr., and Howard K. Wettstein, eds., Contemporary Perspectives in the Philosophy of Language (Minneapolis: University of Minnesota Press, 1979), p. 104.

[39]Stephen P. Schwartz, Introduction to Naming, Necessity, and Natural Kinds (Ithaca: Cornell University Press, 1977), p. 13.

[40]Joseph Priestley, Experiments and Observations on Different Kinds of Air: and Other Branches of Natural Philosophy Connected With the Subject (Birmingham: Thomas Pearson, 1790), vol. II, part I, p. 120.

[41]Antoine Lavoisier, Elements of Chemistry: In a New Systematic Order Containing All the Modern Discoveries (Philadelphia: Mathew Carey, 1799, 4th ed., trans. by Robert Kerr, 1st ed., London, 1789). "We must therefore, in every acid, carefully distinguish between the acidifiable base, which Mr. de Morneau calls the radical, and the acidifying principle, or oxygen," pp. 114-15.

[42]Cf. Peter Achinstein, in Concepts of Science: A Philosophical Analysis (Baltimore: Johns Hopkins, 1971/1968), makes a similar claim about 'position' and 'time' in Bohr's theory and in classical mechanics. "Both theories use the same set of spatial and temporal concepts. Whatever conditions are semantically relevant for a particle having a position at a given time are the same for the Bohr theory as for classical mechanics," p. 102.

[43]Lavoisier, op. cit., p. 520. Here he describes one apparatus for collecting airs that he ascribes to Priestley. Of course, both men performed a variety of experiments, so the above claim will not always hold.

[44] Rudolf Carnap, Meaning and Necessity: A Study in Semantics and Modal Logic (Chicago: University of Chicago Press, 1975/1947), pp. 90-95.

[45] Rudolf Carnap, "Empiricism, Semantics, and Ontology" reprinted in Meaning and Necessity, p. 206.

[46] Cf., for example, Paul K. Feyerabend, "Problems of Empiricism," in R. G. Colodny, ed., Beyond the Edge of Certainty (Englewood Cliffs: Prentice Hall, 1965), p. 180.

[47] Cf., Stephen Schwartz, op. cit., pp. 13-14.

[48] For Kuhn, as we've seen, this would render any notion of progress, even within Normal Science, problematical. For Feyerabend, such a view would make it difficult to understand his claim that (some) competing theories can be compared. See, "Explanation, Reduction, and Empiricism," op. cit., pp. 65-70.

[49] John R. Searle, "Proper Names," Mind, vol. 67, 1958, reprinted in Charles E. Caton, ed., Philosophy and Ordinary Language (Urbana: University of Illinois Press, 1963). "It is a necessary fact that Aristotle has the logical sum, inclusive disjunction, of properties commonly attributed to him: any individual not having at least some of these properties could not be Aristotle," p. 160.

[50] There are other problems with the cluster view that will be considered when I discuss causal theories of reference.

[51] Putnam criticizes what he takes to be Kripke's view that no meanings need be associated with a term as being too liberal. His own view is that for 'linguistic competence', a speaker must know some of the properties usually associated with a term (stereotype), even if

these properties misdescribe the actual kind. I may not, of course, be considered 'linguistically competent' with respect to the name 'Aristotle' if I think that Aristotle was a Greek god. Still, I seem to be wrong about Aristotle, rather than simply failing to refer. In short, questions concerning learning a language, or 'linguistic competence', while certainly important in philosophy of language, may not be central to a theory of reference (denotation). In any event, the above considerations motivated me to add the qualification on the role of meanings according to a causal theory at this point. Putnam's view will be discussed in more detail later.

[52]Searle, op. cit.

[53]Saul A. Kripke, Naming and Necessity (Cambridge: Harvard University Press, 1981), p. 91. Originally published in D. Davidson and G. Harmon, eds., Semantics of Natural Language (Boston: D. Reidel, 1972).

[54]"A rough statement of a theory might be the following: An initial 'baptism' takes place. Here the object may be named by ostension, or the reference of the name may be fixed by a description," ibid., p. 96.

[55]"Don't ask: how can I identify this table in another possible world, except by its properties? I have the table in my hands, I can point to it, and when I ask whether it might have been in another room, I am talking, by definition, about it," ibid., pp. 52-53. It might be added, as in the previous quote in the text, as long as someone had the object 'in her hands', or could point to it, etc., I am talking about that object if I am linguistically linked to that ostensive situation.

[56]Ibid., p. 28. Putnam also sees the traditional views of reference as being entwined with questions concerning the knowledge of

the properties of the object to which the name is applied. "On a traditional view, any term has an intension and an extension. 'Knowing the meaning' is having knowledge of the intension. . . . understanding words is a matter of having knowledge. . . . According to the theory I shall present this is fundamentally wrong. Linguistic competence and understanding are not just knowledge. To have competence in connection with a term it is not sufficient, in general to have the full battery of usual linguistic knowledge and skills: one must, in addition, be in the right sort of relationship to certain distinguished situations (normally, though not necessarily, situations in which the referent of the term is present). It is for this reason that this sort of theory is called a 'causal theory' of meaning." Hilary Putnam, "Explanation and Reference," in Mind Language and Reality: Philosophical Papers, vol. II (New York: Cambridge University Press, 1979), pp. 198-99.

[57]Naming and Necessity, p. 48.

[58]Ibid., pp. 61-62.

[59]Kuhn, op. cit., p. 54. Emphasis added.

[60]Priestley, op. cit., pp. 116-17.

[61]Philip Kitcher, "Theories, Theorists, and Theoretical Change," Philosophical Review, vol. 87, no. 4, reprinted in David Boger, Patrick Grim, and John Sanders, eds., The Philosopher's Annual (vol. II) (Totowa: Rowman and Littlefield, 1979), pp. 128-54.

[62]Ibid., p. 128.

[63]Ibid., pp. 138-39.

[64]Ibid., p. 139.

[65]Ibid., p. 141.

[66] Ibid., p. 143. For more details on the Historical Explanation Theory, see, Keith S. Donnellan, "Speaking of Nothing," Philosophical Review, vol. 83, Jan. 1974. Reprinted in Schwartz, op. cit., esp. pp. 228-34.

[67] Keith S. Donnellan, "Reference and Definite Descriptions," Philosophical Review, vol. 65, July, 1966. Also reprinted in Schwartz, p. 46.

[68] Saul Kripke, "Speaker's Reference and Semantic Reference," in Peter A. French, et al., eds., Contemporary Perspectives in the Philosophy of Language, op. cit., p. 15.

[69] Lawrence Poncinie, "Essentialism With Meaning Change," (Unpublished, University of Oklahoma, August, 1982), p. 4.

[70] Naming and Necessity, p. 116.

[71] He says that one can fix a reference via a description and that the corresponding statement is apriori, but not necessary (e.g., 'Neptune', Naming and Necessity, fn. 33, p. 79, and fn. 42, p. 96). He deals with 'light' and 'heat' by claiming that heat is that which produces a certain sensation in us (ibid., p. 129), or light is "whatever affects our eyes in certain ways" (ibid., p. 130). This is, however, still not a treatment of 'photons' or 'molecules moving', i.e., how is the identity established? A few pages later, he claims that 'electricity' is 'originally identified' as the cause of certain concrete experimental effects (ibid., pp. 136-37). That is more germane to our present problem, but is also about all he says about it. As we shall see, Putnam attempts to elaborate on such issues.

[72] "Explanation and Reference," op. cit., p. 197.

[73]Ibid., p. 200.

[74]Ibid., p. 201.

[75]Ibid., p. 202. As we saw, Kripke also hints at such a view.

[76]Naming and Necessity, fn. 33, p. 79.

[77]Ibid.

[78]George Abell, Exploration of the Universe (New York: Holt, Rinehart, and Winston, 1969, 2nd ed.), p. 322.

[79]Michael E. Levin, "On Theory-Change and Meaning-Change," Philosophy of Science, vol. 46, no. 3, Sept., 1979, pp. 421ff.

[80]Ibid., p. 422.

[81]Berent Enc, "Reference of Theoretical Terms," Nous, no. 10, 1976, pp. 261-81.

[82]Ibid., pp. 269-70.

[83]Ibid., p. 271.

[84]Ibid., p. 278.

[85]Hartry Field, "Theory Change and the Indeterminacy of Reference," Journal of Philosophy, vol. 70, no. 14, August 16, 1973.

[86]Ibid., pp. 465-66.

[87]Ibid., p. 466.

[88]See, for example, M. Russell Wehr and James A. Richards, Jr., Physics of the Atom (Reading: Addison Wesley, 1967), pp. 157-60, esp. p. 159.

[89]Field, op. cit., p. 467.

[90]Ibid., pp. 467-69.

[91]Ibid., p. 470.

[92]Ibid., pp. 479-81.

[93]Peter Smith, Realism and the Progress of Science (Cambridge: Cambridge University Press, 1981), pp. 78-80, esp. p. 79.

[94]Edwin Berk, "Reference and Scientific Realism," Southwestern Journal of Philosophy, vol. 10, no. 2, Summer, 1979.

[95]Peter Smith, op. cit., especially chapters 4 and 5, pp. 70-129.

CHAPTER III

REALISM AND SCIENTIFIC 'VIRTUES'

If the most we can expect from theories of denotation is that they be _compatible_ with our intuitions concerning scientific progress and the truth or falsity of specific theoretical claims, arguments for and against scientific realism must retreat to the level of evidential support for theories and the proper epistemic attitude toward such support. There is a long history of such debates within both philosophy and history of science. Conceptually, a realist must try to establish that some theories at various points in their development have enough evidential support to warrant rational belief in the unobservable entities, properties, and relations that these theories postulate. Historically, she must try to provide means to separate theories that had this support but are, nevertheless, now held to be false from current theories that she wants to establish as warranting rational belief _because_ they have this support. The classical debates between realists and anti-realists have taken place on both fronts, and it seems to me to be requisite for the realist to handle plausibly each kind of anti-realist objection. Still, it seems methodologically preferable to separate, initially, conceptual and historical concerns. Consequently, the present chapter will be divided into two sections. The first will explain what I mean by 'virtues' of scientific theories and isolate those virtues that I think are relevant for supporting realist claims. The second will defend these virtues against what I consider to be the most important conceptual anti-realist objections. The historical objections to scientific realism will be discussed in the following chapter, together with my responses and two detailed historical examples. The end result will provide, I hope, an adequate and plausible defense of scientific realism as delineated in Chapter One.

Virtues of Scientific Theories

The 'virtues' of scientific theories that I have mentioned correspond to some of the criteria that have been argued historically to play important roles in rational theory evaluation, as well as theory choice when competing theories are involved. The term 'virtue' I have borrowed from Quine and Ullian,[1] as well as the names of certain specific virtues that I will soon list and discuss. They have also been called 'factors' or 'guidelines' of theory choice, and often include such concepts as 'simplicity', 'testability', 'falsifiability', 'explanatory power', 'notational elegance', 'scope', 'unification', and the like. In fact, the variations in terminology seem almost endless in both philosophical and historical treatments of science, and this terminological fecundity is further muddled by the equally variable interpretations of what these terms mean, and what methodological importance (if any) they have. The main purpose of this section is to limit this proliferation of terminology, and to explain the meaning of the terms which are employed as a prelude to my subsequent philosophical and historical discussion.

As both an example of these conceptual confusions involving theoretical virtues, and as a suitable introduction to my treatment of them, the virtue of simplicity provides an excellent place to begin. It is obvious historically that scientists have in fact utilized various notions of simplicity in their assessment of the adequacy of scientific theories.[2] It is also obvious, after reflecting on their various interpretations of this virtue, as well as upon the voluminous philosophical work devoted to 'clarifying' this notion, that 'simplicity' is hardly a univocal concept.[3] Some interpretations have emphasized a notational treatment of simplicity, in terms of 'parametric families', or 'dimensions' of a theory.[4] Others stress one or another version of 'Ockham's razor', advocating a parsimony of logical or non-logical primitives in a theory,[5] or the number of assumptions a theory needs to successfully deal with the relevant empirical data.[6] Still others stress the 'overall simplicity' of a scientific theory, how it unifies diverse data within a given field of science, or even by combining previously theoretically disparate fields (e.g., Newton's 'unification' of terrestrial and celestial dynamics).[7] Sometimes, as one might expect, these and other interpretations of simplicity may conflict with one another.[8] Furthermore, all of the above represent 'synchronic' interpretations of simplicity, applicable to a theory isolated from its historical development. George Schlesinger argued twenty years ago that such notions cannot do justice to the actual use of the criterion of simplicity in historical cases of theory choice, and advocated instead a concept of 'dynamic simplicity', which evaluates theories in terms of their historical development.[9]

As a further complication, Schlesinger points out that interpretations of simplicity introduce many different factors into the problem of rational theory choice that need to be weighted when evaluating actual historical cases.

> Copernicus had to assign three types of movements to the earth: (i) lateral--an annual orbital movement encircling the sun; (ii) rotational--a diurnal revolution about its own axis; (iii) axial--an annual conical motion of its axis about the centre of the earth.
>
> Ptolemy was able to account for all celestial phenomena without postulating more than one type of movement for any body--lateral movement. Thus when it comes to the types of movements stipulated, the Ptolemaic system may be regarded as simpler.
>
> On the other hand, Ptolemy had to postulate the alignment of the centres of the inferior planets to the sun, otherwise he could not explain their 'limited elongation'. In the Copernican system this phenomenon follows automatically from the fact that the inferior planets move inside the orbit of the earth and no special restriction on the movement of the inferior planets was needed. In this respect then the Copernican system is the simpler.
>
> Thus we are faced with the problem of weighting different kinds of extra factors against one another. One theory introduces an extra sort of movement, the other a peculiar kind of restriction on movement, and we have to decide which one is to be regarded as more complex as a result.[10]

Further complications, involving the status of any or all of the above interpretations of simplicity (whether it is a semantic, pragmatic, or syntactic virtue, whether it offers any evidence for a theory's truth, etc.) will be discussed in the next section. Already, however, the need for some agreement concerning what the virtue of simplicity amounts to, should be apparent. Such a move toward clarification is, I think, necessarily prior to attempts to formalize or quantify this concept. Some of the difficulties faced by earlier attempts to formally handle this notion involve disagreements over what notion is being formalized.[11] In any event, the present treatment of scientific virtues will attempt only intuitive and historical clarifications of those that I feel are important for realist/anti-realist debates.

A complication concerning the status of scientific virtues that must be considered in this section, however, is that they do not all have the same evidential status in theory evaluation. Gerd Buchdahl, for example, has long held that theory evaluation takes place at many levels. In one of his early papers he claims that assertions, doubts, and denials concerning the existence of theoretical entities can be expressed on at least two levels, the 'phenomenological' (ϕ-level), and the 'epistemological' (e-level).[12] (ϕ-level) assertions and denials occur in a language "which operates within a conventional framework where expressions such as fiction, physical hypothesis, base assumption, evidence for the existence of, and so on, have a relatively fixed grammar."[13] Within this conventional framework there can be both direct and indirect evidence for the existence of theoretical entities, both sorts established by the ongoing community of practicing scientists within a field, and "strictly scientific considerations" determine the language of theory choice. (e-level) assertions and denials, on the other hand, are "extremely general," and questions concerning 'existence' and 'direct' and 'indirect' evidence involve global criteria, instead of the conventional nature of such questions on the (ϕ-level). It is characteristic of, and legitimate within, the (ϕ-level), for example, to make such claims as "whilst originally we had very little evidence for the atomicity of matter, later discoveries greatly strengthened our belief in the physical reality of atoms."[14] The (e-level), on the other hand, often does not admit of such degrees of "the strength of conclusions based on scientific evidence," but rather questions the meaningfulness of even discussing the existence of theoretical entities. The (e-level) is characterized by assertions concerning the impossibility of proving the existence of theoretical entities, or a retreat to only admitting entities for which we have direct evidence.[15] The (e-level), in other words, is meta-scientific, philosophical, and not directly challengable by any scientific work or developments. Buchdahl's charge in this paper is that these levels are often muddled in debates concerning the existence of theoretical entities, and that they need to be carefully separated.[16]

This 'levels' distinction is advanced and much elaborated by Buchdahl in a more recent paper,[17] in which he argues explicitly that typical scientific methodological criteria such as 'degrees of confirmation' are not sufficient for understanding historical cases of theory choice, and must be supplemented by "general notions and principles of a more philosophical kind."[18] Specifically, he lists three components involved in theory choice: the 'architechtonic', the 'explicative', and the 'constitutive' components. The architechtonic component includes criteria such as esthetic considerations, metaphysical preferences, and privileged notions of a general sort such as 'an effect cannot be greater than its cause', or whether or not 'action at a distance' is appropriate in scientific explanations.[19] The explicative component is concerned with the intelligibility of a theoretical concept, and often involves concept formation or important shifts in the application of an already occurring concept. Einstein's treatment of the 'proper' meaning of 'simultaneity', or Maxwell's treatment of the proper constituents in 'dynamical explanations' are examples of the use of the explicative component.[20] Finally, the constitutive component represents more familiar criteria for theory choice, such as degree of confirmation, and the integration of scientific laws into a system.[21] Since only the latter is typically included in discussions of scientific virtues, the addition of the former two once again complicates the status of these virtues.

Buchdahl's example of a historical problem which included all three components of theory choice is Newton's postulation of gravity in his dynamical system. The familiar constitutive components in defense of this hypothesis were included in the Principia itself. Free fall, the orbit of the moon, and the behavior of the tides are among the empirical phenomena 'explained' by the use of this hypothesis and Newton's other laws of motion. The ability of Newton's system to unify and account for such diverse data is commonly held as providing evidence for its acceptance.[22] Most of the original objections to Newton's system, however, were not leveled at the constitutive component, but at the intelligibility and methodological acceptability of gravity interpreted as action at a distance. Since no mechanical 'pushes and pulls' were postulated as accounting for gravitational force in the Principia, the proponents of the mechanical philosophy predominant at the time accused Newton of introducing 'occult' forces into natural philosophy.[23] In other words, these criticisms were leveled primarily from the point of view of architechtonic and/or explicative components. In fact, even the eventual acceptance of gravitational force (and attractive and repulsive forces in general) within natural philosophy was not exclusively because of its success at the constitutive level.[24]

If I am correct in my interpretation of the development of Buchdahl's views, his earlier (e-level) was later divided by him into the architechtonic and explicative components. The 'intelligibility' of attractive and repulsive forces, for example, depends on both epistemological-metaphysical considerations (architechtonic component) and on redefining dynamical terms to make them compatible with attractive and repulsive forces (explicative component).[25] If his arguments that these other considerations have greatly effected historical theory evaluation (and choice) are correct, the discussion of virtues must be correspondingly expanded if they are to act as criteria of theory evaluation (and choice). Such a separation of levels (or components) will be valuable, for example, in assessing 'external' factors involved in theory choice.[26] Such an approach is typically employed by 'Marxist' historians, usually claiming priority of socio-economic factors in the development of scientific theories,[27] though there are also many non-Marxist 'external' interpretations of at least specific historical cases.[28] I concur with many historians that the external/internal distinction leaves much to be desired, especially if external factors are relegated to the area of scientific discovery as opposed to justification. But, however one interprets them, such aspects of theory evaluation should certainly be distinguished from constitutive (ϕ-level) factors. To so <u>distinguish</u> them is not to automatically import corresponding notions of relative rationality.

More importantly, distinctions of levels or components of theory evaluation in the spirit of Buchdahl's are important for separating virtues as relevant or irrelevant for rational <u>belief</u> in a theory (or of the existence of the entities postulated by the theory). Kuhn, for example, argues that Copernicus' claims concerning the greater economy or accuracy of his system as compared with Ptolemy's are largely illusory, much in the spirit of Schlesinger's quotation cited in the text above. Kuhn argues instead that "the real appeal of sun centered astronomy was aesthetic rather than pragmatic."[29] Certainly aesthetic components, as well as various metaphysical-epistemological factors like those discussed earlier, play a role in historical cases of theory choice, as do the 'external' factors discussed in the last paragraph. It is another matter, however, to claim that these factors all play equal roles in theory evaluation, or that the roles played by the architechtonic and explicative components (or whatever terminology one chooses to mark such factors) are as important in theory evaluation as the constitutive component.

In the case of Copernicus and Ptolemy, for example, Clark Glymour has argued persuasively that Copernicus' theory was in fact at the time it was introduced more testable than Ptolemy's.[30] Similarly, Mary Jo Nye, while acknowledging the role played by Buchdahl's architechtonic and explicative components in the 19th century atomic debates,[31] maintains that the most important role was played at the constitutive level.[32] Finally, while Kepler's motivations for formulating his laws of motion included both a belief in mechanical principles and neo-Platonic metaphysics,[33] he certainly used constitutive factors such as 'fitting the data' to check his metaphysical explanations,[34] and his metaphysical speculations were not what led Newton, for example, to utilize his laws. In short, distinctions like Buchdahl's enable us to separate virtues and their respective influences on theory evaluation, and we need not expect them to have equal weight, or equal methodological significance.

With this in mind, I will now list the virtues I think important for evaluating theories in light of realist/anti-realist debates, though my realist arguments for them will not be presented until the next section. It should be clear from the above that I am not claiming that the virtues listed are the only factors involved in theory evaluation, but merely that the ones listed can play a special role in the realist/anti-realist debates. Furthermore, the introductory nature of the present work does not preclude possible further elaboration of the virtues listed, nor adding additional virtues to the list. In other words, the following list is not meant to be exclusive or exhaustive.

Given the plethora of interpretations of 'simplicity' partially listed above, I will not include this virtue, as such, in my list. Instead, I will introduce two notions which I think correspond to different interpretations of this virtue. They will not, of course, cover all of the former notions of simplicity found in particular cases of theory evaluation, nor all the notions isolated in philosophical discussions of the methodological import of simplicity. 'Notational' simplicity and 'curve-fitting', for example, while no doubt playing a role in theory evaluation, do not seem to me to represent constitutive considerations, nor are they obviously relevant to the realist/anti-realist debates.[35] There seem to be no good reasons, after all, to think that theories expressed by 'simpler' mathematical expressions are (thereby) more likely to be true. Furthermore, if the above arguments concerning the different methodological import of various virtues have any weight, their omission does not stack the deck in favor of realism any more than does excluding socio-economic factors from epistemological considerations. Again, while many factors play a role in theory evaluation, they do not thereby have equal methodological weight.

The first virtue that I will discuss corresponds roughly to 'Ockham's razor' interpretations of simplicity, that is, parsimony concerning the number and types of theoretical assumptions needed by a theory to cover the relevant phenomena. I will call this virtue 'modesty', again borrowing from Quine and Ullian.[36] As with most of the virtues with which I am concerned, the role of modesty in theory evaluation is most often comparative (it will generally be evaluated in cases of theory choice, when the theory in question is competing with at least one other theory). While this does not necessarily preclude the possibility of evaluating its role within a single theory, it will be seen later that this is almost never a historical factor within actual scientific debates. Furthermore, the modesty of a given theory is certainly relative (and contextual) in the sense that T_1 is more modest than T_2, or with respect to the present state of the field in question, or in terms of such-and-such weighting of the theoretical components. Again, given the weighting problem discussed above in evaluating Copernicus' and Ptolemy's astronomical systems, I see no hope for a method of evaluating modesty that envisions simply counting relevant factors. Some factors have and (as I will argue later) should have more importance than others.

Examples of modesty in theory choice include such components as whether or not a theory utilizes 'extra wheels' or 'useless appendages' to calculate experimental results.[37] Ptolemy, for example, needed extra 'special' assumptions to account for both the 'limited elongation' of the inferior planets mentioned above, and the fact that superior planets retrograde only during their synodic period. The latter is not only explained by Copernicus' putting the earth in orbit around the sun and interpreting both retrograde motion and synodic periods in terms of the colinearity among the positions of the earth, the planet, and the sun; it is also a commitment of his theory once the earth is placed in motion. In this sense, the one assumption that the earth is in orbital motion and the corresponding reinterpretations of 'synodic period', 'siderial period', and 'apparent retrograde motion' account for a regularity that requires extra assumptions for Ptolemy's system that were clearly created just to account for this particular regularity.

Similarly, Einstein criticized the mechanical assumption of an 'ether' as a medium for transmitting light because of the special assumptions needed to make this construct compatible with experimental data.

> The discussion of all the various attempts to understand the mechanical nature of the ether as a medium for transmitting light would make a long story. A mechanical construction means, as we know, that the substance is built up of particles with forces acting along lines connecting them and depending only on the distance. In order to construct the ether as a jelly-like mechanical substance physicists had to make some highly artificial and unnatural assumptions. . . . The artificial character of all these assumptions, the necessity for introducing so many of them all quite independent of each other, was enough to shatter the belief in the mechanical point of view.[38]

In cases such as this and of Copernicus and Ptolemy, theoretical constructs that play no role, except the handling of otherwise troublesome data, are spurned, especially if another theory can handle them without 'extra wheels'. In such cases the latter theory can be claimed to be more modest than the former, relative to the contemporary state of both theories. In other words, accusations concerning a theory's use of *ad hoc* assumptions often depend upon another theory's ability to handle the same data without 'useless appendages', i.e., its being more modest.

Of course, what constitutes a 'useless appendage' is relative to the state of the science in question at a given time. As we saw earlier, modesty understood as simply counting the number of assumptions in a theory is not a historically propitious approach. *Which* assumptions and entities to count, for example, is not usually evident until *after* a historical debate has progressed for some time. Worse, as will be shown shortly, it is usually only in conjunction with other virtues that this question can be answered with any degree of methodological justification. A modest theory at one time may subsequently fail to account for the data as experiments multiply. A seemingly *ad hoc* adjustment at one time may later become a justified addendum to a theory in light of further research. Consequently, modesty and all of the other virtues can only be claimed for a theory in light of contemporary evidence, and relative to other theories that attempt to account for the same data. It and the other virtues are also *defeasible* in light of further historical developments.

With this in mind, the virtue of modesty can be reformulated to favor the theory which does not *unnecessarily* multiply assumptions or entities in order to account for a given range of data. 'Unnecessarily', like *ceteris paribus* clauses, does not admit of exact formulation, and retains a certain 'fuzziness', much to the chagrin of many historians and philosophers. Still, like *ceteris paribus* clauses, being fuzzy does not necessarily render it unmanageable. Rather, being fuzzy may only indicate that there are many debated cases where the available evidence allows for rational disagreement. This much is true of any historical guideline, however uncontroversial the guidelines themselves may be. Such guidelines have accomplished their task if the relatively clear cases come out in accord with out intuitions, and some of the controversial cases can be changed into clearer ones. I think the troublesome 'unnecessarily' addendum to the above formulation of modesty can be shown to have at least this much clarity.

The revised formulation of modesty can, for example, provide a reasonable account of the much debated 'inductivist' strain in Newton and many of his later admirers. In the famous General Scholium added to the second edition of the Principia in 1713, Newton seems to attack all theory construction that makes claims beyond the observable phenomena and laws describing these phenomena.

> But hitherto I have not been able to discover the cause of those properties of gravity from phenomena, and I frame no hypotheses; for whatever is not deduced from the phenomena is to be called a hypothesis, and hypotheses, whether metaphysical or physical, whether of occult qualities or mechanical, have no place in experimental philosophy.[39]

That he is not arguing against all hypotheses has recently become widely held by historians. Not only in the Principia itself (for example, in the paragraph immediately following the above quotation), but even more clearly in his letters and in his Opticks, he frames many hypotheses, both physical and metaphysical.[40]

While the passage in the General Scholium has been interpreted as a psychological-political response to criticisms concerning his earlier prism experiments and the first edition of the Principia,[41] as well as in numerous other ways, I think that there is also a methodological point underlying it. In the Opticks, for example, immediately before he beings his famous 'Queries', the following passage can be found.

> When I made the foregoing observations, I design'd to repeat most of them with some care and exactness, and to make some new ones for determining the manner how the Rays of Light are bent in their passage by bodies, for making the Fringes of Colours with the dark lines between them. But I was then interrupted, and cannot now think of taking these things into further consideration. And since I have not furnish'd this part of my Design, I shall conclude with proposing only some Queries. . .[42]

A cursory reading of these Queries indicates that the language used does not express disdain for, or even disbelief in, the many hypotheses contained therein. Nor are many of these hypotheses more 'theoretical' than others which he makes, even in the Principia. Rather, I think, they have not yet been experimentally justified to the point that Newton considers them proved, even though he clearly implies that he believes they could be. In other words, if I am correct, he is advocating something like the reformulated virtue of modesty presented above.

If we look again at his first 'Rule of Reasoning in Philosophy', it appears to be very close to reformulated modesty ("We are to admit no more causes of natural things than such as are both true and sufficient to explain their appearances."[43]), especially if coupled with his fourth Rule, which also seems to be an argument against hypotheses.

> In experimental philosophy we are to look upon properties inferred by general induction as accurately or very nerely true, not withstanding any contrary hypotheses that may be imagined till such time as other phenomena occur, by which they may either be made more accurate, or liable to exceptions.
>
> This rule we must follow, that the argument of induction may not be evaded by hypotheses.[44]

If 'induction' is read as it is by inductivist methodologists, then the above is an argument against all hypotheses. It seems clear from

Newton's actual procedure, however, that this is not what he means. Rather, one should not, according to Newton, frame hypotheses that do not correspond to experimental data, and are not reviseable in light of further data. In other words, do not unnecessarily multiply hypotheses or theoretical assumptions and entities.

This interpretation succeeds quite well, I think, in accounting for claims made by later scientists who share Newton's inductivist-sounding approach. Joseph Priestley, for example, in discussing the history of electrical theories proliferated in the 18th century, while seemingly denigrating hypotheses in some passages, certainly does not mean to spurn them all.

> Any experiment in which there is any design, is made to ascertain some hypothesis. For a hypothesis is nothing more than a preconceived idea of an event, as supposed to arise from certain circumstances, which must have been imagined to have produced the same, or a similar effect, upon other occasions.[45]

If this hypothesis is 'absolutely verified' for Priestley, it is no longer called a hypothesis, but a 'fact'.[46] The main danger in formulating hypotheses is to 'jump too soon', or become too attached to a 'favourite hypothesis' so as to "not be convinced of its falsity by the plainest evidence of fact."[47] Furthermore, while there are no limits to the imagination having full play in the formation of hypotheses, if a theorist can "frame this hypothesis so as really to suit all the facts, it has all the evidence of truth that the nature of things can admit."[48] For Priestley, it is also evident that these hypotheses must be defeasible in light of new facts (i.e., the earliest electrical theories could be quite general, when the only electrical 'facts' to account for were attraction and repulsion).[49]

Similarly, Benjamin Franklin, though he'd come to be troubled by certain problems his 'atmosphere' hypothesis was encountering (e.g., the repulsion of negatively charged bodies), was not persuaded that cases of electrical repulsion could be reduced to cases of attraction. He argues against Kinnersly that there are cases of apparent repulsion that are not easily handled by Kinnersly's proposed subsumption under attraction, and that there are other cases of repulsion in nature that certainly cannot be thus explained away. Finally, he argues that the apparent modesty acquired by reducing the causes of electrical phenomena to one cannot be bought at the expense of the recalcitrant data.

> We should not, indeed, multiply causes in philosophy without necessity; and the greater simplicity of your hypothesis would recommend it to me, if I could see that all appearances might be saved by it. But I find, or think I find, the two causes more convenient than one of them alone.[50]

Again, the argument is not to abandon hypotheses, but not to interpret them as established as long as the jury of experience is still out--i.e., the reformulated expression of the virtue of modesty. In other words, while 'fuzzy', the 'unnecessarily' addendum to this virtue seems to have been effectively utilized by several past scientists.

It cannot be overemphasized that the relativity inherent in all of the virtues must be constantly kept in mind. Their defeasibility requires that judgments concerning a theory's modesty can only be justified given the available evidence, and whether or not successful competitors exist. Again, however, this relativity, and the fuzziness of the 'unnecessarily' addendum need not lead to rampant subjectivism or the Oz-like retreat to different conceptual schemes. In most historical debates the combatants are in basic agreement about <u>which</u> criteria to muster for and against accepting a proposed theory, but differ over how well these criteria are met. As far as modesty is concerned, this can be illustrated by Priestley's and Franklin's criticisms of the Abbé Nollet for failing to let his 'favoured hypotheses' submit to the tribunal of experience.[51] On the other hand, the Abbé defends his hypothesis by claiming that some of its experimental commitments are verified, as opposed to the one-fluid theory (concerning the penetrability of glass by the electric fluid, and the direction of the electric flow among other things), and he challenges his antagonists to submit their theory to the same tribunal of experience that they claim he ignores.[52] Also, while Priestley, in a different context, is among the phlogiston theorists criticized by Lavoisier for maintaining, from his point of view, a hopelessly immodest theory,[53] he (Priestley) insists that his own experiments do not bear out the claims of the French antiphlogistonists (Lavoisier, Berthollet, de Fourcroy, et. al.), but if they _did_, the new theory would be preferable because of its greater modesty.[54] Both of these cases represent disagreements over whether or not a theory unnecessarily multiplies assumptions and/or theoretical entities in order to account for the relevant data. That is, while there certainly can be disagreements over _whether_ the virtue is satisfied in a particular case, it is not typically the case that the disagreement concerns whether the virtue itself is relevant.[55] Furthermore, disagreements over particular cases also tend to diminish as the debates continue, and further research tends to favor one of the combatants, or some new theory.

This last claim coincides with Schlesinger's notion of 'dynamic simplicity' briefly mentioned earlier. Roughly, at an early stage of development one can expect disagreements concerning the relative modesty of competing theories, given the complexity of weighting factors and the general paucity of experimental data available. In electrical theory, for example, the first electrical phenomena recognized were only attractions between a narrow range of objects that had to first be rubbed before they produced this effect. Slowly phenomena concerning repulsion were added, as well as a great expansion of objects that produce electrical effects. Then the Leyden Jar greatly proliferated the number of phenomena to be accounted for, etc. By the mid-18th century, the field of electricity had been expanded to the extent that fewer and fewer theories could even begin to handle the relevant data in a relatively modest manner. As the field expanded there was a corresponding convergence of opinion concerning the modesty of competing theories. Such a convergence of opinion is also found in the history of chemistry, astronomy, the atomic theory, theories of light, etc.[56] While some historians have offered explanations of this convergence of opinion by using non-methodological considerations (e.g., Kuhn), I will argue in the next chapter that constitutive factors played the dominant role. In any event, the _fact_ of convergence, however explained, helps to establish that the 'fuzziness' involved in the above formulation of modesty does not render it hopelessly vague, or inapplicable to actual cases of theory choice.[57] The next chapter will further support this claim by considering two historical examples in some detail.

The second virtue I will call '_generality_'.[58] It is also a subspecies of simplicity, but here the concern is with the amount of previously divergent data that a theory can account for, either within a given field, or from previously diverse fields. In the conclusion of the Origin of Species, for example, Charles Darwin defends his theory of natural selection partially on the grounds that it accounts for "several large classes of facts."[59] Similarly, Einstein, in his paper on "The Electrodynamics of Moving Bodies" utilizes the quantity '$\sqrt{(1-v^2/c^2)}$' in a number of equations, unifying such diverse data as transformation equations, the shortening of physical systems in motion, the energy of light rays, and the longitudinal and transverse mass of an electron.[60] More generally, Einstein tells us:

> New theories are first of all necessary when we encounter new facts which cannot be 'explained' by existing theories. But this motivation for setting up new theories is, so to speak, trivial, imposed from without. There is another, more subtle motive of no less importance. This is the striving toward unification and simplification of the premisses of the theory as a whole.[61]

Maxwell defended the kinetic theory of gases, in part, because it could explain a number of previously disparate laws, including gas laws relating the volume, pressure, and temperature of gases, and Gay-Lussac's law of equivalent volumes.[62] In other places, Maxwell praises Faraday's theory for "taking in at one view, all the phenomena which former inquiries had studied separately,"[63] as well as the work which led to the uniting of theories of light with theories of electromagnetism (i.e., the transverse nature of electromagnetic disturbances, corresponding with Young's and Fresnel's treatment of transverse light waves).[64] Other examples of generality include Newton's unification of celestial and terrestrial dynamics, and the 19th-20th centuries' atomic theory's eventual application to virtually all fields of exact science.

The addition of this virtue further complicates the treatment of modesty presented earlier. Clearly the desires to use as few assumptions as possible and to account for as wide a range of data as possible can tug our methodological consciences in different directions. In early electrical theory, when virtually the only electrical phenomenon known was attraction, "many superficial philosophers thought they had given a very good account of electricity, cohesion, and magnetism, by calling them particular species of attraction peculiar to certain bodies."[65] When smell, sight, and the emission of strong sparks (with accompanying pain) were added to the electrical data, however, "electricians were obliged to make their systems more complex, in proportion as the facts were so."[66] As the complexity of theoretical assumptions proliferated to account for an ever-widening range of data, the counter demand for modesty again asserted itself, relegating all but a few of the theoretical candidates to the limbo of false starts.

The tension between the unifying power of a theory and its relative complexity occurs perennially within the history of science. What makes the previously cited examples so striking, however, is that they each managed to somehow satisfy <u>both</u> virtues. In fact, the scientific excitement engendered by such cases is largely due to the remarkable capacity of such theories to unify more data, while reducing or holding constant the number of theoretical assumptions involved. It is usual scientific practice to neither accept a theory with unnecessary extra wheels, nor accept a theory that does not tie in with other accepted theories, or phenomena. Being labeled '<u>ad hoc</u>' can involve <u>either</u> having special assumptions with no extra support, or being restricted to too narrow a range of phenomena with no obvious tie in to other accepted laws. It was an ad hoc assumption on the part of Ptolemy, for example, to add special requirements to his theory to account for the superior planets being in retrograde motion during their synodic period, both because these assumptions were not needed by Copernicus (modesty), and because they played no role except to account for this phenomena (generality). Of course, at a particular stage of development, both aspects of ad hoc assumptions can be debated. As long as there was no general physical system backing up Copernicus' astronomical theory, for example, it could have been maintained that Ptolemy's was more general (i.e., not only in astronomy, but it also fit in with Aristotelian physics and cosmology). After the new dynamical systems of Galileo and Newton, however, the Copernican-Keplerian astronomy quickly became much more general (terrestrial and celestial phenomena could now be handled by the same set of laws). Similarly, in the 19th century, there was a time when the distinction between atoms and molecules seemed ad hoc, in that it seemed to only rescue failed predictions of the atomic theory.[67] Again, both virtues do involve an aspect of relativity and fuzziness. Still, however, as the debates progressed this fuzziness gave way to convergence of scientific opinion. Again, then, these virtues are not historically unmanageable.

The last virtue that I will claim plays a methodological role in realism/anti-realism debates is related to notions such as 'precision', 'testability', 'refutability', etc. I will call this virtue 'determinateness'.[68] Very generally, one theory is more determinate than another if it has a greater number of theoretical commitments than the latter. The number of theoretical commitments a theory has can, of course, be unpacked in a number of ways, including the number of predictions it generates, the range of phenomena it is designed to cover, the precision of its formulation and its predictions, etc. While 'determinateness' may thereby be taken as exemplifying several virtues (testability, falsifiability, precision, etc.), there is a core notion underlying them all which (I think) justifies incorporating them under one concept. Briefly, they are all concerned with how many and how precise the predictions are that can be derived from a given theory, not only in terms of strict logical deducibility, but also in terms of what else (sometimes rather vaguely--hence the loose term 'commitments') we would expect to be the case if the theory were true. On this reading, Copernicus' theory was more determinate than Ptolemy's at the time it was formulated, because putting the earth in orbit made it a consequence of Copernicus' theory that the inferior planets would achieve limited elongations, that the superior planets would be in retrograde motion during their synodic periods, and that the sum of the sidereal and synodic periods of the superior planets would equal the number of solar years passed.[69] Stated another way, these phenomena individually and collectively test Copernicus' theory, while Ptolemy's is only compatible with the first two, and offers no theoretical account of the third. Not only would Ptolemy's theory not suffer refutation if the first two of these phenomena didn't occur, it would be a more modest theory as a consequence. Copernicus' theory, on the other hand, would be refuted if any of these three phenomena didn't occur.

Similarly, Franklin's theory, accepting the law of conservation of electric charge, is committed to the total quantity of 'electrical fire' ('fluid', 'matter') in the Leyden Jar being the same before and after 'charging'.[70] Before this assumption, electrical theories struggled to be merely <u>compatible</u> with the new electrical phenomena associated with the Leyden Jar. On Franklin's theory there was also a commitment concerning the direction of the movement of the electrical fluid (determinateness), the accounting for a wide variety of electrical data (generality), and all of this with a relative paucity of theoretical assumptions (modesty). Similarly, while both Aristotelian and Newtonian dynamics were committed to heavy objects near the surface of the earth falling if unsupported, Newton's theory is more determinate in that it is committed to a specific velocity of the falling body. In short, many of the claims involving the alleged underdetermination of theories by the available evidence do not make the appropriate distinction between being comaptible with (or even <u>generally</u> committed to, e.g., Aristotle) an experimental result, and being theoretically committed to (or more precisely committed to, e.g., Newton) this result. I would go so far as to argue that hardly any of the usual cases of alleged underdetermination are convincing if all three of these virtues are taken into consideration.

While it is a virtue of a theory to be more determinate than its competitors, there are of course more experimental hazards facing the theory precisely because it has more (or more precise) commitments (for example, phlogiston theory ran into increasing difficulties precisely because it was becoming more determinate). It was also a commitment of Franklin's theory, for example, that 'positive electricity' be taken as basic (i.e., it is a surfeit of this that leads to a body's being negatively charged), and that the explanatory model of the electrical fluid be interpreted mechanistically (i.e., he seemed to favor some sort of direct mechanical interaction as opposed to any notion of action at a distance which would take attraction and repulsion as basic, though this may be a bit oversimplified as we will see in the next chapter). These assumptions led to his positing electrical 'atmospheres' to account for the mutual repulsion of positively charged objects, <u>and</u> to problems with interpreting the facts that negatively charged bodies also repel one another. More commitments, then, make a theory more testable, and more refutable; scientifically superior to less determinate theories, but also more subject to experimental failure. In other words, determinateness cuts across testability and falsifiability as one would expect, and explains, I think, Popper's insistence that the stronger theory contains more potential falsifiers. Having more commitments is a virtue, having these commitments backfire constitutes a problem.

The main disagreements in the history of science concerning determinateness most often concern whether a theory's commitments are satisfied, rather than whether determinateness is a virtue. Hence Kuhn's 'normal science' constitutes attempts on the part of proponents of a theory to try to attach 'friendly amendments' to the theory in order to cash in on the theory's commitments without violating modesty or generality (i.e., without making <u>ad hoc</u> adjustments). At different stages of a theory's development, these friendly amendments may seem more or less justified, and, hence, disagreements arise concerning whether or not these amendments are <u>ad hoc</u>. It seemed arbitrary, we noted, (given the contemporary experimental evidence) to make a distinction between atoms and molecules in the first quarter of the 19th century.[71] Later in the same century the quantities involved became more determinate (measureable in a variety of ways, theoretical commitments supported by organic chemistry, etc.),[72] and the distinction between atoms and molecules was no longer generally regarded as *ad hoc*. Many such cases can be found in the history of science, the basic outline generally being that a new assumption added to a theory to make it square with experimental data must (eventually) itself involve commitments that are borne out by experiment or be labeled *ad hoc*. How long one should wait for this experimental verification depends on whether or not there is an equally 'successful' rival theory, as well as on the generality and modesty of the amended theory. Of course, other considerations such as one's philosophical preferences ((e-level), architechtonic components, etc.) also play a role. But I will later argue that the former considerations are methodological ((ϕ-level), constitutive components) *and* play a more important role.

A fourth methodological virtue should at least be mentioned, and has already been briefly alluded to in the preceding discussion. A theory must, of course, 'fit the facts' to even be a candidate for a scientific theory (no theory, of course, fits *all* of the facts).[73] Generally, the virtue of corroboration represents a theory's 'agreeing with' both experimental facts and the existing body of accepted theories. This virtue, however, cuts across realism/anti-realism debates, and consequently will not play an explicit role in the succeeding section. It does, however, play an important role as a methodological virtue, and hence represents yet another reason for separating virtues into 'levels'.

Having positive results regarding a theory's commitments is what ultimately sways general scientific opinion. Having commitments (being determinate) is itself a scientific virtue, but only in conjunction with corroboration does this virtue support scientific realism. In other words, as a historical tool, before the jury of experiment is in, having more determinateness counts in favor of a scientific theory. After these results are in, only a corroborated theory will be accepted. As might be expected, it is primarily during the waiting period that serious debates occur concerning the relative determinateness-corroboration of a given theory (i.e., while even positive results may leave some scientists unswayed, a determinate theory with positive results will eventually lead to the convergence of scientific opinion). Furthermore, just being corroborated is not enough if the commitments of the theory are not precise enough, or if there is an insufficient number of them (again, Aristotle/Newton on free fall). Consequently, determinateness will be one of the three virtues discussed in the remainder of this dissertation, while corroboration will 'tag along', not because it is unimportant, but because it does not distinguish between realist/anti-realist claims.

So far then, three virtues have been delineated as constituting rational grounds for favoring a given theory over its competitors: modesty, generality, and determinateness. Again, it must be emphasized that this does not deny the importance of other virtues for such a choice, but it will be argued that these three (with corroboration 'tagging along') offer the most conclusive reasons for theory choice, and that they can be used to support scientific realism as described in chapter one. Furthermore, the history of science does not generally support a synchronic interpretation of these virtues, but rather a convergence of evaluation during a successful theory's development, given the evidence and other theories available at various times during this development. The next section will provide the realist interpretation of these virtues, while historical problems will be considered in the fourth chapter.

Virtues of Theories and Scientific Realism

There are at least two sorts of objections that can be leveled against the account of scientific virtues just presented. The first and more general of these is that the history of science does not support the methodological/non-methodological distinction between virtues that I have made. Convergence of scientific opinion, from this point of view, has at least as much to do with the paradigmatic nature of scientific texts (Kuhn), or with non-methodological factors of aesthetic, sociological, philosophical, and psychological types as it does with methodological, constitutive factors. What is 'rational' or 'irrational' about scientific theory choice cannot be determined apart from the prevailing methodological views held by contemporary scientists, and such views can be shown to have changed historically (i.e., they are context dependent). Furthermore, adherence to a new theory is not primarily won by methodological considerations, but by acts of faith,[74] or at least by general commitments to a given research tradition, etc. These general historical objections go far beyond realist/anti-realist considerations.

The most extreme historical objections threaten the very distinction between rational and irrational factors in scientific choice, and more moderate ones at least threaten any methodological, or empiricist attempts to ground such a distinction. Both because they are general, and because they are primarily historical, these types of objections will be addressed in the next chapter.

The second type of anti-realist objection accepts notions of rationality, and the importance of methodological considerations in cases of theory choice, but maintains that no amount of evidence mustered in favor of a theory need lead us to believe the theory, or accept its theoretical statements as true. This is the type of anti-realist approach I am concerned with in this section. First, however, in order to provide a background for this new anti-realist position, some previous attempts to provide a realist interpretation for certain virtues of scientific theories will be presented.

The predominant philosophical approach to scientific methodology in the first fifty years of this century was logical positivism in its various forms. The earliest approach to theoretical entities found in this tradition attempted to eliminate theoretical vocabulary in favor of observational vocabulary. Bridgman's 'operational definitions',[75] or various attempts to deal with the theoretical vocabulary of a theory syntactically in terms of observational primitives[76] are good examples of this approach. Generally, the predominant view was that the only cognitively significant components of scientific theories were their empirical predictions, and the theoretical vocabulary, at best, provided a calculative device for organizing and specifying these predictions.[77] The attitude toward virtues of theories from such an approach was essentially twofold. On the one hand, since there was no cognitive status given to the theoretical vocabulary itself, the only legitimate criterion for accepting or rejecting a theory was its ability to unify and predict experimental data, i.e., only its empirical content mattered. On the other hand, for heuristic purposes, simplicity, elegance, and familiarity of principles might be valued, but they could not, of course, carry epistemic weight.[78] This entire enterprise, as originally formulated, was based on the distinction between theoretical vocabulary and observational vocabulary of a theory. As this distinction was increasingly undermined, so was the attitude of this approach concerning scientific theories and their virtues.[79]

What followed was increasing attention to scientific virtues, especially 'explanatory power', and the topic of scientific explanation began to dominate the literature. If an important, or perhaps the most important, role assigned to scientific theories was their relative ability to explain individual events and/or scientific laws, the discussions of scientific virtues and realism/anti-realism debates became (at least partially) reformulated in terms of accounts of scientific explanation.[80] In brief, the empirical content of a theory, however construed, was increasingly giving way as the focal point of the cognitive significance of the theory to the explanatory power of a theory. As indicated by the various citations listed in note 80 above, for some the predictive and explanatory power of a theory followed from the same (deductive) structure--the so-called 'hypethetico-deductive' or 'deductive-nomological' view of scientific explanation. For others, a deductive structure that generated predictions constituted neither necessary nor sufficient conditions for scientific explanation.[81] Especially for the latter, the explanatory power of theories was evaluated by other criteria than its ability to predict empirical results.

As might be expected, a variety of such criteria have been offered as reasons to accept a theory as the 'best explanation' available of the relevant phenomena. Michael Friedman has argued that 'unification' (a combination of generality and modesty) "increases our understanding of the world by reducing the total number of independent phenomena that we have to accept as ultimate or given."[82] The kinetic theory, for example, accomplishes this by integrating the Boyle-Charles' law, Graham's law, and laws relating to the specific heats of gases into one theoretical account; i.e., the law of mechanics. Furthermore, this theory "allows us to integrate the behavior of gases with other phenomena, such as the motions of the planets and of falling bodies near the earth."[83] Paul Thagard has argued that three virtues provide a sufficient criterion for evaluating the explanatory power of a given theory, 'consilience' (generality), 'simplicity' (modesty), and 'analogy'.[84] Thagard's 'analogy' is something like corroboration of the theory with already established theories.

> We get increased understanding of one set of phenomena if the kind of explanation used--the kind of model--is similar to ones already used. This seems to be the main use of analogy in Huygens and Darwin. The explanatory value of the wave hypothesis is enhanced by the model taken over from the explanation of certain phenomena of sound. Similarly, the explanatory value of the hypothesis of evolution by means of natural selection is enhanced by the familiarity of the process of artificial selection.[85]

Of course, all of these proposed virtues constitute criteria for theory evaluation above and beyond fitting the data (the other sense of corroboration discussed in the last section), which, again, is assumed by all evaluation schemes. They are thus criteria that involve giving theories more of a role than that allowed by positivism, and as such, often constitute a more realist interpretation of scientific theories. In brief, since 'better' theories do more than simply predict phenomena (have empirical content), they must be interpreted as more than simple heuristic calculating devices. Consequently, if a theory postulates micro-entities in its explanation of phenomena, the better the theory is, the more reason there is to believe in the micro-level so postulated.

Another strain of realist interpretations arising from the explanatory power of theories involves the causal role of theoretical explanations. The basic position of such an approach is that correlations between data depict known or unknown common causes, which account for these correlations. The belief in a common cause when divergent phenomena exemplify law-like correlations is, of course, an old realist perspective. When further enhanced by scientific virtues such as generality, it is often claimed that it is rational to believe in theories that postulate such common causes, especially at the micro-level. Wesley Salmon, for example, has long argued that an instrumentalist account of such amazing coincidences is untenable.

> The fundamental fact to which I wish to call attention is that the value of Avogadro's number assertained from the analysis of Brownian motion agrees, within the limits of experimental error, with the value obtained by electrolytic measurement. Without a common causal antecedent, such agreement would constitute a remarkable coincidence. . . . In my opinion, the instrumentalist cannot, with impunity, ignore what must be an amazing correspondence between what happens when one scientist is watching smoke particles dancing in a container of gas while another scientist in a different laboratory is observing the electro-plating of silver. Without an underlying causal mechanism--of the sort involved in the postulation

> of atoms, molecules, and ions--the coincidence would be as miraculous as if the number of grapes harvested in California in any given year were equal, up to the limits of observational error, to the number of coffee beans produced in the same year.[86]

Such an account relies on the explanatory power of a theory in the sense that what the theory explains can be linked together by a common causal mechanism (generality), and that more than one way is developed to measure some of the theoretical quantities involved (a kind of determinateness). The latter condition especially has long been held to be a criterion for the reality of theoretical entities, even by many positivists,[87] though 'real' and 'true' in their sense do not go beyond experimental verification.

In a sense, then, the demise of the positivist's narrow role for scientific theories led to a variety of realist interpretations in the general form of 'theories do more than describe and predict observational phenomena, they also describe non-observational phenomena'. The breakdown of the theoretical/observational distinction, however, also led to non-realist interpretations of science, usually of a historical variety, attacking the very notion of objective theory confirmation and progress (the latter was discussed in chapter two). In short, if a theoretical/observational dichotomy cannot be maintained, the theories themselves at least partially determine what constitutes their experimental verification. That is, if there is no theoretically neutral tribunal (like observation statements) that one can appeal to in support of a theory, one seems forced to appeal to the theory itself (at least in part). If this is so, theory acceptance transcends the cumulation of positive test-instances, and the very notion of 'relevant evidence' becomes problematical. Both probability theories of 'relevant evidence' (especially Bayesian models), and Glymour's 'bootstrap strategy' provide ways of answering these objections (and, as was shown in chapter two, a variety of theories of reference can help to answer the anti-cumulative charges concerning scientific progress). These historical objections, however, will be treated in more detail in the next chapter. At present, arguments against the realist interpretations stemming from the explanatory role of scientific theories must be considered.

While it is commonly maintained that explanatory virtues are important in cases of theory choice, this does not, in itself, support a realist interpretation of scientific theories based on these virtues. R. A. Fumerton, for example, makes a much needed distinction between preferring a given theory on the basis of various explanatory virtues, and believing the theory on the basis of these virtues (specifically, on the basis of Thagard's 'consilience' (generality) and 'simplicity' (modesty)).

> A theory which is more consilient and simple than alternatives (in Thagard's sense and in a number of other senses) is certainly more desirable than its competitors in the sense that it would be nice if it turned out to be true. In general, I assume we are interested in explaining as much as we can and a theory which explains a great deal, both in terms of number and kinds of facts, while avoiding unwieldy *ad hoc* additions, would be a happy theory to have. But this not being the best of all possible worlds (some theologians aside) what would be nice is not always so.[88]

Similar remarks, accounting for our preference for the simplest theory in terms of convenience can be found in Quine, though his overall pragmatism makes his position sound more like a realist position.[89] In short, barring metaphysical-theological claims concerning the simplicity of nature, why should the simpler theory be considered more probable?

Similar doubts have been proposed concerning Salmon's insistence that empirical correlations and more than one technique for measuring a theoretical entity necessitate the existence of a 'common cause' underlying the data.[90]

The conceptual level of anti-realist attacks, then, has to do with the epistemic status of scientific virtues, and no longer with whether theories can be said to have virtues beyond covering the data.[91] Given the need for a comparative, diachronic treatment of scientific virtues outlined in the last section, a realist defense of these virtues cannot be expected to be formally precise. Rather, intuitive and general epistemic considerations need to be addressed at the present time, defending the rationality of interpreting these virtues realistically. Furthermore, as argued in the first chapter, we should not expect a categorical defense of realist interpretations of scientific theories. Given the comparative and developmental nature of scientific theory choice, we may well get different judgments at different times, even regarding the same scientific theory. Realist claims, like knowledge claims, depend on the contemporary evidence and the level of available competing theories. Again, atomic theory did not _always_ support a realist interpretation, nor were epistemological discussions regarding it restricted to methodological (constitutive) components at _all_ stages of its development. Some of the anti-realist attacks at the conceptual (and historical) level hinge on an a-historical, synchronic account of realism, or on mistakes in premature formal treatments of scientific virtues. I will next offer intuitive support for a realist interpretation of the three virtues I listed in the last section, and then address what I take to be the most advanced anti-realist arguments, those found in the works of Bas C. Van Fraassen.

The general realist interpretation for modesty, generality, and determinateness stems from an introductory treatment of theory testing found in an elementary textbook by Ronald N. Giere.[92] In the fifth chapter of his book, Giere lists two conditions for a successful test of a scientific theory.[93] The first condition for a good test of a theory, according to Giere, involves the prediction of experimental results on the basis of commitments of the theory in question, the accuracy and appropriateness of experimental measurements and apparatus, and whatever auxiliary assumptions are presently taken to be background knowledge for the theory. Clearly, this condition involves little more than the theory's fitting the data, and if this were the only condition required, cases of the underdetermination of the theory by the available evidence would abound. We noted earlier, for example, that it is a commitment of both Aristotle's and Newton's theories that heavy, unsupported objects will fall, and both Ptolemy's and Copernicus' theories 'saved the phenomena' in this sense. Furthermore, it is not that difficult to construct theories such that if they were true, the experimental phenomena would result as deductive (or inductive) consequences. Pseudo-science is full of cases of theories being saved by being made <u>compatible</u> with the relevant phenomena. Also, merely deducing experimental results is neither a necessary nor a sufficient condition for scientific explanation, as the recent disenchantment with hypothetico-deductive models of explanation indicates.

The second condition for a good test of a theory, according to

Giere, is both more difficult to establish, and more important epistemically. It must also be established, on this condition, that if the conjunction of the theory, statements about measuring procedures and results, and auxiliary assumptions are _not_ true, then the experimental results predicted would not be expected. Intuitively, if my theory is committed to the occurrence of rain in Oklahoma in April, a positive result would not tend to influence our epistemic attitude toward the theory. It would, after all, probably rain in Oklahoma in April whether my theory was correct or not. A more important example can be generated if we (counterfactually) imagine that Halley had used Newton's theory to predict phenomena concerning free fall, or projectile motion, instead of the reappearance of the comet. These phenomena, unlike the comet, were already to be expected, given the work of Galileo and Newton's own claims in the _Principia_. To be committed to unexpected, or previously unexplored phenomena, intuitively counts more in favor of a theory than phenomena that would be likely or expected regardless of the theory's adequacy. Or, the more precise commitments of Newton's theory concerning free fall make it better tested than Aristotle's, much as predicting one-half inch of rain in Norman on April 3rd between 4-5:00 p.m. would constitute a much better test of my meteorological theory than merely predicting rain in April. As an account of theory testing, Giere's second condition offers few surprises, accounting as it does for the commonplace desiderata of variety of evidence, and the importance of new types of tests instead of repeating existing tests _ad infinitum_. Epistemically, however, I think this condition can help to articulate issues that are much less obvious and accepted.

It is my contention that Giere's second condition for a good test of a theory can be construed as a claim that the more difficult it is for experimental data to be handled (*ceteris paribus*), the more confidence we should have in a theory that manages to handle it. Generally, fitting the data (Giere's first condition) is difficult enough, once the range of relevant data begins to proliferate. To fit this data while remaining more modest, general, and determinate than all existing competitors is correspondingly even less likely. That is, given the difficulty involved in getting a theory to be modest *while* at the same time being general and determinate, not just *any* consistent theory that covers the data will succeed, and the eventual convergence of opinion within the scientific community during the development of such a theory testifies to the confidence that a 'successful' theory (in this sense) engenders. Furthermore, our confidence in a theory *should* grow proportionately to its ability to cover a wide range of data in a relatively modest, general, and determinate manner. Intuitively, even if a theorist's imagination is allowed full play, with no restrictions as to the number of assumptions, different types of data, and theoretical commitments that a theory can muster to account for the experimental data, it will be difficult to provide a consistent theory that satisfies the first condition as the data accumulates. Priestley's earlier account of the narrowing range of acceptable theories accounting for electrical phenomena as the types of electrical phenomena multiplied bears witness to this claim. If restrictions such as avoiding the unnecessary proliferation of assumptions to account for the data, covering as many types of data as possible within and outside of the science in question, and having more (or more precise) theoretical commitments are added, the range of acceptable theories should diminish even further. Furthermore, with these added requirements of relative modesty, generality, and determinateness, it becomes correspondingly less likely that a theory can make good on its commitments unless it is at least partially true (or as we will explain later, 'on the right track'). Put another way, the less prior probability we are intuitively willing to assign to a theory's chances of making good on its claims, the more epistemic probability we should assign to it if it succeeds.[94] Our level of belief in a theory should increase proportionately as its initial likelihood of being successful drops below chance or coincidence (or 'lucky guesses'). If this seems intuitively sound, it must now be argued that these conditions are met (relatively) proportionately as a theory is judged to have the three virtues I outlined above.

The virtue of modesty adds restrictions to a theory concerning whether or not it unnecessarily adds assumptions and/or theoretical entities to account for the relevant data. Intuitively, the less theoretical equipment allowed to 'save the phenomena', the harder it is to save the phenomena, and the less likelihood the theory should initially be given to succeed. If Archimedes only needed a place to stand in order to move the earth, a theoretician only needs an unlimited range of theoretical assumptions in order to explain it. Furthermore (*ceteris paribus*), the fewer independent assumptions a theory has, the more each one of these assumptions is 'committed to'--'useless appendages' are not likely to be measurable in more than one way, or be committed to more than the initial phenomena to be explained. It is very unlikely, for example, that Ptolemy should have succeeded in using in his extra assumptions to explain any more than the data they were introduced to explain. It is also unlikely that adding the property of 'negative weight' or 'levity' to phlogiston would enjoy any other experimental payoff than rescuing phlogiston theory from Lavoisier's troublesome experimental results (if it had, the history of chemistry might be quite different). A more modest theory, therefore, should increase our confidence, both because it is initially less likely for the theory to succeed, and because it is (in a sense) <u>thereby</u> more determinate.

The virtue of generality adds restrictions to a theory concerning the unification of previously disparate data, within the field in question and/or from other fields. Intuitively, again, the more diverse data it attempts to unify, the harder it is for a theory to succeed. Consequently, a theory that manages to unify a (relatively) larger amount of data than its competitors should proportionately increase our confidence in the theory. Again, a theory that unifies more data is also committed to more experimental results. Since Copernicus' theory, for example, managed to deal with the correspondence of the retrograde motion of the superior planets with their synodic period, the lesser elongation of the inferior planets, and the correspondence between the sum of the superior planets' synodic and siderial periods and the total number of solar years, without extra assumptions beyond putting the earth in orbit, each of these represented independent tests of the theory of the earth's motion, <u>and</u> for each other. Consequently, as was the case with the virtue of modesty, a more general theory should increase our confidence both because it is initially less likely to succeed, and because it is (in a sense) more determinate as well.

Finally, the virtue of determinateness adds restrictions to a theory concerning the number and precision of that theory's commitments. While Glymour's reservations about believing a theory simply because some of its theoretical quantities can be measured in more than one way are well taken,[90] surely such occurrences are less likely than chance correlations, especially if the proposed theory is modest and general as well. Furthermore, intuitively, the more commitments a theory has, the more likely some of them are to fail (i.e., the less likely it is that the theory will succeed). Also it seems highly unlikely that a theory which makes very precise (especially quantitative) predictions would be able to succeed by chance or coincidence (e.g., predicting that it will rain one-half inch on April 3rd between 4-5:00 p.m.). Copernicus' theory was <u>committed</u> to many phenomena that Ptolemy's theory was merely compatible with (including the above examples, and the relative sizes of the planetary orbits), and satisfied these commitments without adding new assumptions to account for each one. Furthermore, while both Aristotelian cosmology and Newton's law of universal gravitation are committed to heavy objects falling, Newton's theory is committed to many other phenomena, and to the speed at which an object will fall. While both theories thereby satisfy Giere's first condition, Newton's clearly better satisfies the second condition in the sense that it seems highly unlikely that the commitments of his theory (being both more general and more precise) should be satisfied unless the theory were at least partially right ('on the right track'). Again, the harder it is for a theory to make good on its commitments, the more we should be willing to increase our confidence in it when it succeeds.

All three of the virtues of scientific theories that I've delineated both individually and (especially) conjointly provide reasons for increasing our confidence in a theory proportionately as its success would otherwise seem unlikely. At the intuitive level, I think such reasoning is sound. There are three main difficulties that philosophers and historians might have with such an account: first, that such virtues can be (synchronically) read off of a theory without considering its development and main competitors; second, in trying to formally specify the intuitive account of probability utilized; third, in maintaining that a realist interpretation follows automatically from the above account. The first problem can be handled by my repeated insistence that the assessment of a theory's virtues is almost always a historically relative affair, justified if in fact there turns out to be a great deal of convergence of opinion in historical cases and/or whether one can claim there should have been, given the evidence available at the time in question. The second can be solved by insisting that at this stage of realist/anti-realist debates formal precision should await intuitive consensus. No particular theory of probability or assignment of degrees of the virtues I've listed was assumed in the above account. If these notions were made clear enough to achieve intuitive agreement, I would claim that the above account is 'on the right track', and possible formal treatments can be seen as later desiderata. The third problem can be handled by my continued insistence that realist/anti-realist verdicts should not be categorical, but must be decided for particular cases of theory choice at a particular stage of the relevant theory's development. I will subsequently argue that some scientific theories have met a sufficient level of these virtues at particular times to warrant rational belief. It is not part of my claim that any level of these virtues being met constitutes sufficient reason for rational belief in the relevant theory, or that all cases of theory choice can be so decided by evidence that we now have available. The same can be said, I think, for various knowledge claims, which also have a certain amount of intuitive support without generating necessary and sufficient formal conditions for assessing such claims. That is, we have yet to establish universally accepted criteria for when a knowledge claim is justified. Still, there are numerous intuitively unproblematical cases. This is enough to establish that knowledge claims can 'make sense' and be warranted, even though we cannot specify necessary and sufficient conditions for their being warranted. Also, <u>all</u> knowledge claims are not warranted, since attention must always be focused to the evidence available for particular cases at particular times. With these provisions in mind, if the above arguments seem intuitively plausible, part of my realist defense is complete.

Furthermore, though I have tried to establish some historical support for my position thus far, it should be remembered that this section is concerned with abstract conceptual issues. Given this, it is not yet important to base my realist arguments for the three virtues on whether we <u>actually</u> have these intuitions about any actual historical theories. Rather, they can be argued to be <u>conceptually</u> plausible if we can generate ideal cases where our intuitions concerning these realist arguments may converge. Consider then (for now) an imaginary theory which covers _all_ of the relevant data by utilizing a handful of assumptions, all of which have their own testable consequences and a great number of very precise quantitative commitments that are borne out by experiment. I think that, given such a case, most of us would intuitively think it rational to believe in the theoretical entities this theory postulates, because it does not seem likely that a theory could do such a complete job without being true (or 'on the right track'). If this seems plausible, I think that the above treatment of scientific virtues as restrictions on a theory receives conceptual support--i.e., such considerations would tend to effect our epistemic attitudes _if_ they were realized. Again, conceptual support does not thereby establish that this approach can be adequately applied to actual theories, but that is also not a particularly _conceptual_ issue.

A further defense for a realist interpretation of theories based on the above virtues being met should be mentioned now, though it is not fully articulated or defended until the next chapter. An intuition that grounds many realist interpretations concerns explaining the success of certain scientific theories at various times. A naive but intuitively compelling statement of such an intuition would be that there seems to be no other way to account for the remarkable level of virtues met by some theories, in the face of the extreme unlikelihood of their achieving this level by chance, than to claim that these theories are so successful because they are true (or 'approximately true'). Recent attempts to state such a realist position have come under serious attack by Larry Laudan and others, and these attempts will be dealt with in the next chapter. As a promissory note, I will now merely claim that such intuitions can be supported by my account of scientific virtues, and that some of the problems involved in past attempts to state such intuitions involve, again, categorical claims about realism, insufficient theories of reference to ground such intuitions, premature attempts to formalize this intuition, too much weight being given to the role of non-methodological virtues in historical cases of theory choice, and the use of concepts (such as 'approximate truth') that are both too vague and too strict to do justice to historical cases. In any event, I will not attempt to cash this promissory note at this time, but supporting such widespread realist intuitions will later provide further support for the type of realist position I am defending.

Bas C. Van Fraassen has developed a version of anti-realism that is both broader and more adequate than any previous version. It is set up in such a way as to avoid most of the realist and historical objections to prior positivist and instrumentalist views, and to attack the most common current realist positions. In my opinion, his works taken jointly constitute the state of the art as far as (<u>conceptual</u>) anti-realism is concerned. It will consequently be necessary to consider his position in some detail, and to determine whether my intuitive arguments for scientific realism can answer his objections.

Van Fraassen labels his anti-realism 'constructive empiricism', which is supposed to maintain the classical empiricist notion that the main purpose of scientific theories is to 'save the phenomena', <u>and</u> construe the empirical content of a theory in such a way that it does not fall victim to the objections which destroyed the earlier empiricist accounts.[95] The 'empirical content' of a scientific theory from the older empiricist perspective was, again, based on the theoretical/observational term distinction that has since been largely abandoned. Even after Carnap had abandoned his earlier attempts to describe the cognitive significance of theoretical statements individually (i.e., after he had changed to a more 'holistic' picture of theories), he still maintained that the 'empirical meaningfulness' of theoretical terms must be unpacked in terms of their predictions at the observational level.[96] It was soon argued, as my brief historical acocunt has shown, that this is not the only empirical content of the theoretical terms, but rather that theories themselves at least partially determine what the phenomena are that need to be 'saved'.[97] Consequently, one aspect of Van Fraassen's anti-realism consists in developing a notion of empirical content (or empirical adequacy), that does not depend on the theoretical/observational distinction or on such a niggardly interpretation of the status of scientific theories.

He does this by arguing for a semantic (as opposed to the early syntactic) approach to formalizing scientific theories in terms of models which satisfy the theorems of the theory in question.[98] On such a view, the empirical content of a theory can be delineated as parts of these models that the theory itself specifies as "images of the structures described in measurement reports."[99] This formulation escapes the above objections concerning the theoretical/observational distinction by not being committed to any particular linguistic presentation of the models in question (i.e., it does not require an observational vocabulary as opposed to a theoretical vocabulary), and it is quite compatible with the empirical content of a theory being 'immersed' in the language of the theory. A claim that a theory is 'empirically adequate', in other words, is only committed to the theory having "at least one model that all the actual phenomena fit inside."[100] 'Phenomena', again, are no longer non-theoretical 'observations', but rather predicted outcomes of measurements made by the theory in question. Put another way, an 'elementary statement' in a theory asserts a proposition that a given magnitude that the theory is committed to has a certain value at a certain time.[101] The only distinction necessary for specifying the empirical content of a theory is between 'elementary statements' so described, and other statements within the theory, or between 'phenomenal', interpreted in this way, and 'transphenomenal' commitments of the theory.[102]

> That is, between statements within a model of the theory that specify achieved or expected measurement results, and statements which specify conditions, states, or entities which transcend these measurement results. Measurement results, whether achieved by mechanical or human 'instruments' (i.e. whether due to complicated measuring apparatus or to the mere use of human 'senses') are themselves described by the theory in question, or by other theories (for example, theories of light refraction, theories of perception and psychology, etc.). So that (again) 'phenomenal' need <u>not</u> mean theory independent in the old positivist formulation.[103]

The phenomenal/transphenomenal distinction claimed here is defended and explicated by Van Fraassen in terms of an example.

Newton distinguished between the phenomena that his theory was supposed to save and the (theoretical) reality that he postulated to save them.

> It is indeed a matter of great difficulty to discover, and effectually to distinguish, the true motions of particular bodies from the apparent; because the parts of that immovable space, in which those motions are performed, do by no means come under the observation of our senses. Yet the thing is not altogether desparate; for we have some arguments to guide us, partly from the apparent motions, which are the differences of the true motions; partly from the forces, which are the causes and effects of the true motions.[104]

The 'apparent motions' listed here by Newton are what Van Fraassen calls the 'appearances'[105] (or, again, 'phenomena' or 'elementary statements') saved by Newton's theory. Newton's theory goes on to postulate 'Absolute Space' by which the 'true motions' can be determined, as well as 'gravitational force', etc. However, given Van Fraassen's 'model' approach presented above, the 'appearances' ('apparent motions') are a sub-group within (possibly a number of different) models that can be constructed from Newton's axioms and theorems. Consequently, "when Newton claims empirical adequacy for his theory, he is claiming that his theory has some model such that all actual appearances are identifiable with (isomorphic to) motions (structures which are defined as exact reflections of the 'appearances') in that model."[106] Or, there is some model for Newton's theory that is faithful to the observed ('phenomenal' as described above) motions of the objects it is concerned with (there are 'measurement reports' within the model that 'match' the predictions of the theory). It is admitted by Van Fraassen (as opposed to earlier empiricist accounts of theories) that the axioms and theorems of Newton's theory are themselves a much broader class and that the role of their transphenomenal assumptions is more than just as 'calculating devices' (i.e., Newton's theory is committed to the existence of Absolute Space, etc.). Newton himself admitted that the appearances would be the same if, instead of the center of gravity of the solar system being at rest in Absolute Space, it were instead in any kind of constant absolute motion.[107] Therefore, for Van Fraassen, another theory that postulated that the center of gravity moved uniformly in a straight line, would have models that were also isomorphic with the appearances, but clearly this new theory would have different <u>theoretical</u> commitments from Newton's. In short, theories can be empirically equivalent (i.e., parts of their respective models are isomorphic to the same appearances), without being <u>generally</u> equivalent. This is the genesis of Van Fraassen's anti-realism.

Consider, for example, someone who agrees that Newton's theory is empirically adequate. This person would be committed to believing that Newton's theory has at least one model which is isomorphic to the appearances. For Van Fraassen, this person would not thereby be committed to a belief that the theory as a whole was <u>true</u> (i.e., he or she need not accept the existence of Absolute Space, etc.), but only that it was true that at least one model of the theory could save all the appearances. This in itself involves a <u>risk</u> in the sense that claims for the empirical adequacy of a theory, like claims for its truth, must be defeasible in light of further evidence. Still, <u>accepting</u> a theory as empirically adequate, is not the same as the epistemic attitude of believing that the theory is true. Furthermore, while <u>accepting</u> a theory has an epistemic element (believing that the theory is empirically adequate), it also involves what Van Fraassen calls a 'pragmatic' element (being committed to a research program, etc.).

> On the view I shall develop, the belief involved in accepting a scientific theory is only that it 'saves the phenomena', that is, correctly describes what is observable. But acceptance is not merely belief. We never have the option of accepting an all-encompassing theory, complete in every detail. So to accept one theory rather than another one involves also a commitment to a research programme, to continuing the dialogue with nature in the framework of one conceptual scheme rather than another. Even if two theories are empirically equivalent, and acceptance of a theory involves a belief only that it is empirically adequate, it may still make a great difference which one is accepted. The difference is

> pragmatic, and I shall argue that pragmatic virtues do not give us any reason over and above the evidence of the empirical data, for thinking that a theory is true.[108]

As we shall see later, Van Fraassen goes on to argue that neither does the evidence of the empirical data warrant belief in the truth of a theory, but rather, only that the theory is empirically adequate.

So far, then, Van Fraassen's position can be summarized as a semantic 'model' approach to interpreting scientific theories as opposed to earlier syntactic approaches. The main point in such a procedure as far as anti-realism is concerned is to save a notion of the empirical content and empirical adequacy of a theory from the demise of the theoretical/observational distinction. As such, I have no particular quarrel with this move.[109] Regardless of the ultimate formal success of this admittedly programatic approach, there are good independent reasons for rejecting unqualified holistic approaches that deny any distinction between what a theory postulates, and what a theory is about. Clark Glymour, for example, has offered an approach to objective theory testing that also does not depend on a strict theoretical/observational distinction--what he calls the 'bootstrap strategy'.[110] Furthermore, it is also a realist desiderata that a theory can be objectively tested by instances, requiring some sort of (at least relative) distinction between the phenomena to be saved, and the theories that attempt to save them. Also, if the discussion of further virtues is left aside for a moment, I have no particular reason for disagreeing with Van Fraassen's claim that theories can be empirically equivalent (in his sense) without thereby being generally equivalent. In fact, it is a consequence of the type of realism I am advocating that theories go beyond the empirical data, in that I (and all the realists I am familiar with) share Van Fraassen's views concerning the inadequacy of early empiricism. It is also clear that my position shares Van Fraassen's insistence that the decision to accept (or believe) a theory transcends the belief that the theory is empirically adequate (from my perspective this is tantamount to a claim that the theory satisfy only Giere's first condition for a good test of a theory). In fact, the only difference I have with Van Fraassen so far is that beyond a commitment to a theory's empirical adequacy, criteria for theory choice are pragmatic, since he interprets this as irrelevant to a theory's truth. This, however, is a major difference on which much of his anti-realism seems to depend.

Van Fraassen depicts the differences between realism and anti-realism (correctly) as an epistemic difference.

> Scientific realism is the position that scientific theory construction aims to give us a literally true story of what the world is like, and that acceptance of a scientific theory involves the belief that it is true. Accordingly, anti-realism is a position according to which the aim of science can well be served without giving such a literally true story, and acceptance of a theory may properly involve something less (or other) than belief that it is true.[111]

Concentrating on what is believed by a realist or anti-realist does seem to capture the essence of the philosophical and historical debates concerning the two positions. This has all too often been side-tracked by arguments concerning theories of reference, essentialism, the analytic-synthetic distinction, whether scientific theories are just calculating devices, and so on. While all of these issues are (or may be) important for realist/anti-realist debates, they each emphasize topics that are tangental to the core of these debates. No one, it seems to me, would deny that if 'heat is the motion of molecules', or if the term 'electron' denotes an object, then there are such entities. Nor would many philosophers (presently) deny that theoretical statements are either true or false, i.e., theoretical statements have meanings and alleged references that go beyond the empirical data. The argument is (and almost always has been) concerned rather with whether we are warranted in asserting that 'electron' denotes or that 'heat is the motion of molecules' or that we have reason to believe in the entities that a theory postulates, etc. This much of Van Fraassen's depiction of realism and anti-realism I readily agree with. On the other hand, his formulation of the disagreement is categorical in that it is important to Van Fraassen's anti-realism that a realist be committed to the view that all acceptable (successful) scientific theories be regarded as true (or even 'approximately true', or 'on the right track'). This I have repeatedly denied.

Realist claims should be restricted to particular theories at particular stages of their development, and are defeasible in light of further development and new competitors. Consequently, it is quite compatible with my formulation of scientific realism that the most appropriate epistemic attitude towards some theories at a given stage of their development may well be something like Van Fraassen's acceptance of the theory as empirically adequate. It will be argued in the next chapter that this was precisely the case at certain stages of the development of the atomic theory in the 19th century. It also may be the case with some of our current scientific theories, etc. Analogously, to claim that it makes sense to talk in terms of empirical knowledge does not commit one to claim that he or she 'knows' all statements that appear likely, or that the rationality of making a knowledge claim at a certain time is affected by the possibility that it may later need to be withdrawn (defeasibility). Insofar, then, as Van Fraassen's attacks on realism depend on a categorical interpretation of realism, they are neither conceptually nor historically compelling (or even 'appropriate' if the present enterprise proves successful). Van Fraassen's attacks on realism, however, are not completely dependent on this categorical interpretation, and consequently, some of them will need to be investigated in more detail.

The main stalking-horses for Van Fraassen's anti-realism are, as might be expected, attempts to justify scientific realism in terms of scientific explanation. The main realist positions in this century have, as shown above, been based on such ideas as that the best explanation available is likely to be true or that since science aims primarily at explanation, and we do not have a real explanation unless the explanans are true, science must aim at true theories or, that postulating common causes to account for phenomenal correlations does not make sense without realist commitments, etc. Van Fraassen's attacks on such positions are mainly twofold. First, he argues that explanation does not constitute the goal of scientific theories (being empirically adequate is, for him, their goal). Second, he argues that explanatory power is one among many virtues of scientific theories, and like all other virtues besides empirical adequacy, it is irredeemably contextual (pragmatic), and hence, not related to belief in the truth of a theory. While the second claim is the one I will be most concerned with, it will be worthwhile to begin with a brief examination of Van Fraassen's arguments for the former.

Again, Van Fraassen shares with most realist positions the view that classical empiricism did not assign sufficient status to scientific theories--i.e., they do more than predict and organize empirical data. Consequently, Van Fraassen's anti-realism is not a version of classical instrumentalism.

> To my mind, theoretical entities are fictions. To explain this, let me draw an analogy. Suppose someone writes a short story about a quarrel between a man and a cat. There may be or have been in the world, somewhere, a man and a cat who quarreled, and who are by and large just like the characters in the story. This possibility is quite irrelevant to what the author is doing, and to our evaluation of the story. Of course, it keeps us from saying categorically that all short stories are false; but this too is quite irrelevant. Still I do not suppose that this makes you think that short stories are uninterpreted symbolic devices, or mere instruments; nor that we cannot meaningfully talk either about the story's internal structure or about what it says, or about how good it is.[112]

For Van Fraassen, in other words, we can evaluate theories as theories (not as shorthand for empirical phenomena), and assess their relative merits above and beyond their ability to save the phenomena, and even believe that what the theory 'says' is either true or false, without being committed to the truth of the theories. Hence, merely claiming that theories are more than predictive devices, or that they have meaning over and above their empirical content, does not affect 'constructive empiricism' as Van Fraassen formulates it. The fact that scientific theories often explain phenomena, and that this explanatory power is over and above their predictive power, is quite compatible with constructive empiricism. Realist arguments must therefore argue not only for scientific explanation, but must make special claims about scientific explanation if realism is to be based on a theory's explanatory power.

One common opinion among realists is that the 'empirical part' (however generously construed) of a theory is not sufficient to account for theory choice when the 'phenomena saved' are the same (or nearly the same) for two competing theories. Briefly, a theory's explanatory power leads to greater unification, etc., and its consequent increased testability (or confirmation) is due to theoretical explanation, as opposed to (or in addition to) empirical adequacy. As Glymour puts it:

> When we see a common pattern, what we see is the applicability of a common set of principles to diverse circumstances. In scientific contexts, that application ordinarily results in tests of those principles in diverse ways, with the result that disparate regularities, which have alone no mutual bearing, in common support a theory which entails and explains them all. Thus it happens that a finite body of observational consequences of a theory can provide better evidence for that theory, with respect to the theory itself, than that same body of observational consequences provides for the set of observational consequences of the theory with respect to the set of observational consequences of the theory. . . . Successful tests and satisfying explanations are two different sorts of reasons to believe a theory; both make demands of theories far beyond the demand of empirical adequacy alone, and both are virtues of theories which may override small defects of empirical adequacy.[113]

Hence, although both Copernicus and Ptolemy saved the phenomena, Copernicus' theory had more commitments (internally and in virtue of the phenomena) than did Ptolemy's. If the theories are indeed both empirically adequate, then choosing Copernicus' because of its greater determinateness and generality (within astronomy at least) is both rational, and not based solely on his theory's greater empirical adequacy.

A common recent way to interpret this claim in virtue of a theory's explanatory power, is by insisting that it must be interpreted in terms of 'global properties', or the overall simplification and unification of our world picture.[114] Although particular attempts to formalize or clarify this intuition have run into specific problems,[115] others have been proposed that may or may not be subject to new objections.[116] Van Fraassen, however, has more general arguments against any such attempt. First, it is generally accepted that a theory may explain some phenomena while not being accepted as true (e.g., Newton's theory 'explains' the tides).[117] Consequently, claims about a theory's explanatory power do not lead necessarily to a realist interpretation of that theory. Furthermore, Van Fraassen does not deny the empirical fruitfulness of full-fledged theories as opposed to mere calculating devices or phenomenological laws. What he denies is that besides empirical adequacy, explanatory power gives us any reason to believe in the truth (or even the empirical adequacy) of a theory.[118] The empirical adequacy of a theory also demands that it cover as wide a range of data as possible, so that the kinetic theory, by unifying more data, was more empirically adequate than phenomenological thermodynamics. So even if 'inference to the best explanation' can be formulated adequately, this in itself does not provide a means for supporting claims for a theory's truth, over claims for its empirical adequacy.

Realism, for Van Fraassen, needs an 'extra premise' in such cases, that all universal regularities need an explanation, before "the rule (of inference to the best explanation) will make realists of us all."[119] This extra premise is rejected by Van Fraassen, because requests for explanation come to an end in science (e.g., at different times--accepting action at a distance as 'basic', accepting no change in velocity for a body unless there is some kind of force added to the system, etc.).[120] Furthermore, demands for hidden variables or causes are not always acceptable to science, as indicated by the major current interpretations of quantum mechanics.[121] Still, Van Fraassen does not deny that the principle of inference to the best explanation or to search for common causes play roles in scientific methodology. But constructive empiricism can interpret such demands in terms of advice for formulating empirically adequate theories that find "larger scale correlations among observable events."[122] Such advice, however, will be 'vindicated' or 'not vindicated' by actual attempts to discover such explanations and common causes. Even if vindicated, such constructs need not commit us to belief in the truth of theories thus formulated.

> One way to construct a model for a set of observable correlations is to exhibit hidden variables with which the observable one's are individually correlated. This is a theoretical enterprise, requiring mathematical embedding or existence proof. But if the resulting theory is then claimed to be empirically adequate, there is no claim that all aspects of the model correspond to 'elements of reality'. As a theoretical directive, or as a practical maxim, the principle of the common cause may well be operative in science--but not as a demand for explanation which would produce the metaphysical baggage of hidden parameters that carry no new empirical import.[123]

Insofar as the demand for scientific explanation leads to greater empirical adequacy, then, it is acceptable to constructive empiricism. Insofar as it goes beyond this, it is at best a pragmatic consideration, and at worst metaphysical baggage.

Some of my objections to Van Fraassen's first attack on realism via explanation should be obvious. I do not accept that realism need be committed to the view that all regularities need explanation, or that all scientific explanations are true. There are coincidences in nature that can be proliferated almost at will with the added power of mathematical formulae and statistics.[90] Furthermore, I have no objection to interpreting demands for scientific explanation and scientific virtues as demands for greater unification of empirical phenomena, since my realist arguments partially depend on 'saving the phenomena' in such a way that the theory has enough restrictions placed on it to make it unlikely that it could cover the data so effectively by chance. I do not agree that this part of Van Fraassen's concept of the empirical adequacy of a theory is divorced from notions of the truth of a theory (though, of course, one can assent to the empirical adequacy of a theory without thereby assenting to the truth of that theory, as I've repeatedly claimed). Rather, I think that some levels of empirical adequacy (supplemented by scientific virtues) provide adequate reasons for believing the theory. Furthermore, while generality and determinateness may be open to interpretations in terms of empirical adequacy, I do not believe that modesty can. This issue, however, should await our discussion of Van Fraassen's second attack on scientific realism via scientific explanation.

Van Fraassen also claims that the decision to accept a theory involves more than epistemic considerations. We would both agree, for example, that some aspects of theory choice involve notions such as computational ease, philosophical commitments, socio-economic considerations, etc. (architechtonic, explicative components). We disagree, however, on how to classify some of the other virtues. For Van Fraassen, anything other than empirical adequacy is classified as a pragmatic component, which he interprets as being irrelevant for assessing truth (or empirical adequacy, for that matter). This constitutes the main disagreement between the type of realism I am proposing and Van Fraassen's constructive empiricism. Consequently, his arguments for the pragmatic aspect of scientific explanation and scientific virtues must be considered in some detail.

To establish his claim that the explanatory power of a theory is yet another pragmatic virtue, Van Fraassen first discusses a widespread problem with all attempts to formulate scientific explanation. It was quickly seen that the deductive-nomological (hypothetico-deductive) model for scientific explanation and prediction constituted neither necessary nor sufficient conditions for scientific explanations. Sylvan Bromberger, for example, listed many counterexamples to this view, showing that true deductive relationships can nevertheless fail to explain (i.e., the model is not sufficient).[124] The main feature of these counterexamples consists in showing that there are cases where we can deduce from true empirical laws and accepted theories accurate results, without thereby explaining these results. We can deduce, for example, from a number of laws that the Andromedan Galaxy is 1.5 x 10^6 light years away from the earth. Still, of course, these laws that permit us to make this prediction do not provide an explanation of why this galaxy is to be found at this distance.[125] There are also laws in elementary trigometry that allow us to deduce the height of a building, mountain, or pole, from information concerning the angle which a ray of light forms in a line to the base of the object to a spot x feet away from the object.[126] Clearly, we would not claim that we have thereby explained the height of the building, even though the example also fits the deductive-nomological model. Such cases can be multiplied, and various accounts have been formulated to respond to such counterexamples, called the problem of 'assymetries' in scientific explanation.[127] Whether or not non-deductive-nomological accounts can handle the assymmetry problem, however, Van Fraassen argues that the correct answer depends at least partially on the context-dependence of explanations.[128]

Aristotle discussed the difference between deduction from true premises and scientific explanation, in terms of the difference between 'knowledge of the fact' and 'knowledge of the reasoned fact'.[129] Knowledge of the reasoned fact requires, besides (or perhaps in some cases 'instead of') deduction from true premises, that the proper 'middle term' be used--i.e., that true causal links are involved.[130] True causal explanations, however, depend at least partially on what kind of question is being asked, although Aristotle implies that scientific knowledge involves answering all four of his 'why questions'.[131] Van Fraassen pushes this distinction between deducibility and explanation further, claiming that the demand for explanation, as such, is outside of science and (as above) highly contextual. "Which features are explanatory is decided not by features of the scientific theory, but by concerns brought from outside."[132] Furthermore, while examples like Bromberger's flagpole seem to establish assymmetry (it is not generally an explanation of why the pole has a certain height, that this follows from trigometric laws and stated initial conditions), this assymmetry can be reversed in certain contexts. Van Fraassen tells a story in which the above information does explain the height of a given tower, showing (he thinks) that the relevance of explanations cannot be established in a context-independent manner.[133] Briefly, a spurned lover built his veranda so that at a certain time of day a shadow would cover the exact point at which his former lover stood when . . .--i.e., the length of the shadow does explain why the veranda (or whatever) is x feet high.[133] This is the background of Van Fraassen's claim that explanatory power and other scientific virtues that do not directly enhance the empirical adequacy of a theory, are pragmatic.

This is where Van Fraassen and I part company concerning the varying levels of scientific virtues.

> When a theory is advocated, it is praised for many features other than empirical adequacy and strength: it is said to be mathematically elegant, simple, of great scope, complete in certain respects; also of wonderful use in unifying our account of hitherto disparate phenomena, and most of all, explanatory. Judgements of simplicity and explanatory power are the intuitive and natural vehicle for expressing our epistemic appraisal. What can an empiricist make of these other virtues which go so clearly beyond the ones he considers preeminent?
>
> There are specifically human concerns, a function of our interests and pleasures, which make some theories more valuable or appealing to us than others. Values of this sort, however, provide reasons for using a theory, or contemplating it, whether or not we think it true, and cannot rationally guide our epistemic attitudes and decisions. For example, if it matters more to us to have one sort of question answered rather than another, that is no reason to think that a theory which answers more of this first sort of questions is more likely to be true (not even with the proviso 'everything else being equal'). It is merely a reason to prefer that theory in another respect.[134]

While I concur that some of the virtues Van Fraassen lists here are not directly related to our epistemic attitudes concerning scientific theories, though they may provide other reasons for accepting a theory, I disagree about others. Generally, Van Fraassen labels any virtue other than consistency, empirical adequacy, and empirical strength 'pragmatic'. While this in itself may not be objectionable (depending on one's treatment of 'pragmatic'), Van Fraassen explicitly makes the further claim that the other virtues are not related to decisions concerning a theory's truth.

> The other virtues claimed for a theory are pragmatic virtues. In so far as they go beyond consistency, empirical adequacy, and empirical strength, they do not concern the relation between the theory and the world, but rather the use and usefulness of the theory; they provide reasons to prefer the theory independently of questions of truth.[135]

There are two things wrong with this claim. One involves an ambiguous use of the term 'pragmatic', and the other (following upon the first) is the claim that 'pragmatic' virtues, so described, have no epistemic significance.

Van Fraassen first uses Charles Morris' original introduction of the term 'pragmatics' in symiotics: "By 'pragmatics' is designated the science of the relation of signs to their interpreters."[136] Then he further characterizes pragmatics formally in terms of studies concerning context-dependent expressions.[137] These are not the same considerations, at least if 'relative to their context' and 'relative to their interpreters' have their usual interpretation. 'Relative to their interpreters' has the connotation of being 'subjective', 'psychological', etc.; the type of pragmatics criticized by positivists, and not related to epistemic considerations. Van Fraassen and many others criticize this subjective interpretation of pragmatics. He certainly does not claim that pragmatics in terms of context-dependent assertions cannot be formalized, and often mentions David Kaplan's[138] work in this field as paradigmatic. Context-dependence, however, does not divorce a statement from truth, but only from formal semantics. Here, I think, is the hidden ambiguity in Van Fraassen's use of the term. Traditionally (and vaguely), notions concerning truth are assigned to semantics, and some of my arguments in the last chapter tried to establish that theories of reference often muddle this by confusing 'being about x' with 'denoting x'. In this sense, 'pragmatics' as opposed to 'semantics' is indeed unrelated to truth. However, the formal treatment of context-dependent expressions, also called pragmatics, does not share the view that semantics enjoys autonomy with respect to truth. "I am now sitting at my desk in Dale Hall Tower" is contextual (given the demonstrative 'I'), and so, for Van Fraassen, should be handled by pragmatics. It is also, however, true. Consequently, his arguments against the epistemic status of scientific virtues besides consistency, etc., cannot follow from their contextual nature, or their being labeled pragmatic because of this contextual nature.

I concur with Van Fraassen that 'explanatory power' is yet another virtue of scientific theories and that it is contextual (as are modesty, generality, and determinateness). Nothing I have said commits me to taking scientific explanation as the goal of scientific theories, nor that all scientific explanations are true. I have also developed my account of scientific virtues directly in terms of scientific theories and not in terms of their relation to scientific explanation. I choose to remain open as to whether explanation is a fundamental feature of science, or a result of having good scientific theories. The remaining question is, then, if explanation is temporarily bracketed, has Van Fraassen provided any reasons to reject my formulation of scientific realism?

I think not. Other than some of the earlier passages cited where Van Fraassen claims that belief in the empirical adequacy of a theory accomplishes everything that believing in the theory's truth accomplishes, without 'metaphysical baggage',[139] his usual claim is simply that acceptance of a theory in terms of its empirical adequacy is at least as rational as believing in the theory's truth.

> We have warrant to believe a theory only because, and in so far as, we have warrant to believe that it is empirically adequate.

> In that case it is left open that it is at least as rational as believing that the theory is empirically adequate.[140]

From my diachronic account of realism, this claim is sometimes true. There are cases where the most appropriate epistemic attitude toward a theory at a given time is acceptance of its empirical adequacy, and non-commitance concerning its truth. There are other cases where either view might be rational, and, I will argue, cases where believing the theory is most appropriate. A diachronic realist need not maintain that a (diachronic) anti-realist position is irrational, though I do think that a synchronic position on either side is at least inadequate, both historically and philosophically. Furthermore, I have already provided intuitive arguments which, if successful, should counteract Van Fraassen's claims both that consistency, etc., are the only epistemically relevant virtues, and that the only appropriate epistemic attitude toward a theory is believing it to be empirically adequate. Nothing Van Fraassen has said counteracts these arguments, but it will be worthwhile to consider some possible constructive empiricist responses to my position, both in order to further clarify and support it, and to make way for the following chapter.

First, as I've already hinted, a constructive empiricist can claim that generality and determinateness are related to the empirical adequacy of a theory. As I've presented these virtues, they concern the amount of data that a theory covers, as well as the number and precision of the commitments of that theory. These can be taken as claims for a theory's empirical adequacy, provided that 'phenomena' and 'empirical content' are not taken too narrowly. It is crucial to my claim, for example, that generality and determinateness both lead to a theory being better tested. While it is sometimes unclear how narrowly Van Fraassen interprets empirical adequacy, I find nothing that would commit him to arguing against these claims (I am made somewhat confident regarding this last claim in view of Van Fraassen's claim in a footnote in The Scientific Image, that he doesn't see why Glymour's bootstrap strategy could not be adopted to anti-realist views[141]). In any event, if a constructive empiricist claims that generality and determinateness can be related to empirical adequacy, the realist/anti-realist arguments concerning this would then hinge on whether a sufficient amount of empirical adequacy is not itself a reason for believing in the truth of a theory. Since I have already given most of my realist arguments for generality and determinateness, I would now claim that if such a subsumption under empirical adequacy were successful, this would provide an argument for a realist interpretation of (certain levels of) empirical adequacy. I claim, after all, that it is the way a theory covers the data, and not just its covering the data, that warrants rational belief in the theory. A constructive empiricist would then either need to claim that a theory itself is not to be interpreted over and above its phenomenal consequences (a regress, I fear, back to classical empiricism), or claim that these virtues warrant no more than acceptance of the empirical adequacy of a theory (first, however, my realist arguments for these virtues would have to be refuted).

Since the first alternative does not seem attractive for constructive empiricism, I will investigate the consequences of opting for the latter. It would be somewhat deceptive, in the absence of actual constructive empiricist arguments against my position, to formulate possible constructive empiricist objections and then knock them down. This is not, however, the only way to investigate the second option in more detail. I have, after all, made exactly parallel realist claims for the virtue of modesty as for generality and determinateness. If it can be shown that this virtue cannot be easily subsumed under the concept of empirical adequacy, then the second option will be considerably weakened without setting up straw-men counter arguments. Though all of the virtues I've developed were formulated in terms of how a theory covers the data, modesty in particular is not even directly related to the data alone. Not to unnecessarily proliferate assumptions and theoretical entities in order to account for the phenomena is a guideline for the formulation of the theory itself. According to Van Fraassen, two theories can be empirically equivalent, without being generally equivalent (and I concur). Choosing one of these empirically equivalent theories would, for him, only involve pragmatic criteria (as he formulates this). If my above arguments hold, however, a theory which covers the data while retaining relative modesty is thereby less likely to work than a theory which accomplishes this without the modesty restriction, and consequently we should have more confidence in the former theory. In other words, a more modest theory effects our epistemic judgments, contrary to Van Fraassen's claims. This should suffice (in the absence of constructive empiricist arguments against my position) to weaken the second option open to constructive empiricism.

Another possible response of a constructive empiricist to my position might attempt to use the virtue of modesty against it (hoist me on my own petard). Since there is certainly less commitment involved in accepting a theory's empirical adequacy as opposed to believing the truth of the theory, would modesty not compel us to choose constructive empiricism over realism, since it can (allegedly) cover the same data without these extra assumptions? Briefly, if at least modesty involves epistemic attitudes toward theories that are not confined to empirical adequacy, there is at least this aspect of our epistemic behavior that constructive empiricism cannot account for. This, of course, depends on my arguments having at least intuitive appeal, and, in the absence of counterarguments, I think it is appropriate to provisionally accept this. Second, I'm not sure that epistemology is _that_ 'rationalized'. Briefly, I think further arguments are needed to treat epistemic concerns on a par with scientific concerns--it is not clear that such a move would constitute a proper use of the virtues I've developed. Finally, and most importantly, even if the above considerations fail to hold, there is another problem with using modesty to argue against my version of scientific realism. For such a move to work, it would need to be made quite generally--not only for theoretical entities, but for _all_ inferred entities. Van Fraassen, at least, does not seem willing to accept such a restriction for all inferences. He considers 'inference to the best explanation', for example, as _telling_ in 'ordinary' cases, but argues that establishing this rule as a methodological maxim at the ordinary level does not justify it at the level of theoretical entities.

> Surely there are many telling ordinary cases: I hear something in the wall, the patter of little feet at midnight, my cheese disappears--and I infer that a mouse has come to live with me. Not merely that these apparent signs of mousely presence will continue, not merely that all the observable phenomena will be as if there is a mouse, but that there really is a mouse.[142]

It would be more modest in the above sense to bracket commitment to 'real mouseness' in this case, but it would also be a silly use of this virtue. This, if anything, would violate the 'unnecessarily' addendum added to modesty. Hence, more argumentation would be needed to scuttle my position by using my own virtue against me, to the tune of clearly separating the above inference from theoretical inference.

In conclusion, another analogy between knowledge claims and my characterization of realist claims may help to summarize this chapter. Sometimes the most rational account of our experience is an inference to beliefs that go beyond experience, both in ordinary (mousy) cases as well as in science. Very few knowledge claims in such circumstances are not defeasible and probably no scientific realist claims are not defeasible. Still, we intuitively feel warranted in believing many such knowledge claims--'intuitively', here, means that no universally recognized necessary and sufficient conditions have yet been established for knowledge. My arguments in this chapter have tried to establish weaker, but analogous claims for some realist assertions. If we can agree that some levels of evidential support via the three virtues I've listed can support reality claims, then the (conceptual) case for my formulation of realism is established. Imagine, again, a theory that is as modest, general, and determinate as you wish (at the limit, all relevant data are handled in a modest, general, and determinate manner), then I would argue that we have good reasons to believe the assertions of the theory in question. That actual cases in the history of science can be argued to have achieved a level of virtues sufficient to warrant belief is, admittedly, harder to establish. This is where 'benign chauvinism' and 'being on the right track' come in, as well as the argument that realism gives the best account for the success of some actual scientific theories. This is relevant for the historical objections to my version of scientific realism, that will be discussed in the next chapter. At the present, conceptual, level, however, I am still free to rest my case on 'ideal' situations (Van Fraassen's conceptual support for constructive empiricism, if anything, is even less historically adequate, in that it fails to provide more than a few token historical cases).[143] I will now try to establish historical adequacy for the version of scientific realism that I have developed in this chapter.

NOTES

[1]W. V. Quine and J. S. Ullian, The Web of Belief (New York: Random House, 1978), pp. 66-82.

[2]For example, Joseph Priestley, The History and Present State of Electricity, With Original Experiments, Vol. II (New York: Johnson Reprint Corporation, 1966/1775). "(We think it) an argument in favor of any system, if it exhibits a variety of effects springing from a few causes," p. 12. Sir Isaac Newton, Sir Isaac Newton's Mathematical Principles of Natural Philosophy and His System of the World, trans. Andrew Motte, Florian Cajori (Berkeley: University of California Press, 1962/1713). "We are to admit no more causes of natural things than such as are both true and sufficient to explain their appearances. . . . for Nature is pleased with simplicity, and affects not the pomp of superfluous causes," p. 398. Albert Einstein, "Physics and Reality," Journal of the Franklin Institute, Vol. 221, No. 3, 1936, reprinted in Albert Einstein, Idea and Opinions (New York: Dell, 1979). "The aim of science is, on the one hand, a comprehension, as complete as possible, of the connection between the sense expressions in their totality, and, on the other hand, the accomplishment of this aim by the use of a minimum of primary concepts and relations. (Seeking, as far as possible, logical

unity in the world picture, i.e., paucity in logical elements.)," p. 286.

[3]Elliot Sober, Simplicity (Oxford: Clarendon Press, 1975). "The diversity of our intuitions about simplicity is matched only by the tenacity with which these intuitions refuse to yield to formal characterization. Our intuitions seem unanimous in favor of sparse ontologies, smooth curves, homogenous universes, invariant equations, and impoverished assumptions. Yet recent theorizing about simplicity presents a veritable chaos of opinion," p. vii. Mary Hesse, The Structure of Scientific Inference (Berkeley: University of California Press, 1974). "Even superficial investigation of simplicity soon reveals that there is not one but many types of concept involved," p. 223. Mario Bunge, "The Weight of Simplicity in the Construction and Assaying of Scientific Theories," Philosophy of Science, Vol. 28, 1961, reprinted in Alex C. Michalos, ed., Philosophical Problems of Science and Technology (Boston: Allyn and Bacon, 1974). "Simplicity is not a single kind but, on the contrary, is a complex compound; furthermore simplicity is not a characteristic isolated from other properties of scientific systems, and it often competes with further desiderata, such as accuracy," p. 409.

[4]See Karl R. Popper, The Logic of Scientific Discovery (New York: Harper and Row, 1968/1959), p. 125-45. Clark Glymour, Theory and Evidence (Princeton: Princeton University Press, 1980), esp. pp. 323-35.

[5]See, Bunge's analysis of "Syntactical Simplicity," op. cit., p. 410.

[6]W. V. O. Quine, Word and Object (Cambridge: MIT Press, 1970/1960), discussion of 'sufficient reason', p. 21.

[7]Philip Kitcher, "Explanatory Unification," Philosophy of

Science 48 (1981), esp. pp. 519-530.

[8]W. V. O. Quine, "On Multiplying Entities," in The Ways of Paradox: and Other Essays (Cambridge: Harvard University Press, 1977/1966). "Of itself multiplication of entities should be seen as undesirable, conformably with Occam's razor, and should be required to pay its way. Pad the universe with classes or other supplements if that will get you a simpler, smoother overall theory; otherwise don't. Simplicity is the thing, and ontological economy is one aspect of it, to be averaged in with others. We may fairly expect that some padding of the universe is in the interest of the overall net simplicity of our system of the world," p. 264.

[9]G. Schlesinger, Method in the Physical Sciences (New York: Humanities Press, 1963), pp. 32-44.

[10]Schlesinger, op. cit., p. 41. See also Derek J. de S. Price, "Contra Copernicus: A Critical Re-estimation of the Mathematical Planetary Theory of Ptolemy, Copernicus, and Kepler," in Marshall Claggett, ed., Critical Problems in the History of Science (Madison: University of Wisconsin Press, 1959), pp. 197-218, and Thomas S. Kuhn, The Copernican Revolution: Planetary Astronomy in the Development of Western Thought (Cambridge: Harvard University Press, 1976/1959), pp. 134-184, esp. pp. 171-184.

[11]See Schlesinger's objections to such attempts in the earlier pages of his chapter on simplicity, op. cit., pp. 8-32.

[12]Gerd Buchdahl, "Sources of Scepticism in Atomic Theory," British Journal for the Philosophy of Science, Vol. 10, No. 38, 1959.

[13]Ibid., p. 120.

[14] Ibid.

[15] For example, Jean Baptiste Andre Dumas, Lecons sur la Philosophie Chimique (Paris: Gauthier-Villars, 1878/1836). "Ma conviction, c'est que les équivalents des chimistes, . . . ce que nous appelons atomes, ne sont autre chose que des groupes moléculaires. Si j'en étais le maître, j'effacerais le mot atome de la science, persuadé qu'il va plus loin que l'expérience; et jamais en Chimie nous ne devons aller plus loins que l'expérience," p. 315. (My conviction is that the 'equivalents' of the chemists, . . . that we call 'atoms', are nothing else but molecular groups [smallest physical particles that compose matter, carrier of electric charge, etc.]. If I were master, I would erase the word 'atom' from science, persuaded that it goes further than experience; and never in chemistry should we go further than experience.)

[16] Buchdahl, op. cit., esp. pp. 131-134.

[17] Gerd Buchdahl, "History of Science and Criteria of Choice," in Roger H. Stuewer, ed., Historical and Philosophical Perspectives of Science (Minneapolis: University of Minnesota Press, 1970).

[18] Ibid., p. 205.

[19] Ibid., p. 206. The latter example is interesting in the history of science because of the change in considerations concerning what is basic to scientific explanations from mechanical systems to a scientific ontology including forces. For a good, brief, historical discussion of this see Theodore Mischel, "Pragmatic Aspects of Explanation," Philosophy of Science 33/1-2 (1966). For a participants view, see James Clerk Maxwell, "Attraction," in The Scientific Papers of J. C. Maxwell, Vol. II (Paris: Libraire Scientifique, 1890), pp. 485-491, and "On

Action At A Distance," in *ibid*., pp. 311-323.

[20]"History of Science and Criteria of Choice," *op*. *cit*., pp. 206-208. For Einstein's treatment of 'simultaneity', see Albert Einstein, "On the Electrodynamics of Moving Bodies," in W. Perrett and G. B. Jeffrey, trans., *The Principle of Relativity: A Collection of Original Memoirs on the Special and General Theory of Relativity* (New York: Dover, 1952/1923), pp. 38-48. For Maxwell's treatment of 'dynamical explanations' see James Clerk Maxwell, "On the Dynamical Evidence of the Molecular Constitution of Bodies," in *Scientific Papers*, *op*. *cit*., p. 418.

[21]"History of Science and Criteria of Choice," p. 208.

[22]*Ibid*., p. 214.

[23]Examples of accusations of there being 'occult forces' in Newton can be found in Alexandre Koyre, "Concept and Experience in Newton's Scientific Thought," in *Newtonian Studies* (Chicago: University of Chicago Press, 1954), esp. pp. 38-39. Gottfried Wilhelm Leibniz, Letter to Clark, Feb. 25, 1716 in H. G. Alexandre, ed., *The Leibniz-Clarke Correspondence: With Extracts from Newton's Principia and Optics* (New York: Barnes and Noble, 1956). "If God would cause a body to move free in the aether round about a certain fixed centre, without any other creature acting upon it: I say, it could not be done without a miracle; since it cannot be explained by the nature of bodies," p. 30. Roger Cotes, in the Preface to the second edition of the *Principia*, 1713, in Motte and Cajori, trans., *op*. *cit*., esp. pp. xxvi-xxvii. For Newton's own uncomfortableness with 'action at a distance', see Letter to Bentley, Feb. 25, 1692/93, in H. S. Thayer, ed., *Newton's Philosophy of Nature: Selections From His Writings* (New York: Hafner, 1974/1953), p. 54.

Gerd Buchdahl, "Gravity and Intelligibility: Newton to Kant," in Robert Butts and John Davis, eds., The Methodological Heritage of Newton (Toronto: University of Toronto Press, 1970), esp. pp. 81-89.

[24]"Gravity and Intelligibility . . . ," op. cit., pp. 82-87. On the other hand, Maxwell presents the case for attractive and repulsive forces almost as if they had been dynamically demonstrated. That is, he makes it sound as if the matter could be handled at the constitutive level. In fact, he utilizes a component of the overall simplicity of dynamical systems that Kitcher calls 'unification', and I will subsequently call 'generality'. James Clerk Maxwell, "Attraction," in op. cit., "There can be no doubt that a path is now open by which we may trace to the action of a medium all forces which, like the electric and magnetic forces, vary inversely as the square of the distance, and are attractive between bodies of different names, and repulsive between bodies of the same names," p. 488.

[25]Maxwell, for example, presents this conceptual analysis in terms of the concept of 'energy'. "Attraction," op. cit., "We may therefore express the fact that there is attraction between the two bodies by saying that the energy of the system consisting of the two bodies increases when the distance increases. The question, therefore, 'Why do the two bodies attract each other?' may be expressed in a different form. 'Why does the energy of the system increase when the distance increases?'," p. 486.

[26]For a brief discussion of the 'internal' and 'external' approaches to the history of science, see Mary Hesse, "Hermeticism and Historiography: An Apology for the Internal History of Science," in

Stuewer, ed., Historical and Philosophical Perspectives of Science, op. cit., esp. p. 135.

[27] See, for example, Dirk Struik's interpretation of the development of mathematics in the West as due to advances in trade, and the accounting techniques and technological problems associated with it. Dirk Struik, A Concise History of Mathematics (New York: Dover, 1967/1948), esp. pp. 85-114. Also see the excerpt from Boris Hessen's The Social and Economic Roots of Newton's Principia, in Willis H. Truitt and T. W. Graham Solomons, eds., Science, Technology, and Freedom (Boston: Houghton Miflin, 1974), pp. 89-99.

[28] See Mary Jo Nye, "N-Rays: An Episode in the History and Psychology of Science," Historical Studies in the Philosophy of Science, Vol. 11, No. 1, 1980, where she discusses four factors in the structure of the French scientific community at the turn of the century that at least partially account for Blondot's non-existent rays. See esp. pp. 144-56.

[29] Thomas S. Kuhn, The Copernican Revolution, p. 172.

[30] Clark Glymour, Theory and Evidence, pp. 178-203.

[31] Mary Jo Nye, "The 19th Century Atomic Debates and the Dilemma of an 'Indifferent Hypothesis'," Studies in the History and Philosophy of Science, Vol. 7, No. 3 (1976), pp. 249-50.

[32] Ibid., p. 251, and "It was not merely a Gestalt-switch or an intuitive leap which suddenly made the atomic hypothesis more than an indifferent hypothesis. It was experiment, some of it due to new instruments like the vacuum tube and the ultramicroscope, which revitalized the atomic programme," p. 268.

[33]See, for example, Gerald Holton, "Johannes Kepler's Universe," in _Thematic Origins of Scientific Thought: Kepler to Einstein_ (Cambridge: Harvard University Press, 1975/1973), pp. 76-85. Marie Boas, _The Scientific Renaissance: 1450-1630_ (New York: Harper Torchbooks, 1966/1962), pp. 296, 302-305, 307-10.

[34]Marie Boas, ibid., "There is little in Kepler of the neo-Platonic number nonesense of the late 15th century--entirely self-sufficient--or of the religico-philosophic pantheism of Geordono Bruno. To Kepler his newly discovered mathematical harmonies were so many laws which revealed the wonder and order of the world of God; this was a world ruled by mathematical law, which in turn was discoverable by astronomical observation," p. 309.

[35]For problems with a realist interpretation of 'curve-fitting', see Clark Glymour, _Theory and Evidence_, pp. 322-40. And, for a clearly pragmatic interpretation of 'curve-fitting', see W. V. O. Quine, "On Simple Theories of a Complex World," in _The Ways of Paradox_, pp. 255-58.

[36]Quine and Ullian, _The Web of Belief_, pp. 68-69. It should be noted that I am _only_ borrowing the _term_ 'modesty', as I am uncomfortable with Quine's and Ullian's treatment of it, and the other 'virtues'.

[37]The terminology here comes from Clark Glymour, _Theory and Evidence_, p. 144.

[38]Albert Einstein and Leopold Infeld, _The Evolution of Physics: From Early Concepts to Relativity and Quanta_ (New York: Simon and Schuster, 1966/1938), pp. 119-20.

[39]_Sir Isaac Newton's Principles of Natural Philosophy_, p. 547.

[40]See, Letter to Boyle, Feb. 28, 1678/79 in _Newton's Philosophy_

of Nature, pp. 112-16, Letter to Oldenberg, Jan. 25, 1675, ibid., pp. 82-99, Letter to Bentley, Dec. 10, 1692, ibid., pp. 46-50. Sir Isaac Newton, Optics (New York: Dover, 1979/1730), pp. 339-406. For the view of one historian on the passage in the 'General Scholium', see N. R. Hanson, "Hypotheses Fingo," in The Methodological Heritage of Newton, pp. 14-33.

[41] See Cajori's appendix to the Principia, vol. II, no. 55, pp. 671-76.

[42] Newton's Optics, pp. 338-39.

[43] Principia, p. 398.

[44] Ibid., p. 400.

[45] Joseph Priestley, The History and Present State of Electricity, p. 14.

[46] Ibid.

[47] Ibid., p. 15.

[48] Ibid., p. 16.

[49] Ibid., pp. 16-18.

[50] Letter from Benjamin Franklin to Mr. Kinnersly, Feb. 20, 1762, in I. Bernard Cohen, ed., Benjamin Franklin's Experiments: A New Edition of Franklin's Experiments and Observations on Electricity (Cambridge: Harvard University Press, 1941), pp. 366-67.

[51] Franklin called Nollet an "uncompromising hypothesis maker," see Letter to Kinnersley, p. 367. Priestley criticizes his proliferation of pores for emitting and receiving the effluent and affluent electrical currents. "A man of less ingenuity than the Abbé could not have maintained himself in such a theory as this; but with his fund of

invention, he was never at a loss for resources upon all emergencies, and in his last publication appears to be as zealous for this strange hypothesis as at the first." Joseph Priestley, The History and Present State of Electricity, vol. II, p. 23.

[52]"I would freely appeal to the occular testimony of any unprejudiced person." Abbe Nollet, Lecons de Physique, p. 363, quoted and translated by Priestley in The History and Present State of Electricity, vol. II, p. 24. See also this entire section of Priestley's account, ibid., pp. 22-25.

[53]See Ida Freund, The Study of Chemical Composition: An Account of its Methods and Historical Development (New York: Dover, 1968/1904), pp. 55-56. Douglas Mckie, Antoine Lavoisier: Scientist, Economist, Social Reformer (New York: Henry Schuman, 1952), esp. pp. 147-58.

[54]Joseph Priestley, Experiments and Observations on Different Kinds of Air: and Other Branches of Natural Philosophy Connected With the Subject (Birmingham: Thomas Pearson, 1790), vol. III, "And did facts correspond on this theory, it would certainly be preferable to that of Stahl, as being more simple; there being one principle less to take into our account in explaining the changes of bodies," p. 541. That the facts do not, for Priestley, support the otherwise admittedly more modest theory, see ibid., pp. 540-63. See also his Letter to William Withering, Oct. 27, 1795, in Robert E. Schofield, ed., A Scientific Autobiography of Joseph Priestley, 1733-1804: Selected Scientific Correspondence, With Commentary (Cambridge: MIT Press, 1966), pp. 287-89, and Letter to Berthollet, La Place, Fourcroy, and Hasenfratz, June 15, 1796, ibid., pp. 289-90. The objections mentioned in this letter are

those listed in Experiments and Observations on Different Kinds of Air: and Other Branches of Natural Philosophy Connected With the Subject, vol. III, pp. 540-563. See also, S. E. Toulmin, "Crucial Experiments: Priestley and Lavoisier," Journal of the History of Ideas, vol. 18, no. 2, 1957, pp. 205-20.

[55]For a good account concerning the agreement over 'criteria' in the 19th century atomic debates, see Michael R. Gardner, "Realism and Instrumentalism in 19th-Century Atomism," Philosophy of Science, vol. 46, no. 1, 1979, pp. 1-34.

[56]The 'new chemistry' was generally accepted as more modest early in the 19th century. Copernican-Keplerian astronomy was generally accepted after a system of physics was developed to account for it by Galileo and Newton. The atomic theory was generally accepted by the early 20th century. The wave/particle debates concerning light were generally resolved after the work of Young and Fresnel, to be later resurrected in a new guise in the 20th century.

[57]See Albert Einstein, "Principles of Research," in Ideas and Opinions, ". . . one might suppose that there were any number of possible systems of theoretical physics, all equally well justified; and this opinion is no doubt correct, theoretically. But the development of physics has shown that at any given moment, out of all conceivable constructions, a single one has always proved itself decidedly superior to all the rest," p. 221. While Einstein's statement here is too extreme, I think it can be shown that increasing experimental development decreases the number of theories that can be held to satisfy the virtues I am delineating.

[58]I have again borrowed this term from Quine and Ullian. See _The Web of Belief_, pp. 73-79.

[59]Charles Darwin, _The Origin of Species: By Means of Natural Selection or the Preservation of Favoured Races in the Struggle for Life_ (New York: Mentor, 1958/1859). "It can hardly be supposed that a false theory would explain, in so satisfactory a manner as does the theory of natural selection, the several large classes of facts above mentioned," p. 442.

[60]A. Einstein, "On the Electrodynamics of Moving Bodies," esp. pp. 48, 49, 57, 59, and 63.

[61]Albert Einstein, "On the Generalized Theory of Gravitation" in _Ideas and Opinions_, pp. 332-33.

[62]J. C. Maxwell, "On the Dynamical Evidence for the Molecular Constitution of Bodies," p. 436.

[63]J. C. Maxwell, "Faraday," in _Scientific Papers_, vol. II, p. 358.

[64]J. C. Maxwell, "On Action At A Distance," p. 322.

[65]Joseph Priestley, _The History and Present State of Electricity_, p. 17.

[66]Ibid., p. 18.

[67]See Gerd Buchdahl, "Sources of Scepticism in Atomic Theory," esp. pp. 125-27.

[68]See Michael R. Gardner, "Realism and Instrumentalism in 19th-Century Atomism," pp. 14-23. As with the case of modesty and generality borrowed from Quine and Ullian, I am only borrowing the _term_ 'determinateness' here.

[69]For the last example in particular see Clark Glymour, Theory and Evidence, pp. 184-85, 196-98.

[70]Benjamin Franklin's Letter to Peter Collinson, April 29, 1749, in Benjamin Franklin's Experiments, "For if, on the explosion, the electrical fire came out of the bottle by one part, and did not enter in again by another, then if a man, standing on wax and holding the bottle in one hand, takes the spark by touching the wire hook with the other, the bottle thereby *discharged*, the man would be *charged*; or whatever fire was lost by one, would be found in the other, since there was no way for its escape. But the contrary is true," p. 189.

[71]Ida Freund, The Study of Chemical Composition, pp. 330-40. Gerd Buchdahl, "Sources of Scepticism in Atomic Theory," pp. 126-28.

[72]Freund, ibid., pp. 340-58. Buchdahl, ibid., p. 130.

[73]At least any *modern* scientific theory. It is debatable, for example, whether Eudoxus cared whether or not his concentric spheres 'saved the phenomena'. Also, 'saving the phenomena' is not univocal in ancient sources, given the ambiguous use of 'των φαινομενων' in Aristotle. See G. E. L. Owen, "Tithenai ta Phainomena" in Jonathan Barnes, Malcolm Schofield, and Richard Sorabji, eds., Articles on Aristotle: vol. I, Science (London: Duckworth, 1975), pp. 113-26.

[74]Thomas S. Kuhn, The Structure of Scientific Revolutions (Chicago: University of Chicago Press, 1970/1962). "Paradigm debates are not really about relative problem-solving ability, though for good reasons they are usually couched in those terms. Instead, the issue is which paradigm should in the future guide research on problems many of which neither competitior can yet claim to resolve completely. A

decision between alternative ways of practicing science is called for, and in the circumstances that decision must be based less on past achievement than on future promise. The man who embraces a new paradigm at an early stage must often do so in defiance of the evidence provided by problem solving. He must, that is, have faith that the new paradigm will succeed with the many large problems that confront it, knowing only that the older paradigm has failed with a few. A decision of that kind can only be made on faith," pp. 157-58. Max Planck, Scientific Autobiography and Other Papers, Frank Gaynor, trans. (New York: Philosophical Library, 1949). "The famous controversy between Newton's emission theory and Huygen's wave theory of light is also a phantom problem of science. For every decision for or against either of these two opposing theories will be a completely arbitrary one, depending on whether one accepts the point of view of the quantum theory or that of the classical theory," p. 59. Paul K. Feyerabend, "Against Method: Outline of an Anarchist Theory of Knowledge," in Michael Radner and Stephen Winokur, eds., Analysis of the Theories and Methods of Physics and Psychology (Minneapolis: University of Minnesota Press, 1970). "What attitude shall we adopt toward the various theories of confirmation and corroboration . . . ? . . . These theories are now all quite useless. They are as useless as a medicine that heals a patient only if he is bacteria free. In practice they are never obeyed by anyone. Methodologists may point to the importance of falsifications--but they blithely use falsified theories; they may sermonize how important it is to consider all the relevant evidence, and never mention those big and drastic facts which show that the theories which they admire and accept, the theory of relativity, the quantum theory, are at least as badly off as the older theories which they reject. In practice methodologists slavishly repeat the most recent pronouncements of the top dogs in physics . . . ," p. 42.

[75]P. W. Bridgman, The Logic of Modern Physics (New York: Macmillan, 1960/1927).

[76]For a good, brief discussion of this approach, see Carl G. Hempel, "Empiricist Criteria of Cognitive Significance: Problems and Changes," and "The Theoreticians Dilemma: A Study in the Logic of Theory Construction," both in Aspects of Scientific Explanation: and Other Essays in the Philosophy of Science (New York: Free Press, 1965), and Frederick Suppe, ed., The Structure of Scientific Theories (Urbana: University of Illinois Press, 1974), pp. 3-61.

[77]This latter view, previously held by Mach, exercised considerable influence on the history of positivism. See Ernst Mach, The Science of Mechanics: A Critical and Historical Account of Its Development, Thomas J. McCormack, trans. (Lasalle: Open Court, 1942/1883). "The atomic theory plays a part in physics similar to that of certain auxiliary concepts in math: it is a mathematical model for facilitating the mental reproduction of facts. Although we represent vibrations by the harmonic formula, the phenomena of cooling by exponentials, falls by the squares of times, etc., no one will fancy that vibrations in themselves have anything to do with the circular functions, or the motion of falling bodies with squares. It has simply been observed that the relations between the quantities investigated were similar to certain relations obtaining between familiar mathematical functions, and

these more familiar ideas are employed as an easy means of supplementing experience," p. 590. See also, H. Poincare, Science and Hypotheses (New York: Dover, 1952/1905). "The object of mathematical theories is not to reveal to us the real nature of things; that would be an unreasonable claim. Their only project is to coordinate the physical laws with which physical experience makes us acquainted, the enumeration of which, without the aid of math, we should be unable to effect," p. 211. This is also the spirit in which to understand Hertz' famous claim that Maxwell's theory is his equations.

[78]Mach, for example, values 'economy of thought', ibid., "My conception of economy of thought was developed out of my experience as a teacher, out of the work of practical instruction. . . . I should most certainly characterize items of simplicity and beauty which so distinctly mark the work of Copernicus and Galileo, not only as aesthetical, but also as economical," pp. 592-93. See also Pierre Duhem, The Aim and Structure of Physical Theory, Philip Wiener, trans. (New York: Athenium, 1977/1905). "Our physical theories do not pride themselves on being explanations; our hypotheses are not assumptions about the very nature of material things. Our theories have as their sole aim the economical condensation and classification of experimental laws; they are autonomous and independent of any metaphysical system," p. 219.

[79]The controversy concerning the observational/theoretical distinction constitutes one of the most prolific areas in philosophy of science in this century. The following comprise a small sample of philosophical and historical works at least partially devoted to this issue. Peter Achinstein, Concepts of Science: A Philosophical

Analysis (Baltimore: Johns Hopkins, 1971/1968), pp. 67-119, 157-201. Norwood Russell Hanson, Patterns of Discovery (Cambridge: Cambridge University Press, 1972/1958), pp. 4-49. Frederick Suppe, The Structure of Scientific Theories, pp. 66-86, 192-99. Hilary Putnam, "What Theories Are Not," in Mathematics Matter and Method: Philosophical Papers, vol. I (Cambridge: Cambridge University Press, 1980/1975). Rudolf Carnap, "Empiricism, Semantics, and Ontology," in Meaning and Necessity: A Study in Semantics and Modal Logic (Chicago: University of Chicago Press, 1975).

[80]Carl G. Hempel, Philosophy of Natural Science (Englewood Cliffs: Prentice Hall, 1966). "Theories are usually introduced when previous study of a class of phenomena has revealed a system of uniformities that can be expressed in the form of empirical laws. Theories then seek to explain those regularities and, generally, to afford a deeper and more accurate understanding of the phenomena in question," p. 70. Stephen Toulmin, Foresight and Understanding: An Enquiry into the Aims of Science (New York: Harper Torchbooks, 1963). "We started out to explain explanation in terms of prediction; but now prediction itself can be made to meet the case, only if we import into it the idea of 'explaining' and 'making sense of' natural connections," p. 34. Norman Cambell, What Is Science? (New York: Dover, 1952/1921). "Even if we were sure that all possible laws had been found and that all the external world of nature had been completely ordered, there would still remain much to be done. We should want to explain the laws," p. 77. Ernest Nagel, The Structure of Science: Problems in the Logic of Scientific Explanation (New York: Harcourt, Brace, and World, 1961). "It is

the desire for explanations which are at once systematic and controllable by factual evidence that generates science; and it is the organization and classification of knowledge on the basis of explanatory principles that is the distinctive feature of the sciences," p. 4. R. B. Braithwaite, Scientific Explanation: A Study of the Function of Theory, Probability, and Law in Science (Cambridge: Cambridge University Press, 1968/1953). "Science as it advances, does not rest content with establishing simple generalizations from observable facts: it tries to explain these lowest-level generalizations by deducing them from more general hypotheses," p. ix.

[81]See, for example, Toulmin, ibid., Michael Scriven, "Explanation, Prediction, and Laws," in Herbert Feigl and Grover Maxwell, eds., Scientific Explanation, Space, and Time (Minneapolis: University of Minnesota Press, 1971/1966). Sylvain Bromberger, "Why-Questions" in Robert G. Colodny, ed., Mind and Cosmos: Essays in Contemporary Science and Philosophy (Pittsburgh: University of Pittsburgh Press, 1966).

[82]Michael Friedman, "Explanation and Scientific Understanding," Journal of Philosophy, vol. 71, no. 1 (1974), p. 15.

[83]Ibid., pp. 14-15. Philip Kitcher has recently reformulated the formal component of Friedman's 'unification' in "Explanatory Unification," op. cit.

[84]Paul Thagard, "The Best Explanation Criteria for Theory Choice," Journal of Philosophy, vol. 75, no. 2 (1978).

[85]Ibid., p. 91.

[86]Wesley C. Salmon, "Why ask Why?", Proceedings and Addresses of the American Philosophical Association, vol. 51, no. 6 (1978), pp. 698-99.

[87]Philip Frank, Between Physics and Philosophy (Cambridge: Harvard University Press, 1941/1929). "One always obtains the same numerical value for the Planck constant h, using various methods. . . . The theory in which h plays a role then asserts that all the various groups of experiences, which are qualitatively so different from one another, nevertheless should give the same numerical value of h. It is therefore only a matter of comparing experiences with one another. This procedure, which the physicist is accustomed to use in his work, has been made by Mach and James into a general conception of the criterion of truth," p. 74. See also pp. 81-82. And Bridgman, The Logic of Modern Physics, op. cit., p. 59.

[88]R. A. Fumerton, "Induction and Reasoning to the Best Explanation," Philosophy of Science, vol. 47, no. 4 (1980).

[89]W. V. Quine, "On Simple Theories of a Complex World," pp. 255-58.

[90]Clark Glymour, "Causal Inference and Causal Explanation," in Robert McLaughlin, ed., What? Where? When? Why? (Dordrecht: Reidel, 1982). "Many Philosophers, including myself, lust for a good argument for scientific realism. Too often we settle for an argument of a kind we would dismiss out of hand if it came from other voices with other aims. I think Salmon's argument about the amazing coincidence between the values of Avogadro's number determined from molecular kinetic theory and from the electrochemical theory of electrolysis is of that kind. I certainly concur with him that the agreement gives us reason to believe the atomic theory; I cannot believe that it does so because, without the atomic theory, the coincidence would be nearly miraculous. The world is

full of surprising numerical agreements: there are any number of systems of quasi physics and plain pseudo-physics founded on little more than the amazing agreement of combinations of physical constants," p. 190. Bas C. Van Fraassen, "Rational Belief and the Common Cause Principle," in What? Where? When? Why?, op. cit., "I cannot regard the principle of the common cause as a principle of rational belief or inference. It is in my opinion, instead a tactical maxim of scientific inquiry and theory construction. There is no belief, in the case of tactics, that they must be successful. Whether there can be epistemological principles that rationally compel beliefs going logically beyond what we accept as data, is a large question. In the case of the present principle, I am inclined to say that its acceptance does not make one irrational; but its rejection is rationally warranted as well," p. 209.

[91] Again, this convergence of opinion seems accurate at the conceptual level. Some historical anti-realists maintain that these virtues are not objective. This will be taken up in the next chapter.

[92] Ronald N. Giere, Understanding Scientific Reasoning (New York: Holt, Rinehart and Winston, 1979).

[93] Ibid., pp. 84-94.

[94] The language used here should not be understood in terms of Bayesian probability models. 'Prior probability' and 'epistemic probability' are not here used as technical terms, but rather as alternate formulations of the intuitive notion of 'less likely', and the subsequent 'increased confidence' that I have been arguing for.

[95] Bas C. Van Fraassen, The Scientific Image (Oxford: Clarendon

Press, 1980). "Science aims to give us theories which are empirically adequate; and acceptance of a theory involves a belief only that it is empirically adequate. This is a statement of the anti-realist position I advocate; I shall call it constructive empiricism," p. 12.

[96]See, for example, Rudolf Carnap, "The Methodological Character of Theoretical Concepts," in Herbert Feigl and Michael Scriven, eds., The Foundations of Science and the Concepts of Psychology and Psychoanalysis (Minneapolis: University of Minnesota Press, 1976/1956). "Let 'M' be a theoretical term of VT (the theoretical vocabulary of the theory); it may designate a physical magnitude M. What does it mean for 'M' to be empirically meaningful? Roughly speaking, it means that a certain assumption involving the magnitude M makes a difference for the prediction of an observable event. More specifically, there must be a certain sentence 'Sm' about M such that we can infer with its help a sentence 'So' in Lo (the observational vocabulary of the theory)," p. 49.

[97]See many of the authors and works cited in footnote 80 above, and, for a good, brief summary of the subsequent attacks on the T/O distinction, and the 'surplus' empirical status of theoretical terms and statements, Grover Maxwell, "The Ontological Status of Theoretical Entities," in Scientific Explanation, Space, and Time, op. cit., pp. 3-27.

[98]Bas c. Van Fraassen, "A Formal Approach to the Philosophy of Science," in Robert G. Colodny, ed., Paradigms and Paradoxes: The Philosophical Challenge of the Quantum Domain (Pittsburgh: University of Pittsburgh Press, 1972), pp. 305-12. The Scientific Image, op. cit., "The syntactic picture of a theory identifies it with a body of theorems, stated in one particular language chosen for the expression of that

theory. This should be contrasted with the alternative of presenting a theory in the first instance by identifying a class of structures as its models. In this second, semantic, approach the language used to express the theory is neither basic nor unique; the same class of structures could well be described in radically different ways, each with its own limitations. The models occupy centre stage," p. 44. For a brief, general, and non-technical discussion of the semantic-model approach to scientific theories (as well as other references) see The Structure of Scientific Theories, op. cit., pp. 221-30.

[99]Bas C. Van Fraassen, "To Save the Phenomena," Journal of Philosophy, vol. 73, no. 18 (1976), p. 631.

[100]The Scientific Image, op. cit., p. 12. See also "A Formal Approach to the Philosophy of Science," op. cit. "The essential job of a scientific theory is to provide us with a family of models, to be used for the representation of empirical phenomena. On the one hand, the theory defines its own subject matter--the kind of systems that realize the theory; on the other hand, empirical assertions made by holders of the theory have a single form: the phenomena can be represented by the models provided," p. 310. And "To Save the Phenomena," op. cit. "A theory provides, among other things, a specification (more or less complete) of the parts of its models that are to be direct images of the structures described in measurement reports. . . . The structures described in measurement reports we may continue to call appearances. A theory is empirically adequate exactly if all appearances are isomorphic to empirical substructures in at least one of its models," p. 631.

[101]"A Formal Approach to the Philosophy of Science," op. cit.,

p. 312.

[102]"To Save the Phenomena," *op. cit.*, p. 630. "A Formal Approach to the Philosophy of Science," *op. cit.*, p. 312.

[103]"To Save the Phenomena," p. 630. See also, W. V. O. Quine "Epistemology Naturalized" in *Ontological Relativity and Other Essays* (New York: Columbia University Press, 1969).

[104]Sir Isaac Newton, *Sir Isaac Newton's Principles of Natural Philosophy and His System of the World*, *op. cit.*, p. 12.

[105]*Scientific Image*, pp. 44-45. "To Save the Phenomena," pp. 624-25.

[106]*Scientific Image*, p. 45.

[107]Sir Isaac Newton, *Sir Isaac Newton's Principles of Natural Philosophy and His System of the World*, Corollary 4 of the Laws of Motion, pp. 19-20.

[108]*The Scientific Image*, p. 4. See also, ibid., pp. 12-13, 46, 57, 71, 98, 100.

[109]But see Michael Friedman's review of *The Scientific Image* in *Journal of Philosophy*, vol. 79, no. 5 (1982), pp. 276-79.

[110]See Clark Glymour, *Theory and Evidence*, pp. 110-75, and "Relevant Evidence," *Journal of Philosophy*, vol. 72, no. 14 (1975).

[111]*Scientific Image*, p. 9.

[112]Bas C. Van Fraassen, "On the Radical Incompleteness of the Manifest Image," in Suppe and Asquith, eds., *Proceedings of the Philosophy of Science Association*, vol. II (Ann Arbor: Edwards Brothers, 1977), p. 335.

[113]Clark Glymour, "Explanations, Tests, Unity, and Necessity,"

Nous, vol. 14, no. 1 (1980), pp. 48-49.

[114]See, again, Michael Friedman, "Explanation and Scientific Understanding," op. cit., and Paul Thagard, "The Best Explanation Criteria for Theory Choice," op. cit.

[115]Against Friedman's formulation, for example, see Philip Kitcher, "Explanation, Conjunction, and Unification," Journal of Philosophy, vol. 73, no. 8 (1976).

[116]For example, Glymour's 'bootstrap strategy' may well succeed in tying 'generality' into testability. See also, Philip Kitcher's reformulation of Friedman's 'unification' principle in "Explanatory Unification," op. cit.

[117]Scientific Image, p. 98.

[118]Ibid., pp. 156-57.

[119]Ibid., p. 21.

[120]Ibid., p. 112, and "The Pragmatics of Explanation," p. 146.

[121]Ibid., pp. 28-31.

[122]Ibid., p. 31.

[123]Ibid.

[124]"Why-Questions," op. cit., pp. 92-93.

[125]Ibid., p. 93.

[126]Ibid., p. 92.

[127]See, for example, Wesley C. Salmon, Statistical Explanation and Statistical Relevance (Pittsburgh: University of Pittsburgh Press, 1971), pp. 71-76. Philip Kitcher, "Explanatory Unification," op. cit., pp. 522-26.

[128]Scientific Image: "The description of some accounts as an

explanation of a given fact or event, is incomplete. It can only be an explanation with respect to a certain relevance relation and a certain contrast class. These are contextual factors, in that they are determined neither by the totality of accepted scientific theories, nor by the event or fact for which an explanation is requested," p. 130.

[129] See Aristotle, Posterior Analytics (I, 13, 78a-78b12).

[130] Ibid., 78a22-31, 78b12-34.

[131] Aristotle, Physics (II, 3, 194b16-195b30).

[132] "Pragmatics of Explanation," op. cit., p. 148.

[133] Scientific Image, pp. 132-34.

[134] Ibid., p. 87.

[135] Ibid., p. 88.

[136] Charles W. Morris, "Foundations of the Theory of Signs," in Otto Neurath, Rudolf Carnap, and Charles Morris, eds., International Encyclopedia of Unified Science (Chicago: University of Chicago Press, 1955/1938), p. 108. See also Scientific Image, p. 89.

[137] Scientific Image, pp. 134-37.

[138] See, for example, David Kaplan, "On the Logic of Demonstratives," in Peter A. Frence, Theodore E. Uehling, Jr., and Howard Wettstein, eds., Contemporary Perspectives in the Philosophy of Language (Minneapolis: University of Minnesota Press, 1979).

[139] This was in direct response to 'common cause' theorists, again, in the context of realism via scientific explanation. Since I am not committed to this view, I also would seem to be innocent of expousing metaphysics, from Van Fraassen's perspective.

[140] Scientific Image, p. 99.

[141] Ibid., pp. 221-22.

[142] Ibid., pp. 19-20.

[143] See again Michael Friedman's review of _The Scientific Image_, _op. cit._, pp. 279-81.

CHAPTER IV

REALISM AND THE HISTORY OF SCIENCE

So far, though historical examples have been interspersed to document the use of the virtues I've delineated, I have concentrated on conceptual rather than historical objections to scientific realism. As indicated at the beginning of the last chapter, however, there are also historical objections to scientific realism that must be dealt with. These too can be divided into conceptual historical objections, stressing the alleged inability of the realist to interpret scientific progress in terms of a theory's closer approximation to the truth, and historical problems which have to do with interpreting actual historical cases of theory choice from the perspective of constitutive considerations, much less from the perspective of a realist interpretation. Often these two strategies are combined, arguing simultaneously that a realist approach is unintelligible, and that cases of actual theory choice do not fit into any reasonable realist framework. Still, it would be advisable to initially separate these considerations when responding to historical objections to realism. Consequently, this chapter will also be divided into three sections. The first will attempt to elucidate and respond to conceptual historical objections (that a realist approach is *in general* unsatisfactory), while the second and third will attempt to establish that at least some important historical examples are capable of being explicated by the realist approach I have presented. In the process, I will further explicate the notions of 'being on the right track' and 'benign chauvinism' introduced in the first chapter, and the comparative, diachronic version of realism that I have presented in chapters one and three.

The Problem of Scientific Progress and 'Truthlikeness'

One of the results obtained in chapter two was to establish that some of the more familiar attacks on scientific progress are at least partially the result of muddling the notions of the meaning and the denotation (or less formally, the reference) of scientific terms. It was there shown that Kuhn's and Feyerabend's arguments for the 'change in physical reference' of central theoretical terms engendered by changes in overall theory are unfounded or inconsistent, given any of the three current approaches to denotation. It should be clear from the last chapter that my diachronic approach to scientific realism depends upon some theories getting 'better' throughout their historical development, and that some of *these* theories receive enough evidential support via the three virtues to warrant rational belief. Consequently, while the last part of chapter two argued against *basing* realist claims on theories of denotation, they are nonetheless *necessary* conditions for diachronic realism.

Conceptual historical anti-realism, however, is not limited to this muddling of meaning and denotation. There are much more general attacks on realism and even on the primacy of constitutive components in cases of theory choice, which also create problems for scientific realism. Briefly, if evidential support for theoretical claims cannot be separated from 'external', aesthetic and philosophical considerations in actual historical cases, then realism via scientific virtues, even if conceptually adequate, becomes historically sterile. If philosophy of science is to be about science, then philosophical arguments concerning science should have a bearing on real cases. It is the purpose of the present section to counteract historical objections that claim a realist approach cannot in principle adequately handle historical cases.

The main aspect of traditional scientific realist positions involves the alleged cumulative nature of scientific progress, which for a realist (in a variety of ways), is usually interpreted as a successful theory's closer and closer approximation to the truth. We have already dealt with some of the more familiar historical attacks on this general notion which are concerned with the concepts of the meaning and denotation (or more often and vaguely, reference) of theoretical terms, but there are a variety of other arguments against this general notion of cumulative progress, involving particular features that various realists have felt to be desirable.[1] Some of these include the notions of 'approximate truth', the retention of the explanatory successes of older theories by newer theories, the existence of 'crucial experiments', and the explanation of the success of a theory via its approximation to truth. Above and beyond these there are, of course, attacks on the meaning-invariance of scientific virtues (i.e., claims that methodological considerations change so drastically that one can only delineate virtues within given research traditions), and more general arguments against the ability to distinguish methodological virtues from other non-methodological decision criteria. This section will discuss representative arguments against all of these traditional realist desiderata, sometimes providing counterarguments, and sometimes arguing that the traditional desiderata are indeed reprehensible given the type of realism I'm advocating.

The general arguments concerning the ability to delineate virtues have already been partially addressed in the last chapter, and will be further supported in the next section. There is historical support for believing that, whatever other factors may be involved, some considerations weigh more heavily than others in theory choice, and that these have been fairly consistently employed throughout much of the history of science. Newton's laws, for example, were indeed controversial and for reasons that transcend normal methodological considerations. Perhaps, even, their eventual victory led to something like the acceptance of new perspectives concerning what were to count as legitimate theoretical concerns. Still, I maintain, the most important consideration was, and remains, their ability to determinately cover a wide range of data in a relatively modest manner--i.e., methodological considerations. The acceptance of new theories or research traditions may well lead to new methodological perspectives, but architechtonic and explicative components are never transformed without empirical and methodological <u>success</u>, and this success, while not univocal, is at least relatively constant.

Consider, for example, Laudan's claim that empirical problems are context-dependent, that is, they are empirical <u>because</u> a certain generation of scientists treats them as though they were objective problems about the world.[2] It was once an important empirical problem for geologists, for example, to explain how the earth could take shape within six to eight thousand years.[3] This problem was of course (from our perspective) never _solved_, but rather _dissolved_ when approaches were accepted that allowed for millions of years of development. Still, while we might deny empirical status to such a problem, are there any inherent differences between accepting it as an empirical problem given the prevalent contemporary theories, and our acceptance of determining the legitimacy or illegitimacy of Bell's Inequality regarding the behavior of photons in the singlet state? Once the theoretical/observational distinction is surrendered, are there any non-trivial (e.g., unlike 'this paper is white') examples of 'empirical data' that do not depend on the state of current theories? In other words, are there 'phenomena to be saved'?

Laudan, of course, maintains that there _are_ empirical problems for a theory to account for, but it seems that, for him, these are just what scientists who adopt the relevant theory postulate as empirical problems for that theory.[4] At least, for Laudan, methodological considerations do not so much justify a theory as they are themselves justified by the adherence to a theory.[5] If either of these claims is accepted, the 'success' of a theory, and, at least partially, the phenomena it saves, are determined by the theory itself. This result threatens not only scientific realism, but also claims concerning the objective status of scientific confirmation, and the _relevance_ of empirical evidence for a given theory. That is, even its degree of confirmation may not be directly relevant to the theory's ability to 'solve' empirical problems.

As Laudan claims:

> I shall claim that: a theory may solve a problem as long as it entails even an approximate statement of the problem; in determining if a theory solves a problem, it is irrelevant whether the theory is true or false, well or poorly confirmed; what counts as a solution to a problem at one time will not necessarily be regarded as such at all times.[6]

What exactly is it to 'solve' a problem if this does not mean that a well-confirmed theory handles it, or that this theory provides a true account of it? As far as I can tell, this is never explicitly spelled out by Laudan.[7] Still, there seems to be something both important and challenging about the above claims, especially since near-variants are advanced by several other historians.

Thomas Kuhn, for example, while in many respects more extreme than Laudan, seems to be in basic agreement with him when he argues that the (especially initial) results of measurements conducted to allegedly 'test' a theory are almost never in "unequivocal agreement" with the quantitative commitments of the theory.[8] Galileo, for example, in reporting his inclined plane experiments, gives no actual numbers, but claims that "in such experiments, repeated a full 100 times, we always found that the spaces traversed were to each other as the squares of the times."[9] In fact, however, according to Kuhn, "groups of the best in France announced their total failure to get comparable results."[10] Similarly, early 19th-century chemists did not have precise enough methods and instruments to unambiguously confirm Dalton's law of multiple proportions. Proust, for example, got a weight ratio for oxygen of 1.47:1 when analyzing copper oxides, as opposed to Dalton's prediction of 2:1.[11]

> As a result chemical texts can now state that quantitative analysis confirms Dalton's atomism and forget that, historically, the relevant analytic techniques are based upon the very theory they are said to confirm. Before Dalton's theory was announced, measurement did not give the same results. There are self-fulfilling prophecies in the physical as well as in the social sciences.[12]

In other words, there is, historically, an interplay between theory and experiment, both conditioning and helping to frame the other, rather than the unambiguous support of a theory by experimental evidence claimed by empiricist methodologists. Furthermore, a theory accepted as 'successful' helps to establish the methodology appropriate to itself, rather than being judged successful because of methodological considerations somehow independent of it.

A similar claim can be made concerning the case of Galileo. Although he was at least partially persuaded by his new telescopic 'facts' to champion the Copernican astronomical theory, this was certainly not his only reason, nor was it clearly his _main_ reason.[13] This 'saving of the phenomena' was not the reason he gave, for example, for favoring Copernicus' system in his _Dialogue Concerning the Two Chief World Systems: Ptolemaic and Copernican_,[14] since _both_ theories, strictly speaking, were refuted by careful observations. Instead he praises Copernicus (and Aristarchus before him) for trusting their _reason_ instead of their senses.

> There is no limit to my astonishment when I reflect that Aristarchus and Copernicus were able to make reason so conquer sense that, in defiance of the latter, the former became mistress of their belief. . . . Were it not for the existence of a superior and better sense than natural and common sense to join forces with reason, I much question whether I, too, should not have been much more recalcitrant toward the Copernican system than I have been since a clearer light than usual has illuminated me.[15]

And, a few pages later, he again praises Copernicus for favoring reason.

"We may see that with reason as his guide he resolutely continued to affirm what sensible experience seemed to contradict."[16] Of course, it would be irresponsible to conclude from these and similar passages either that Galileo never conducted experiments, or that he did not try to test his theories by experiment. Still, they seem to provide more historical support for the claims of those like Kuhn that the theoretical concepts accepted by scientists act partially as legislative concepts, or regulative ideals--i.e., that they partially determine what is observed.

Such challenges, of course, threaten the very notions of objective theory tests and context-independent notions of 'relevant' evidence. Fortunately, such positions are not forced upon us even if we abandon the theoretical/observational distinction. To maintain that theories can be objectively tested, and that certain experimental results are relevant to a theory's confirmation while others are not, one need only establish that it is possible to test parts of a theory (contra holism), and that the theory-evidence relation is not viciously circular. Clark Glymour has long maintained that the theoretical/observational distinction is not necessary for the objective testing of theoretical commitments, as long as the criteria used for the test are independent of the part of the theory being tested, whether these are theoretical or observational. In short, one can use other parts of the same theory to test the particular part of the theory in question.[17] Even if initial measurement results do not unambiguously support a given theory, the eventual convergence of these results, as well as a variety of different theoretical commitments being satisfied within the theory, can be established as context-independent evidence for the theory.

What justifies this claim for context-independent evidence? For Glymour, the evidence in such instances can turn out incompatible with the theory (the tests do not comprise 'self-fulfilling prophecies'). Furthermore, we have already established in chapter two that there are a variety of ways in which we can meaningfully claim that some later theories are 'about' the same entities as earlier ones, which also counters the extreme context-dependent claims made by some proponents of the incommensurability of succeeding theories. Finally, the previous chapter has provided at least some historical evidence that there is more agreement about methodological factors in cases of theory choice than these proponents of incommensurability indicate. This last response seems to me to constitute the real crux of this issue, and will be further supported in the next two sections. In any event, nothing presented thus far compels us to abandon the hope for the preponderance of objective, methodological factors in cases of theory choice, nor the hope that some of these factors can support diachronic realism.

The second traditional methodological desiderata that has increasingly come under attack concerns the possibility of conducting a 'crucial experiment' to empirically decide between conflicting theories.[18] Historical cases that have often been thought to count as such crucial experiments include Lavoisier's measuring the weight of a sealed container before and after calcination (oxidation) took place,[19] and Foucault's experiments showing that light traveled faster in air than in water.[20] Empiricists themselves have come to be sceptical concerning the existence of such crucial experiments. On purely methodological grounds, it has come to be believed that the failure of a given theory to agree with the results of a single experiment is not sufficient reason to single out that the <u>theory</u> is to blame for this failure. After all, the measurements might be in error, some known or unknown factor may have interfered with the results, the experimental apparatus used to conduct the experiment may have been inadequate, it may be possible to add 'friendly amendments' to the theory so that it would agree with the results, etc. Such considerations have led many empiricists to become disillusioned with the idea that a single experiment can confirm or disconfirm particular parts of a theory, and led them, rather, to adopt more holistic approaches to theory confirmation.[21] Without opting for the resurrection of the 'crucial experiment' concept, I think that the arguments for the possibility of objective tests of a theory provided above show that the holism option is not mandatory.

Non-empiricist attacks on crucial experiments are much more varied than those just listed. It is quite common historically, for example, for proponents of the theory 'refuted' by the alleged crucial experiment to fail to be swayed (examples: Priestley and Nollet discussed in the last chapter). For some historians, this failure to surrender to the success of a new theory is based on allegiance to the conceptual scheme of the older theory ('paradigm'), preventing them from recognizing the phenomena that the new theory could explain.[22] In other words, the persuasiveness of the experimental support for the new theory cannot be made logically compelling to those who do not accept the basic framework of the new theory.[23] In fact, it is claimed that proponents of the older theory often 'die out' without followers, rather than recognize (for purely methodological reasons) the greater success of the new theory.[24]

There is something to this historical objection to crucial experiments. As I've previously argued, there are many considerations operating in cases of theory choice, not all of them methodological (constitutive). It is not surprising, therefore, that there would be cases of individuals holding on to their research tradition in the face of what we can retrospectively call more successful theories. Furthermore, the diachronic model I've been developing must certainly allow for methodological disagreement at certain stages in the development of a given theory (at least concerning whether a theory can be claimed to have the relevant scientific virtues, and to what degree). It seems to me that both methodological and non-methodological considerations play a role in the kinds of cases cited in the last paragraph. At least some anti-atomists, for example, were convinced by Perrin's experiments concerning Brownian motion, and others may well have been, had they lived to witness those experiments.[25] It is also arguable that, given Dumas' original benevolence toward the atomic theory, his experimental results would have supported the theory had he made the appropriate distinction between 'atoms' and 'molecules' (i.e., he may have been persuaded by Cannizzaro's treatment had it occurred earlier). After his initial failure, however, and given his increasing allegiance to a more positivist position,[26] he may not have been swayed by Cannizzaro's successful use of this distinction in 1858.[27] Architechtonic and explicative components, in other words, do play a role in theory choice, and no doubt can lead individual scientists to maintain their scepticism in the face of highly 'virtuous' competitors. Still, this is not surprising given the diachronic model I've developed, and certainly does not lead to the incommensurability positions mentioned above. I have already argued that there is overwhelming convergence of opinion on the part of a majority of scientists after the development of a successful theory, and the next two sections will provide further support for the view that this convergence is primarily due to methodological (constitutive) considerations.

This interpretation concerning the primacy of methodological considerations in cases of theory choice has also been attacked in a more subtle (and, I think, more reasonable and persuasive) manner by Stephen Toulmin.[28] Considering the case of Lavoisier's 'crucial experiment' on the red calx of mercury against the phlogiston theory, Toulmin argues that Priestley's refusal to accept the new theory was not due to his pride, or to the incommensurability of the two theories, but to his own experimental results which seemed to contradict Lavoisier's position.

> Is it likely that such a man as Priestley, whose distinguished contributions to chemistry nobody can deny, could ever have been blind to the force of a crucial experiment in this field, if a genuinely crucial experiment were possible?
>
> In point of fact, Priestley was well aware of what Lavoisier had been doing, and saw clearly what the implications of this work might be. Writing in 1783 about the new oxygen theory he says: "The arguments in favour of this opinion, especially those which are drawn from the experiment of Mr. Lavoisier made on mercury, are so specious that I own I was myself much inclined to adopt them."[29]

Why then was Priestley reluctant to accept these results?

His own experiments on minium (lead oxide, Pb_3O_4) had convinced him that one could 'see' the imbibing of phlogiston in the calx (in the form of hydrogen), even more clearly than Lavoisier's experiment allegedly showed the contrary.[30] In a letter to Benjamin Franklin, dated June 24, 1782, Priestley reveals the gist of his experiment.

> . . . my experiments are certainly inconsistent with Mr. Lavoisier's supposition, of there being no such thing as phlogiston, and that it is the addition of air, and not the loss of anything that converts a metal into a calx. In their usual state calxes of metals do contain air, but that may be expelled by heat; and after this I reduce them to a perfect metalic state by nothing but inflammable air [hydrogen], which they imbibe in toto, without any decomposition. I lately reduced 101 ounce measures of this air to two by calx of lead, and that small remainder was still inflammable.[31]

Remember that for the phlogiston theorists calcination consisted in the loss of phlogiston from a metal, and the heating of the resulting calx (with, for example, carbon) reproduced the metal by reintroducing phlogiston.[32] Lavoisier's experiment reputedly demonstrated not only that something was added to the metal during calcination (oxidation), but the diminishing of the volume of air in the sealed container corresponded to the weight gain of the calx (hence, oxidation/calcination).[33] Priestley's experiment on lead oxide, therefore, attacked Lavoisier's results on two fronts. Not only does he provide a different account of the presence of 'air' in the calx, but he also seems to provide a clear case where phlogiston is necessary for the reconstitution of the metal (on the then not uncommon identification of phlogiston with inflammable air-hydrogen).[34]

What are we to make of Priestley's 'crucial experiment'? Well, as Toulmin points out, there are at least two important questions here.[35] The first is, roughly, what kind of chemical account do we give that would explain Priestley's result? Since the experiment was conducted in a bell jar of hydrogen enclosed over water, heating the lead oxide released oxygen which could then combine with the hydrogen to form water. Consequently, oxygen was given off in the reconstitution of the metal, rather than hydrogen being imbibed by the lead oxide as Priestley supposed.[36] The second question is, again roughly, what kind of criteria do we employ if we so interpret Priestley's experiment in Lavoisier's favor? If Lavoisier's experiment is taken as 'crucial', we seem to be committed to the view that <u>at the time</u> the experimental data were open to only one interpretation,[37] i.e., that Priestley's interpretation of his own experiment must be explained away, rather than appreciated as strong methodological support for phlogiston theory, given the state of chemistry at that time. Certainly Toulmin, in agreement with the diachronic model I've developed, agrees that <u>eventually</u> the available evidence converged to favor Lavoisier's theory. At the time in question, however, this was not the case. Consequently, though Kuhn's interpretation is extreme, there are good reasons for not taking Lavoisier's experiment as 'crucial', at least if the case in interpreted synchronically.

The details of the case, therefore, do not support an incommensurability argument, i.e., that the experiments on both sides were so theory laden that the two sides could not communicate with one another. Priestley agrees with Lavoisier, for example, on the <u>kinds</u> of virtues a theory needs to have in order to be successful--for example, modesty.

> And did the facts correspond on this theory [Lavoisier's], it would certainly be preferable to that of Stahl [phlogiston theory], as being more simple; there being one principle less to take into our account in explaining the changes of bodies.[38]

Furthermore, his refusal to take <u>weight</u> in the chemical reaction involved in calcination as the physical magnitude of prime importance is not, as many interpreters suppose, inherently less scientific (or even less quantitative) than Lavoisier's approach.[39] In general, besides the empirical results of his own experiment on lead oxide, his general position concerning his reluctance to accept the new theory seems methodologically sound. "If a former theory will sufficiently account for all the facts, there is no occasion to have recourse to a new one, attended with no peculiar advantage."[40] Considered at the time of his experiment, then, there is no evidence of a 'shift of world views' inherent in his refusal to accept Lavoisier's fledgling theory. The disagreement with the anti-phlogiston theorists concerns whether the virtues are present to a greater degree in the new theory, not which virtues are relevant nor what these virtues 'mean'.

Nothing that Toulmin says in this article leads me to believe that he would disagree with the above analysis. His attack on the possibility of crucial experiments is limited to a synchronic view of these experiments--considered at the time they were performed, not with respect to the future development of the relevant theories. I am in full agreement with his summary of the consequences of Lavoisier's and Priestley's experiments, and, in fact, see his interpretation as in full accord with the objectivity of methodological considerations that I am defending.

> We must acknowledge that Priestley's interpretation of his experiment is at least consistent. Whatever grounds we have for preferring our own, Lavoisierian explanations (and I would not dream of disputing the fact that they are vastly preferable to Priestley's) the original mercury experiment is by no means the crucial or the uniquely-vivid one it at first seemed. . . . The same compelling impression of seeing a chemical formula verified before one's eyes, which was so happily suggestive to Lavoisier, was equally misleading to Priestley.[41]

Therefore, from my perspective, there are no such things as single, crucial experiments if synchronically interpreted, but for reasons following from the above analysis, not because there is nothing objective or agreed upon by competing 'paradigms'. Insofar then as particular earlier empiricist (and/or realist) interpretations of scientific methodology were committed to such experiments, they were misguided. The absence of such experiments (or <u>any</u> synchronically interpreted methodological components for that matter), however, does no damage to the diachronic realist position I am defending.

The remaining traditional realist desiderata attacked by conceptual historical anti-realism (the retention, and often 'explanation' of the success of former theories by later ones, and notions of 'approximate truth') are often lumped together both by realists and anti-realists. Consequently, it will be appropriate to deal with them as a group. While there is a history of conceptual historical objections to these desiderata, the most important and current objections occur in various works by Larry Laudan. Laudan's arguments will therefore receive the most attention in what follows.

One of the ways in which earlier philosophers of science have tried to handle the retention of explanatory success in cases of theory change is via various forms of 'reduction' of the older theories into the newer, often involving the formal derivation of the former (or parts of it) by the latter.[42] Such a view has long been attacked by conceptual historians.[43] Laudan's attack on this particular version of this view (what he calls the 'extreme version of the cumulative postulate' (CP)) is both logical and historical. He first formulates this extreme model of (CP) as follows: "T_2 is progressive with respect to T_1, if and only if T_2 entails all the true consequences of T_1, plus some additional true consequences."[44] His <u>logical</u> objection to this position is simply that the class of true consequences of any two universal theories cannot be ennumerated, scuttling any attempt to directly compare the theories with respect to their true consequences.[45] His historical objection to this version of (CP) is equally simple.

> Sadly, none of the major cases of theory change in the history of science seems to satisfy the stringent conditions of entailment between theories required to decide whether absolute cumulativity has occurred. Copernican astronomy does not entail Ptolemaic astronomy; Newton's mechanics does not entail the mechanics of Galileo, Kepler, or Descartes; the special theory of relativity does not entail the Lorentz-modified aether theory; Darwin's theory does not entail Lamarck's; . . .[46]

Certainly Laudan is right in claiming that none of the above theories entailed their predecessors in this sense. There is possibly another sense of a theory entailing the measurement results of its predecessor, which may rescue part of the intuition behind this version of (CP) (at least for quantitative theories, like Newton's mechanics being a 'limiting case' for Einstein's). Since Einstein's equation for the kinetic energy of a material point is '$\frac{mc^2}{1-\frac{v^2}{c^2}}$', while the classical equation is '$m\frac{v^2}{2}$', it follows from Einstein's equation that, for objects with relatively small velocities with respect to the speed of light, the experimental results of the classical theory will not be measurably different from those predicted by the newer theory.[47] This, however, does not rescue this version of (CP) form all of Laudan's objections, nor will it apply to the less quantified theories he mentions in the above quote. In any event I have no wish to rescue a view that I find suspect for independent reasons, and which is totally irrelevant for the type of realism I'm defending.

A weaker version of the (CP) view surrenders the idea of the entailment of the former theory by the latter, and instead insists that: "T_2 is progressive with respect to T_1, if and only if all the facts thus far explained . . . by T_1 can be explained . . . by T_2, and T_2 can also be shown to explain some fact . . . not explained by T_1."[48] This is certainly intuitively more plausible than the extreme version of (CP) and doesn't share its logical flaws, but it too is overly strict to fit many important historical cases. Laudan discusses four important examples where phenomena were explained by an earlier theory, which the theory that replaced it couldn't even formulate.[49] The result, as far as Laudan is concerned, is that no version of (CP) is adequate to deal with the actual history of science. There is no reason to ennumerate all possible versions of (CP) to establish this point, because no cumulative view can cope with the historical fact (for Laudan) that most cases of theory change involve gains and losses of phenomena explained.[50] Hence, if scientific progress depends on the cumulation of explained phenomena (as is certainly assumed by all previous proponents of (CP) views), then science does not progress. Laudan instead opts for the view that scientific progress must be articulated in a non-cumulative manner, involving the number and importance of 'problems solved', rather than data explained, or progress toward truth, etc.[51] Here Laudan and I partially part company, but I need to clarify Laudan's further attacks on the 'retention of success' desideratum before I try to establish a new version of cumulative progress.

Laudan's further attacks on this desideratum, and especially the view that realist interpretations of theories explain the success of some theories, is found most succinctly and persuasively in his recent article "A Confutation of Convergent Realism."[52] The main stalking-horse in this article is labeled 'Convergent Epistemological Realism' (CER) by Laudan, and contains elements of the (CP) views already discussed, as well as new features particularly important for most realists. It will be worthwhile to begin this discussion by presenting Laudan's depiction of (CER), summarized by him in five theses.

> R1) Scientific theories (at least in the 'mature' sciences) are typically approximately true and more recent theories are closer to the truth than older theories in the same domain;
> R2) The observational and theoretical terms within the theories of a mature science genuinely refer (roughly, there are substances in the world that correspond to the ontologies presumed by our best theories);
> R3) Successive theories in any mature science will be such that they 'preserve' the theoretical relations and the apparent referents of earlier theories (i.e., earlier theories will be 'limiting cases' of later theories);
> R4) Acceptable new theories do and should explain why their predecessors were successful in so far as they were successful.
> To these semantic, methodological and epistemic theses is conjoined an important metaphilosophical claim about how realism is to be evaluated and assessed. Specifically, it is maintained that:
> R5) Theses R1-R4 entail that ('mature') scientific theories should be successful; indeed, these theses constitute the best, if not the only, explanation for the success of science. The empirical success of science (in the sense of giving detailed explanations and accurate predictions) accordingly provides striking empirical confirmation for realism.[53]

R3 has already been discussed in the preceding paragraphs, and at least until a new approach to cumulative progress is provided, I think Laudan is right in rejecting it. Some versions of the remaining four are important for my version of scientific realism, as well as most current realist positions. Consequently, Laudan's attacks on them need to be considered in some detail.

Hilary Putnam is one of the most prominent supporters of R1, R2, R4, and R5. He claims, for example, that:

> The typical realist argument against Idealism is that it makes the success of science a miracle. . . . and the modern positivist has to leave it without explanation (the realist charges) that 'electron calculi' and 'space-time calculi' and 'DNA calculi' correctly predict observable phenomena if, in reality, there are no electrons. . . . If there are such things, then a natural explanation of the success of these theories is that they are partially true accounts of how they behave. And a natural account of the way in which scientific theories succeed each other . . . is that a partially correct/partially incorrect account of a theoretical object . . . is replaced by a better account of the same object or objects.[54]

Following Boyd, and further demarcating the (CER) view that Laudan is attacking, he claims that realism, as an empirical, explanatory hypothesis concerning the success of some scientific theories, has two main principles.

1. Terms in a mature science typically refer.
2. The laws of a theory belonging to a mature science are typically approximately true.[55]

Given Putnam's succinct account of practically all of the theses of (CER), he can be taken as one of the main targets for Laudan's criticisms.

As far as the referential status of terms in a mature theory is concerned, it is the conjunction of this with the claim that realism 'explains' success, that Laudan attacks. He distinguishes four claims necessary to make this conjunction work, at least two of which he finds objectionable; that a theory whose central terms refer will be successful, and that a successful theory's central terms will refer.[56] First, does the success of scientific theories depend in any way on their central terms being referential? Laudan claims that this is not generally the case, on any reasonable account of 'refer', or 'successful'. There is, for example, no aether, so theories that postulated an aether as a central theoretical term could not have achieved referential success for this term on any reasonable current theory of denotation or reference.[57] Still, aether theories comprise some of the more successful theories of the 19th century (e.g., Maxwell's theory). Similarly, there is no phlogiston, no electrical fluid, no caloric, etc., and yet each of these were central terms in highly successful theories (relative, of course, to certain stages in the development of the relevant sciences). Nor does it help to adopt a causal view of denotation (CT), both for reasons presented at the end of chapter II, and because it is not clear that 'aether', 'phlogiston', or 'caloric' pick out <u>anything</u> (i.e., the problem is not just that what the theories said about these alleged entities was not quite right). It seems then, that either such theories were not really successful (a disastrous move historically) or their success did <u>not</u> depend on the reference of their central terms.[58]

What of the claim that theories whose central terms refer are (thereby) successful? Laudan claims that this too is not generally the case. Most of us, I think, would want to claim that the term 'atom', for example, refers. Still, there are many stages in the development of the atomic theory when the theories using the term 'atom' were not the most successful account of the relevant phenomena at the time. To note just a few examples, the ancient atomic theories were by and large clearly inferior to Aristotle's account. Dalton's theory was not clearly the most successful chemical theory through <u>all</u> of the 19th century, and even Cannizzaro's amended atomic theory and the kinetic theory of gases were not, at all stages, clearly more successful than phenomenological thermodynamics. Similarly, assuming that the key terms of wave theories of light had reference (although on some readings of 'photons', I suppose, corpuscular theories could also be said to have referred), they still remained inferior to Newton's corpuscular theory until at least the 1820's.[59] In any event, there seem to be important historical cases of theories whose central concepts we would now (probably) claim referred, that were nonetheless not thereby more successful theories than their opponents whose central terms did not refer.

If reference and success are _not_ always connected, what becomes of the realist claim that the success of a theory is best explained by its (at least 'approximate') truth? This claim, if anything, is the _central_ tenet of (CER) positions. Furthermore, reference and truth _are_ connected for most realists, and, consequently, even non-(CER) accounts of realism would also seem to be in jeopardy if the above objections hold. As Laudan claims:

> It might be said (and Putnam does say this much) that we can explain why a theory is successful by assuming that the theory is true or approximately true. Since a theory can only be true or nearly true (in any sense of those terms open to the realist) if its terms genuinely refer, it might be argued that reference gets into the act willy-nilly when we explain a theory's success in terms of its truth (like) status.[60]

Rather than helping the realist, however, this linking of reference and truth leads, for Laudan, to even greater conceptual historical muddles. First, and perhaps least important, there do not seem to be any semantically or epistemically adequate formulations of 'approximate truth'.[61] Intuitively, if not formally, there may be some hope for establishing such a notion for quantitative theories, where 'approximate truth' might well be unpacked in terms of measurable parameters (for example, _perhaps_ Newton's theory makes approximately true predictions for medium sized objects moving slowly with respect to the speed of light). It is not at all clear, however, what kind of notion of 'approximate truth' will handle cases like electrical fluid, caloric, or phlogiston. It does seem to be an embarrassment for (CER) accounts that the crucial concept of approximate truth has remained an intuitive, promissory note.[62] Still, this objection need not be considered devastating, since some philosophers hope to formulate more adequate accounts of this concept.

The second sort of objection to 'approximate truth', however, _is_ devastating. The connection between notions of 'truth' and 'reference' is, again, the main source of the problem. Laudan certainly seems to be correct when he claims that, _however_ 'approximate truth' may come to be understood, "a realist would never want to say that a theory was approximately true if its central theoretical terms failed to refer."[63] Given this, examples of successful, non-referring theories seem to hamstring (CER) accounts. As Laudan claims, there seems to be "a plethora of theories which were both successful and non-referential" in the history of science.[64] Besides the examples already listed, crystalline sphere theories in astronomy, humoral theories in medicine, catastrophist geological theories, vital force theories of physiology, and theories of circular inertia could also be mentioned, and these would by no means exhaust the list.[65] Such theories 'had their day' in terms of being successful (on any reasonable account of scientific success), and yet had central theoretical terms that failed to refer (on any reasonable account of reference). Consequently, they could not be considered 'truthlike', if this is interpreted as being either true, or approximately true (both being tied to notions of reference). Generally,

> In the absence of an argument that greater correspondence at the level of unobservable claims is more likely than not to reveal

> itself in greater accuracy at the experimental level, one is obliged to say that the realists' hunch that increasing deep-structure fidelity must manifest itself pragmatically in the form of heightened experimental accuracy has yet to be made cogent.[66]

Laudan, then, argues (successfully, I think) that the success of a theory does not entail either the referential status of the theory's central theoretical terms, or the theory's 'truthlikeness', and vice versa. Hence, (CER) forms of realism not only fail to explain the success of (some) scientific theories, they are not even cogent.

What about the form of realism that I have been developing? Some of Laudan's objections to (CER) accounts are tangental to diachronic realism. It is not important to this form of realism, for example, that all successful theories be referential, true, or truthlike, as long as the level of virtues achieved by non-truthlike theories does not rival that achieved by theories I would claim we should believe. Nor do I deny the rationality or (relative) success of phlogiston theory, for example, at particular stages in the development of chemistry. I do deny that the level of virtues achieved by this theory is comparable to that (eventually) achieved by oxidation theory, which I would also argue we should believe. Oxidation theory, in other words, became modest, general, and determinate enough to warrant rational belief, though it did not always warrant this belief. Similar claims could (and will) be given for atomic theory, etc. Although my intuitive defense of diachronic realism via these three virtues does not allow for a completely clear demarcation of truthlike and non-truthlike theories, I will argue that it can establish this demarcation for some important cases, and may well prove useful for others. Only part of this promissory note will be made good in this work, but that, alone, does not present a conceptual or historical problem.

I have, however, in the previous chapter, promised to defend the intuition that certain levels of success achieved by some theories are best explained by a (diachronic) realist account. Laudan has provided several reasons to suspect _any_ realist explanation of scientific success, and he succeeds in debunking (CER) versions of this. Part of the problem with (CER) accounts is that they are interpreted synchronically. This problem, at least, is not shared by my version, but other problems listed by Laudan concern the very notions of 'approximate truth' and 'reference' that seem to be the cornerstones of _any_ realist explanation of the success of scientific theories. Consequently, I must show that the notion of 'approximate truth' needs to be replaced by another notion in view of Laudan's objections, _and_ that this replacement is not _ad hoc_ (i.e., not _merely_ engendered by Laudan's objections). Luckily, I think that the reformulations I am about to suggest for realist explanations of scientific success follow naturally enough from diachronic realism as I've presented it, so that they are not merely _ad hoc_. Furthermore, I think that they provide the most promising approach to supporting the central realist claim that the remarkable success of at least some scientific theories is best accounted for by their 'truthlikeness' (a notion which, not surprisingly, will also need to be reformulated). These arguments will therefore conclude my conceptual defense of diachronic realism, and the following section will attempt to show that it can be easily applied to two important historical cases.

First, a perspective is necessary for evaluating past theories in terms of whether or not they achieved a sufficient level of the three virtues to warrant rational belief. This perspective must tread a fine line between two unacceptable extremes. On the one hand, one must be careful about 'going native', and trying to evaluate the virtues of theories completely from the perspective dominant at the time. Trying to go native (besides being difficult, if not impossible) can quickly lead to the epistemic quandary of alternative or competing conceptual schemes. Realism, after all, is committed to some theories being well enough confirmed to warrant belief in these theories' truth. While what would warrant rational belief may change historically (though many of the examples I've already used should make even this assertion questionable), truth, of course, does not. Aristotle, for example, was undoubtedly justified, given the evidence available to him, in asserting the truth of many of his cosmological beliefs. Rationality notwithstanding, however, the earth is not stationary at the center of the universe, and such assertions must now be considered to be false. Like historical knowledge claims, considerations pertaining to historical realism must be decided from our perspective. If this represents a degree of cultural chauvinism, it is also a necessary bulwark against serious epistemic mischief.

On the other hand, pushing this chauvinism too far (if it is chauvinism), would result in a shamelessly 'whiggish' interpretation of historical cases (i.e., theories are 'good', 'rational', 'scientific', 'successful', etc. only in so far as they foreshadowed our clearly 'good', 'rational', 'successful', 'scientific' theories). As I stated earlier, if philosophy of science is to be about science, (completely) reinterpreting historical cases is as damaging in the content direction as going native is in the conceptual direction. In short, the chauvinism necessary for epistemic reasons needs to be 'benign', in the sense that the historical data, if not our final epistemic judgments concerning this data, need to be as historically accurate as possible. We do not want to rewrite history, but we reserve the right to reevaluate the strength of the evidence then available for a theory in light of its subsequent development. Whether scientists were then warranted in believing the theory is a different question from whether we should believe it given current evidence. Benign chauvinism then, is the necessary perspective to take in interpreting the success of historical theories. If we are to try to keep both our epistemic and our historical consciences unblemished, then it follows (as I've already argued) that nonmethodological considerations that may have played a role in historical cases of theory choice must be neither ignored on the one hand, nor overemphasized on the other. In short, my position that methodological considerations play the predominant role in such cases of theory choice needs to be supported with historical, as well as conceptual evidence. Furthermore, if our chauvinism is to remain benign, then as far as possible, this historical evidence must be gleaned from the cases themselves, rather than imported after the fact. I've already attempted to partially justify my claim for the predominance of methodological factors in the last chapter by showing the agreement in a variety of cases over which virtues were important. I will further defend this claim in the following two sections by considering two historical cases in some detail.

If benign chauvinism is the best epistemic attitude to adopt towards the success of historical scientific theories, a perspective is also needed to account for this success by means of diachronic realism without falling into the problems Laudan has established for (CER) views. Specifically, while it is a general realist intuition that the best explanation for the success of at least some theories is that they were somehow 'truthlike', none of the more common ways to unpack this 'truthlikeness' seem to work. It seems, for example, that no notion of 'truth' open to the realist can do without the central terms in a theory referring. Yet, as Laudan has argued, many past successful theories fail to meet this criterion. Apparently, then, no available notion of 'truth' (or even 'approximate truth') can explain their success. If this common realist desideratum is to be realized, then, some new account of 'truthlikeness' will be necessary.

Such a new perspective is available, I think, and, in fact, more or less follows from the account of diachronic realism I've developed. Remember, for example, that it is not a commitment of diachronic realism that all successful theories are true, or even that all successful theories warrant rational belief. Rather, some theories have achieved a sufficient level of virtues to warrant such belief, and cases of theory choice most often favor the theory that has achieved the highest level of these virtues, whether or not this level is sufficient, from the perspective of benign chauvinism, to warrant belief. What diachronic realism needs to explain is how some past scientific theories have achieved rather high levels of these virtues while not being true. In the early stages of a theory's development, of course, being successful does not require a very high level of virtues, so that even though the 'better' theory at the time was better <u>because</u> of these virtues, the most appropriate epistemic attitude towards it would be something less than belief (for example, <u>acceptance</u> as empirically adequate, or as a fruitful model, etc.). Still, when a theory is as successful as Newton's or as Bohr's model of the atom, or even as Franklin's theory of electricity, it becomes necessary to explain this higher level of theoretical virtues. After all, if a theory can achieve a high level of these virtues without being true or approximately true, realist arguments based on these virtues seem bankrupt (why should our confidence be high concerning 20th-century atomism, if past highly successful theories were false?).

From the perspective of diachronic realism, an account of the high level of virtues attained by some past theories should be developed along the following lines. First, a sufficient level of virtues should warrant belief in a particular theory at a given time with respect to the available evidence, competitors, etc. Hence, like past knowledge claims, a diachronic realist should be prepared to assign the status of 'warranting belief at the time' to some (not all) past theories, even though current evidence leads us to no longer assent to them. Given benign chauvinism, such a decision on our part is independent of whether these theories were actually believed at the time. This can be made clearer if we leave aside belief temporarily, and restrict our attention to a theory's being 'better' at the time. From the perspective of diachronic realism, this judgment of a theory's being better is also decided by means of the virtues. Consequently, as I've argued, Copernicus' theory was more modest, more general, and more determinate than Ptolemy's at the time it was proposed, and was, therefore, an objectively better theory. This judgment is not affected by the fact that most contemporaries did not share this judgment and had legitimate reasons for not believing that the earth moves. Constitutive components are not the only components involved in theory choice, but if my arguments in the last chapter are sound, they are the most important epistemic components.

More importantly, diachronic realism should try to account for the high level of virtues attained by some past theories in terms of their 'truthlikeness'. This need not mean, however, that these theories were true, or approximately true, or that their central theoretical terms successfully referred. Rather, their truthlikeness can be established by reference to current theories, that we have reason to believe are true. A past theory with a high level of virtues can be said to be truthlike if the reason it attained such a high level of virtues (regardless of the referential status or approximate truth of its central terms) is because it 'tapped into' central components of later, more accurate theories whose central terms do have referential status, and are true or approximately true. 'Tapped into' is, of course, a vague notion. This is partially because there are a number of different ways in which a former theory can achieve truthlikeness with respect to later theories. Sometimes they can be said to be truthlike, for example, because their theoretical accounts, while not true, can be easily amended to agree with later accounts (i.e., they are readily 'translatable' into true or approximately true theoretical accounts). Sometimes we can claim that a past theory achieved truthlikeness because, though not translatable into true theoretical accounts, it succeeded in connecting phenomena that are connected. It is largely because of the connection it made between various forms of combustion, for example, that phlogiston theory was as successful as it was. Sometimes a past theory can be said to have achieved truthlikeness because it directly led to true accounts, not merely in the sense that later theories developed from it, but in the sense that its theoretical account was struggling with concepts that later were reformulated by true accounts. While there are no electrical fluids, for example, Franklin's strange, inconsistent treatment of them transcended the mechanical models predominant in the 18th century, and directly foreshadowed the notion of an electrical field. In all of these cases, it was because the past theories got something right (or nearly right) that they were as successful as they were. When this success reaches a certain high level, the theory can be said to have been on the right track. I am proposing, in other words, that the 'truthlikeness' of at least some successful theories is not so much due to their particular theoretical accounts, as it is due to the research tradition began or continued by them being directly related to a later theory that can be claimed to have sufficient virtues to warrant rational belief. If this account is to prove promising, it at least needs to be shown that such a perspective enables us to separate some theories that were on the right track, from other (successful) theories that were not, with respect to the virtues I've delineated.

Thus I want to argue that the remarkable success of early 20th-century atomism gives us reason to believe that there are atoms. While the quantum theory renders all of the early models of atoms wrong, this does not affect (what I take to be) the fact that there are submicroscopic entities that _do_ combine in certain ways with other submicroscopic entities to account for the chemical phenomena they were originally postulated to explain. Whether or not their construction consists of particles or probabilistic wave packets, does not affect the evidence for the _existence_ of a relatively stable unit that is the smallest amount of a substance that combines with units of other substances. If this is right, then Dalton's atomic theory was on the right track, not because atoms are very much like he claimed, but because the tradition of chemical atomism directly resulted in the highly successful atomic theories of the early 20th century. Put another way, it is _because_ (and in so far as) Dalton's theory shares important aspects of these more adequate theories that it successfully accounted for the law of multiple proportions by weight, the law of constant composition for chemical compounds, the law of the conservation of mass, etc. Similarly, it is because static electricity actually behaves in a manner closer to Franklin's postulation than to Nollet's that Franklin's theory gave such a better account of the Leyden Jar, etc. As was stated earlier, one of the major problems in spelling out (CER) accounts is _misplacing_ the 'truthlikeness' of past successful theories. The realist intuition that being related to truth explains (certain levels of) success, can be rescued from Laudan's attacks, I maintain, by shifting the 'bearers' of truthlikeness to theories that _can_ be argued to be true (or, on some yet to be specified account of the term, 'approximately true') via the concepts of 'benign chauvinism' and 'being on the right track'.

An enterprise such as this would, of course, be historically doomed if success were categorically connected to being on the right track, or theory choice was confined to constitutive components like the virtues I've listed. As for the latter, I've consistently maintained that diachronic realism is not committed to constitutive components being the sole factors involved in theory choice, but only to their playing a dominant epistemic role. The historical cases I've already presented and will subsequently present should at least partially justify this claim. The former claim has also been repeatedly denied by my account. Theories are successful, I've argued, in so far as they satisfy scientific virtues, but not all levels of success via virtues warrant rational belief. Hence, phlogiston theory can be successful without being on the right track, and Franklin's theory can be on the right track without warranting belief; and being successful, being on the right track, and warranting rational belief are all decided by relative levels of the virtues I've delineated. That is, being modest, general, and determinate is what makes a theory successful and, given my arguments in the previous chapter, a theory is not likely to achieve all of these virtues unless it is at least partially right about something. Hence, achieving the virtues is best explained by a theory's being truthlike (right about something), and having a high level of the virtues warrants rational belief in the theory, or at least the theory's being on the right track. Furthermore, I will maintain that such an account is not ad hoc (tacked onto diachronic realism in order to save it from Laudan's objections), but that it is the type of account that one would expect given my earlier arguments. Warranted belief is not the correct epistemic attitude toward all theories at all times, but the most important epistemic considerations (belief, acceptance, or whatever) are all based on the scientific virtues. Benign chauvinism and being on the right track reflect the notions that I've argued for throughout this work.

Given the view I've maintained, one should not expect that levels of virtues can at this stage be quantified, formalized, or totally demarcated. The entire approach to diachronic realism that I've been maintaining remains at the intuitive level, on the assumption (again) that general convergence of intuitions must precede more detailed analysis. Still, as I noted above, for the present account to be shown to be even on the right track, means of at least roughly distinguishing levels of virtues must be provided. At this point, a brief account using historical examples must suffice. Consider, for example, three cases that I believe intuitively represent three different levels of virtues: phlogiston theory, which was successful but not on the right track; Franklin's electrical theory, which was on the right track but did not achieve a sufficient level of virtues to warrant belief; and early 20th-century atomism, which achieved a sufficient level of virtues to warrant rational belief.

What does it mean to claim that phlogiston theory was 'successful', given that it was neither on the right track nor is there such a thing as phlogiston? William Whewell accounted for the success and importance of the phlogiston theory in the following way.

> The phlogiston theory was deposed and succeeded by the theory of oxygen. But this circumstance must not lead us to overlook the really sound and permanent part of the opinions which the founders of the phlogiston theory taught. They brought together, as processes of the same kind, a number of changes which at first appeared

> to have nothing in common; as acidification, combustion, respiration. Now this classification is true, and its importance remains undiminished, whatever are the explanations which we adopt of the processes themselves. . . .[67]

In other words, the phlogiston theory was sufficiently general to be the most successful theory of calcination and combustion yet proposed. It linked diverse phenomena that have few obvious connections and did so in a relatively modest manner. Furthermore, while the theory was <u>wrong</u>, the phenomena it connected really are connected, and the successor theory continued to connect them. Still, the basic postulate of something being lost during combustion and calcination was wrong enough to prevent this theory from developing much further. The problems involved in trying to rescue the theory by means of negative weight or identifying phlogiston with hydrogen, for example, while not inherently irrational or unscientific, <u>became</u> sufficient to render the theory non-progressive (although, labeling such moves as <u>ad hoc at the time</u>, would be an example of unbenign chauvinism). Hence, in spite of its initial admirable success (in terms of the virtues), phlogiston theory was not on the right track, since it ultimately cannot be translated into later theories that we are warranted in believing, nor did it directly lead to true theoretical accounts. Unlike Franklin's theory, phlogiston theory was not struggling with new concepts that were later to be reformulated by better accounts, but instead remained committed to a (nonexistent) substance being <u>released</u> by combustion. Lavoisier's oxygen theory, on the other hand, while <u>at the time</u> not obviously superior, did directly lead to truer accounts (again, despite the non-existence of caloric, etc.) and so <u>was</u> on the right track.

Franklin's theory of electricity was also successful in the same sense as phlogiston theory (it was more general, for example, than its competitors). Unlike phlogiston theory, however, it was also on the right track because some of its crucial concepts did directly lead to current views (as will be shown in the next section). Still, even at the time, it did not warrant rational belief in electrical fluid, and especially in electrical atmospheres, because of the serious anomaly of accounting for the mutual repulsion of two negatively charged bodies.[68] This is not to claim that a theory cannot be rationally believed just because there are some recalcitrant phenomena, since this may well be true of any theory. Rather, in this case, the phenomena in question had been previously explained, and, even though the Leyden Jar phenomena transcended this data, both in generality and in eventual importance, it remained important phenomena for electrical theory. Since it had been explained, and was still considered to be important, its recalcitrance bothered Franklin,[69] and from our perspective, it should have. Hence, while successful and on the right track, Franklin's theory encountered sufficient difficulties to render its level of virtues insufficient to warrant belief.

Finally, by about 1911, most scientists were convinced of the existence of atoms. The doubts of the 1830's and 40's due primarily to still insufficient chemical formulae and the failure to distinguish between atoms and molecules, were largely nullified by the work of Cannizzarro and Mendeleev as well as by the initial success of the kinetic theory of gases. The remaining doubts were handled in the early 20th century by the recognition that atoms had internal structures, and experimentally by the work of Jean Perrin.[70] Regardless of the problems later confronting the early models of the structured atom at the sub-atomic level, I do not believe that these problems create serious reasons for doubting the existence of atoms, however structured. Unlike both of the earlier examples, the level of virtues achieved by the atomic theory at this time rendered it not only successful and on the right track, but also sufficient to warrant rational belief. Consequently, the analysis via virtues that I have been developing is capable of demarcating different epistemic attitudes appropriate towards some important historical theories, in spite of the fact that this demarcation is still intuitive. Phlogiston theory was successful because of its level of virtues (and truthlikeness), but not on the right track; Franklin's theory was on the right track but did not warrant rational belief; and early 20th-century atomism warranted rational belief in atoms.

If the above account seems intuitively sound, then diachronic realism can account for the realist intuition that the 'truthlikeness' of some theories explains their level of success without falling prey to Laudan's objections to (CER) accounts. This account can be clarified a bit more by comparing my treatment of 'truthlikeness' with a recent defense of (CER) views against Laudan's attack.[71] I argued above that one of the problems Laudan finds with the notion of 'approximate truth' is that it is too imprecise, lacking both a semantics and an epistemology. At the time I argued that this was a problem, but also claimed that it was not devastating, given the possibility that some adequate treatment of approximate truth may be forthcoming. Hardin and Rosenberg also claim that from Laudan's attack on the lack of precision of 'approximate truth', it does not follow that (CER) intuitions are not correct.

> This conclusion does not follow; it is like saying the assertion that we lack, or, for a long time, did lack an adequate analysis of the central terms in the claim that there are other minds, implies that the claim that there are other minds was or is unintelligible. The _non sequitur_ does not minimize the importance of the problems in expounding realism that Laudan puts before us, but it does put them in perspective. Like the thesis of other minds, scientific realism is intuitively plausible and widely embraced, but is very difficult to state perspicuously.[72]

So far, then, my account is in agreement with Hardin's and Rosenberg's. They begin to differ when they each consider Laudan's insistence that the realist must associate truth and approximate truth with the reference of the central terms in a theory.

I, of course, agreed with Laudan, and saved the 'truthlikeness' of some past theories by shifting considerations of truthlikeness to theories that _do_ warrant rational belief (benign chauvinism and being on the right track). Hardin and Rosenberg, instead, deny that reference is necessary for truth or at least, approximate truth. There need not, for example, be anything like a gene for Mendel's theory to be regarded as 'approximately true'.

> Mendelian genetics is still represented as an approximately true theory, even though its central theoretical terms can, on this account, plausibly be said not to refer. The causal role Mendel accorded to genes is parceled out to other entities. In brief, this is done by showing that the diverse units of genetic functions--of mutation, of replication, of expression--work together often enough to give a false impression of unity and to yield an approximately true set of predictions about the distribution and transmission of paradigmatically heritable properties, which Mendel mistakenly took to be phenotypes. The units of function, the 'muton', 'recon', 'cistron' do not add up to the gene.[73]

I would agree with all of this _except_ with attributing approximate truth to Mendel's theory. Mendel's theory was, on my account, on the right track, and therefore truth_like_. Still, I don't think it follows, or is even plausible, that his admittedly non-referring theory was approximately true. An "approximately true set of predictions," after all, does not constitute the approximate truth of a theory.

I think this case is particularly striking because Hardin and Rosenberg, in another passage, come very close to interpreting 'approximate truth' as 'being on the right track'.

> Mendel's 1866 theory, embodying its laws of segregation and assortment, clearly constitutes the first in a sequence of successive theories which are held by life scientists to constitute a series converging on the truth. Mendel's theory is often credited with approximate truth and still taught because of its simplicity, and the ease with which it can be complicated in the direction of presumably more accurate and more complete genetic theories, theories more nearly approximate to the truth. Yet it may plausibly be reported by a realist that there is nothing like genes, or phenotypes, as Mendel or his immediate successors construed them.[74]

I would again hardly agree with this line of reasoning except the part attributing approximate truth to Mendel's theory. Newer theories, if sufficiently virtuous to warrant rational belief, may be construed as true or approximately true, but this, I think, entails that their central terms do refer (or at least partially (?) refer).[75] In other words, like Laudan and most philosophers working on semantics, I balk at calling anything true or approximately true whose key terms aren't referential. 'Truthlikeness', on the other hand, in the sense of being right about something, or eventually converging in a theory that is true, is quite different in that it (on my reading) shifts truth bearers without giving up the explanatory role truth is held to play concerning some successful theories. Consequently, I share many of Hardin's and Rosenberg's intuitions, but feel that a new perspective is necessary to specify what is 'truthlike' in earlier, highly successful but false, theories. I claim that a promising way to begin constructing a new perspective is in terms of diachronic realism, benign chauvinism, and being on the right track. Virtually _any_ level of success with respect to the scientific virtues I've delineated requires that the theory in question be right about _something_. When the level of success becomes high the theory can be said to be on the right track. Truthlikeness, then, explains the success of past theories, and some theories become successful enough to eventually converge in a theory that is true or approximately true. These latter can be said to be on the right track. Still, since their central terms do not refer, or their theoretical accounts are not correct, they should not themselves be claimed to be true or even approximately true.

So far in this chapter I have attempted to defend diachronic realism against some important conceptual-historical objections. I have argued that some of these objections do not affect diachronic realism (even if they hold for other realist accounts), and that others can be answered from my perspective. If I have been successful in this, I would now claim that diachronic realism is neither logically nor historically bankrupt (it is neither historically _implausible_, nor refutable without consulting the historical record). I have also attempted to cash a promissory note I issued in the last chapter concerning diachronic realism's ability to account for the realist intuition that theoretical success is best explained by a theory's relation to truth. To accomplish this, I have tried to answer Laudan's objections to (CER) views by introducing the notions of 'benign chauvinism' and 'being on the right track' to account for a theory's 'truthlikeness' even though it cannot be said to be true or approximately true. It is now time to consider whether such a position can provide a satisfactory account of some important historical cases.

Historical Cases: Atomic Theory

It is often remarked that Dalton's atomic theory, following upon Lavoisier's work, helped to establish chemistry as a quantitative physical science. As with most such generalizations, there is both an element of truth, and an oversimplification contained in this assertion. Although it is often claimed, for example, that Lavoisier's 'chemical revolution' was a victory of quantitative chemistry over chemical 'philosophy' (measurements, as opposed to vague chemical 'principles', etc.), the background of quantitative chemistry extends *at least* back to the pneumatic chemists (including the phlogiston theorists) that Lavoisier supplanted. As was argued earlier, Priestley's work, for example, was also concerned with measurements and experimental verification, to the extent that *at the time* it would not be appropriate to consider Lavoisier's experiment on the red calx of mercury 'crucial'. Furthermore, many of the techniques and apparatus' used to isolate 'factitious' gases were in existence prior to Lavoisier's work, and, consequently, gases with distinct properties had already been isolated. What *is* true in the above claim is that, in Dalton's theory, a particularly fruitful perspective was introduced that did rapidly lead to increased quantification and accuracy.

For one thing, though the mechanical corpuscular philosophy had been prevalent since the 17th century 'scientific revolution', a *chemically* fruitful conception of these corpuscles had not been established by the beginnings of the 19th century.[76] Consider, for example, Newton's claim concerning the existence of particles in "Query 31" of his Optics.

> It seems probable to me, that God, in the Beginning form'd Matter in solid, massy, hard, impenetrable, moveable Particles, of such Sizes and Figures, and with such other Properties and in such Proportion to Space, as most conduced to the End for which He'd form'd them; and that these primitive Particles being Solids, are incomparably harder than any porous Bodies compounded of them; . . . And therefore, that Nature may be lasting, the Changes of corporeal Things are to be placed only in the various Separations and new Associations and Motions of these permanent Particles; compound Bodies being apt to break, not in the midst of solid Particles, but where those Particles laid together, and only touch in a few Points.[77]

What was lacking in this account and in its immediate successors as far as a chemist was concerned, was a way to tie in such physical explanations to actual chemical experience. Neither chemistry nor physics were yet advanced enough to establish such a union.[78] What is the nature of this union?

Dalton, like many other innovators in the history of science, came to chemistry with principles gleaned from work in another discipline, in his case, meteorology. He had long been concerned with the composition of the atmosphere, especially with respect to the nature of the combination of the four main gases which constituted it. The proportion of these gases in the atmosphere was more or less constant, whether the sample was taken from mountain tops, valleys, etc. Why did the four gases, whose relative weights by volume were different, not instead form layers, with the heaviest on the bottom and the lightest on top? The fact that they did not could be explained, according to Dalton, by assuming that each gas was composed of indivisible particles that were neither attracted nor repulsed by particles of the other gases. If this were the case, these unlike particles could be thoroughly mixed in the atmosphere, so that each gas could retain its own density (whether 'mixed' or not). Consequently, since the densities of the various gases remained independent of their mixture, adding up to the total density of atmospheric air, the different densities of the various gases would not result in the layers that might otherwise be expected. Hence, Dalton proposed the 'law of partual pressures' in 1801, which provided the conceptual background for his atomic theory.[79]

At the same time, an eight year controversy was brewing between Berthollet and Proust over whether the proportion by weight of combining chemical substances was constant, or whether the composition of a given compound could vary.[80] Berthollet favored the hypothesis of varying composition, and supported this by results he had gained by studying various chemical solutions, alloys, glasses, and metallic oxides, as well as numerous (incorrect) analyses reported by others.[81] Especially since some of the metals he studied formed several oxides (e.g., lead forms oxides that range in color from gray through shades of yellow, to red), he felt that "the change in composition was continuous rather than intermittent."[82] Proust, on the other hand, believed that the composition of chemical compounds was constant, and between 1802-1808 exchanged amiable correspondence with Berthollet in the *Journal de Physique*. Given his deft experiments conducted in his excellent lab in Madrid, Proust was gradually able to show that Berthollet's results were sometimes due to confusing mixtures with actual chemical compounds and otherwise due to faulty apparatus and incomplete analyses. He showed, for example, that there were two oxides of tin, each with a fixed proportion of tin and oxygen. Berthollet's results favoring intermediate percentages were again, based on his analysis of impure mixtures.[83] Eventually, due primarily to Proust's work, the 'law of constant composition' was accepted by most chemists. As is often the case, a measure of serendipity entered into this exchange, since at this level of the development of quantitative chemistry, had they analyzed different compounds (berthollides) the results would have supported Berthollet.[84]

What Proust's results did was to help establish generality for Dalton's fledgling chemical atomic theory. At this point, the hypothesis of chemical change being due to the 'reshuffling' of indivisible atomic particles provided a modest account of Dalton's own law of partial pressures, Lavoissier's law of the conservation of mass (not discovered, but employed by Lavoisier), and the newly established constant composition by weight of chemical compounds. Furthermore, unlike the earlier corpuscular philosophy, and especially unlike ancient speculative atomism, there was reason to hope for determinateness regarding Dalton's theory, since <u>weight</u> had been singled out in all of the above laws as a 'crucial quantity'. Consequently, when Dalton shifted his attention to chemical composition in general, his fledgling theory was already modest, general, and (potentially) determinate enough to warrant some confidence in its eventual success (not, of course, virtuous enough to yet warrant <u>belief</u>).

Given this brief background, Dalton's 'discovery' of the law of multiple proportions involved the mixture of experimental fact and theoretical insight that we have come to expect in important episodes in the history of science. He already was in possession of a promising (virtuous) theory,[85] so that his experimental results, clumsy as they were,[86] were already organized, and consequently 'fit' into his theoretical expectations. As was claimed in the last section, the numbers did not (and never do) speak for themselves. Still, the combination of data covered renders 'changes in world view' interpretations too extreme to adequately interpret the historical data. While someone without the organizational power of the appropriate theory would not necessarily see such a fit, the confidence that Dalton and others had at this point in his theory was still more methodological (via the virtues) than metaphysical. The absence of single, synchronic, crucial experiments does not, as shown above, jeopardize the hope for the primacy of constitutive components in theory choice.

Part of Dalton's experimental work that led to the law of multiple proportions (sometimes 'multiple ratios') was stated as follows.

> If 100 measures of common air be put to 36 of pure nitrous gas . . . , after a few minutes the whole will be reduced to 79 or 80 measures, and exhibit no signs of either oxygenous or nitrous gas. If 100 measures of common air be admitted to 72 of nitrous gas . . . , there will, as before, be found 79 or 80 measures of pure azotic gas (nitrogen) for a residium. If, in the last experiment, <u>less</u> than 72 measures of nitrous gas be used, there will be a residium containing oxygenous gas; if more, then some residiary nitrous gas will be found. These facts clearly point out the theory of the process: the elements of oxygen may combine with a certain proportion of nitrous gas, or with twice that portion, but with no intermediate quantity. In the former case <u>nitric</u> acid is the result, in the latter <u>nitrous</u> acid: but as both these may be formed at the same time, one part of the oxygen going to <u>one</u> of nitrous gas, and another to <u>two</u>, the quantity of nitrous gas absorbed should be variable; from 36 to 72%, for common air.[87]

Similarly, he found that in carburretted hydrogen obtained from stagnant water, 4.3 measures of carbon combined with 2 of hydrogen, while in olefiant gas (ethylene), 4.3 measures of carbon combined with 1 of hydrogen.[88] As Freund points out, "Obviously, the numbers expected by theory, and not the experimental results, are given."[89] The law of multiple proportions was further publicized in 1807 by its inclusion, along with Dalton's atomic theory, in Thomas Thomson's System of Chemistry.[90] Together with the three regularities already mentioned, this law increased the generality and experimental power of Dalton's theory, and became fairly widely disseminated. All of this took place at a time when most chemists were increasingly concentrating on the quantitative aspects of chemical phenomena. Hence, the initial success (for methodological reasons) of Dalton's theory, at least as a promissory note.

Dalton's original atomic theory, presented in his New System of Chemical Philosophy (1808-1810), contained the following postulates. Chemical elements were made up of small, indivisible atoms, which were alike in terms of their mass and properties for the same element, but different from those of other elements. Chemical combinations between two or more elements are formed by the atoms of these elements joining into a firm union with one another in definite proportions by weight. These 'combinations' of atoms were also called 'atoms' by Dalton, leading to fifty years of confusion within atomic theory.[91] In order to determine the relative weights of constituent atoms in a compound atom, since these clearly could not be directly measured, Dalton reasoned as follows.[92] Suppose, for example, the chemical compound in question is a binary one, composed of elements A and B, whose atoms weigh a and b respectively. If one individual (compound) atom of AB is formed by the combination of m atoms of A with n atoms of B, then the actual weights of the two elements combining with one another in one compound atom will be ma of A and nb of B. The crucial quantities to be determined then, are the ratios a:b and m:n. Since the ratios of quantities which form one atom of the compound will be the same as those ratios which form any number of compound atoms, they can be determined by the experimentally ascertained ratio in which A and B are present in AB (call this experimental ratio 'p:q'). It follows then that the actual relative weights of the constituent atoms of the compound can be expressed by the equation 'ma:nb=p:q'.

Unfortunately, since m:n and a:b are unknown, this equation cannot be solved as it stands. One can measure the relative proportion by weight of a large number of atoms of A combining with a large number of atoms of B to give the compound AB. This, however, will not provide the actual relative weights of the constituent atoms without some value for the number of atoms of each chemical element that enter into the combination. Analogously, if one wished to determine the relative weights of individual uniform concrete and asphalt tiles contained in a stretch of road, it is not sufficient to determine the <u>total</u> proportion of the weight of each. Without knowing how many tiles are asphalt and how many are concrete, the relative weights of the individual tiles are indeterminate.

To return to the chemical problem if one had values for the number of individual atoms of each element combining (represented by accurate chemical formulae), and a value for the <u>total</u> proportion by weight (given experimentally), one could then determine the relative weight of individual atoms of the two elements. Or, if one had values for the relative weights of individual atoms of the two elements, and a value for their total proportion by weight, one could determine the accurate chemical formula for the compound. Since one has neither the weight of individual atoms, nor the number of atoms of each element combining, one cannot proceed to make the atomic hypothesis more determinate without some added assumptions.[93] Since the determinateness of these quantities was of prime importance for the atomic hypothesis, this became the fundamental problem confronting both proponents and opponents of 19th century atomism.[94] As will be shown, this agreement concerning the importance of the determinateness of these quantities within atomic theory by both atomists and anti-atomists, is yet another reason to regard constitutive components as primary in cases of theory choice, and to discount the 'alternate world views', non-cumulative, 'gestalt switch' claims of Kuhn, Feyerabend, and others.

Given the indeterminateness of some of the crucial quantities implied by the atomic theory and the impossibility of directly determining either the individual weights of constituent atoms or correct chemical formulae, two options were open to proponents of the theory. One option would have been to make assumptions concerning the number of atoms of each element contained in equal volumes of the gas in question. This was soon proposed by Avogadro and Ampère, but, as we shall see, was rejected by Dalton because of some of the assumptions in his original theory. The other option was to make assumptions regarding the relevant chemical formulae for these compounds. This was the option adopted by Dalton and most other chemists until the 1860's. Dalton attempted to make the chemical formulae more determinate by using what he called the 'Rule of Greatest Simplicity' (RGS). This rule is explained by Dalton in the following manner.

> If there are two bodies, A and B, which are disposed to combine, the following is the order in which the combinations may take place, beginning with the most simple, namely:
> 1 atom of A and 1 atom of B = 1 atom of C, binary.
> 1 atom of A and 2 atoms of B = 1 atom of D, ternary.
> 2 atoms of A and 1 atom of B = 1 atom of E, ternary.
> 1 atom of A and 3 atoms of B = 1 atom of F, quaternary.
> 3 atoms of A and 1 atom of B = 1 atom of G, quaternary, etc.
> The following general rules may be adopted as guides in all our investigations respecting chemical synthesis.
> First, When only one combination of two bodies can be obtained, it must be presumed to be a binary one, unless some cause appear to the contrary.
> Second, When two combinations are observed, they must be presumed to be a binary and a ternary.
> Third, When three combinations are obtained, we may expect one to be a binary, and the other two ternary. (etc.)[95]

In other words, if only one chemical compound is known having hydrogen and oxygen as constituents, assume that the chemical formula is 'HO'[96] (so that one constituent atom of oxygen combines with one constituent atom of hydrogen to form one compound atom of water, binary, etc.), and since the total proportion by weight of the two elements is 7:1 (one of the values used) in favor of oxygen, the relative weight of single oxygen and hydrogen atoms is also 7:1. Similarly, if carbon and hydrogen form at least two compounds, assume that one is binary ('CH', olefiant gas) and the other ternary ('CH_2', carburretted hydrogen), and determine the relative weights of carbon and hydrogen atoms accordingly, etc.[97]

While such a procedure indeed results in definite relative weights of individual constituent atoms, it certainly does not render these weights determinate. As Freund claims:

> The arbitrariness of these rules is self evident. Why, if we know one compound only, should this be the binary one? Another may be discovered any day, and why should nature be so complacent, in the quite accidental sequence of discovery, as to always put us into the way of the binary compound first? Why any of these rules? No

> attempt is made to place them in connection with observed facts, and criteria are lacking for testing the validity of any one of them. Moreover they are not only arbitrary, but also insufficient and vague. What for instance is to constitute 'a cause to the contrary'?[98]

Hence, there were few methodological reasons for adopting (RGS), and the 'pseudo-determinateness' gained for the crucial quantities of the atomic theory seemed to many chemists at the time (as it does now) quite arbitrary. Before these quantities could really be made determinate, a convergence of measuring techniques would be necessary, and assumptions concerning correct chemical formulae and the relative weights of individual atoms needed to be bolstered by independent (usually using the bootstrap strategy)[99] techniques. This was to take at least several more decades of quantitative and theoretical work in chemistry, incorporating results from apparently diverse and unrelated fields of chemical study. Still, even at the time, while not being virtuous enough to warrant belief (from our perspective), Dalton's theory was quite successful in accounting for the original data he was concerned with. Furthermore, the crucial quantities postulated by his theory remained the fundamental problems in chemical theory throughout the 19th century, for both opponents and proponents of the atomic theory.

Independent studies conducted at the same time on the proportion by volume of elements entering into chemical compounds could have added further generality (and determinateness) to Dalton's atomic theory. In fact, this potential support was ambiguously interpreted by many chemists, and Dalton himself refused to accept the experimental results on which it was based. These studies were conducted primarily by Joseph Louis Gay-Lussac, and were published in the Memoires de la Societe d'Arcveil, II, (1809). Together with Humboldt, he attempted to devise an accurate means of measuring the oxygen content of the atmosphere, and then generalized this technique for other gaseous substances.[100] The technique used involved passing a spark through mixed gases in a eudiometer and measuring the resulting shrinkage of the air. Gay-Lussac found that the combining ratios by volume of hydrogen and oxygen, for example, were very close to 2:1. Similarly, experiments on other gases indicated (within the limit of then acceptable experimental error[101]) a constant proportion of combination by volumes of elemental gases entering into chemical compounds. These results could have supported Dalton's theory, both because the atomic hypothesis could have provided a modest explanation of these results, and because such a technique would have helped to determine atomic formulae (at least for gaseous substances), but Dalton never accepted them.

There are several reasons why Dalton could not bring himself to accept Gay-Lussac's results, all stemming from assumptions within his own theory. For one thing, his earliest formulation of the atomic hypothesis to account for the 'mixing' of the components of the atmosphere led him to conclude that the atoms of different substances had different diameters, each atom surrounded by a specific amount of caloric, and each being in contact with its neighbors (hence the 'mixing', much like shaking a bag of differently sized marbles).[102] Consequently, the seemingly natural assumption that equal volumes of gases contain equal numbers of atoms, which would have included Gay-Lussac's law of combining volumes within the range of data covered by atomic theory, seemed impossible to Dalton. Furthermore, how, on the assumption that atoms are indivisible (together with the failure to distinguish individual atoms of an elementary substance and stable molecules composed of these atoms), is it possible for <u>two</u> atoms of hydrogen to combine with <u>one</u> of oxygen to produce <u>two</u> compound atoms of water (as would be necessary if Gay-Lussac's ratios were interpreted atomistically)?

> It is evident the number of ultimate particles or molecules in a given weight or volume of one gas is not the same as in another; for, if equal measures of azotic and oxygenous gases were mixed, and could be instantly united chemically, they would form nearly two measures of nitrous gas, having the same weight as the two original measures; but the number of ultimate particles could at most be one half of that before the union.[103]

Using Dalton's symbolism, and Gay-Lussac's measured proportions, the combination of oxygen and hydrogen to form water would look like this: '$\odot\odot + \bigcirc \rightarrow \odot\bigcirc\ \odot\bigcirc$' ('2H+O → 2HO'); a flat contradiction if each of the symbols represents a single atom, and atoms are indivisible. Finally, and most ludicrously (given Dalton the theorist and sloppy experimenter, and Gay-Lussac the careful experimenter and reluctant theorist), he questioned Gay-Lussac's experimental results themselves.[104]

The only way that the early atomic theory could account for Gay-Lussac's results was by postulating that equal volumes of gases contain equal numbers of atoms. Given the above objections to such a view from the perspective of Dalton's theory, this option did not seem open to atomic theorists. Of course, these objections are of unequal importance. Dalton's law of partial pressures, for example, does not entail the static, 'mixed bag of marbles' view that he adopted to account for the mixing of gases in the atmosphere. Hence, one of his main reasons for rejecting equal numbers of atoms in equal volumes of gases was not only wrong, it was not independently supported. Asserting distances between individual atoms and molecules, and attractive and repulsive forces between them (instead of the purely mechanical 'pushing' analogy via envelopes of caloric), would have made the equal volumes-equal number of atoms view more acceptable from Dalton's perspective. As will be seen again later, non-mathematical, strictly mechanical perspectives often inhibited the development of more determinate theories. Similarly, Dalton's rejection of Gay-Lussac's measurements was eventually bound to backfire, given Gay-Lussac's care and Dalton's sloppiness. Hence, the increasing evidence for accepting Gay-Lussac's results hurt Dalton's theory instead of helping it, since the controversy itself made other chemists somewhat leary.

Finally, the objection that Gay-Lussac's law entailed splitting atoms, while seemingly conclusive, rested on the contemporary ambiguity in the use of the term 'atom'. If a distinction had been consistently made between the smallest unit of an element that can enter into chemical combination with other elements (atom), and the smallest stable unit of this element found in isolation (molecule), Gay-Lussac's results could easily have been covered by the atomic theory, without sacrificing the indivisibility of atoms. Such a distinction was made in 1811 by Avogadro in the Journal de Physique, and although a few chemists used it from time to time in the interim (e.g., Dumas), it received little attention until forty years later. Avogadro's original paper distinguished between three types of molecules, 'molecule integrante' standing for molecules in general, 'molecule constituante' referring to individual molecules of elementary gases, and 'molecule elementaire' referring to individual atoms of elements.[105] In other words, the smallest naturally occurring units of elements might themselves be composed of more than one atom of the element. So that the contradiction implied in '$\begin{smallmatrix}\odot\\ \odot\end{smallmatrix} + O \rightarrow \begin{smallmatrix}\odot & O\\ \odot & O\end{smallmatrix}$' ('$2H + O \rightarrow 2HO$') could be handled by assuming that oxygen ('O') and hydrogen ('$\odot$') naturally occurred as diatomic molecules of two atoms--'$\begin{smallmatrix}\odot & \odot\\ \odot & \odot\end{smallmatrix} + \begin{smallmatrix}O\\ O\end{smallmatrix} \rightarrow \begin{smallmatrix}\odot & \odot & O\\ \odot & \odot & O\end{smallmatrix}$' ('$2H_2 + O_2 \rightarrow 2H_2O$').[106]

Had this addendum to Dalton's theory been acceptable to Dalton and other chemists, it would have, of course, extended the generality of the atomic theory. As Avogadro claimed:

> It will have been in general remarked on reading this Memoir that there are many points of agreement between our special results and those of Dalton . . . This agreement is an argument in favor of our hypothesis which is at bottom merely Dalton's system furnished with a new means of precision from the connection we have found between it and the general fact established by M. Gay-Lussac.[107]

It was, however, not accepted, partly because it seemed at the time *ad hoc*, arbitrary, with little independent experimental support to recommend it. Also, it seemed tangental to the main problem then accepted by atomic chemists, means for determining relative *atomic* weights, instead of relative molecular weights. Furthermore, since the number of gaseous and gasifiable substances was then relatively small, Avogadro's hypothesis *at the time* had a rather limited applicability, insufficient to sway chemists who were impressed by, *or* suspicious of, Dalton's theory.[108] Finally, since Dalton's (RGS) was arbitrary, many chemists developed their own methods of determining workable chemical formulae, the mere proliferation of which worried other chemists and directed attention away from Avogadro's potentially fruitful approach. In short, many of the reasons for rejecting Avogadro's hypothesis (not all) *sound like* architechtonic and explicative reasons (the problems it addressed seemed at the time tangental to 'paradigmatic' problems, a *general* distrust of chemical hypotheses not firmly supported by experiments, etc.). Still, this is an oversimplification. There were also what appeared at the time to be good methodological (constitutive) reasons for pursuing alternative approaches.

Central among these reasons is a problem that wasn't adequately handled until the atom itself was taken to be structured. How could like atoms attract one another and form stable chemical bonds? Such a view would, for one thing, undermine Dalton's explanation of the law of partial pressures and the observed 'mixed' composition of atmospheric air. Worse, after 1811 an increasing number of chemists began to use recent work in electrochemistry, and it was generally accepted by electricians and chemists that like charges _repel_ one another. Consequently, _if_ atoms are the smallest chemical units, and if they could be (reasonably) taken to be the bearers of 'plus' and 'minus' charges, then two or more atoms of the same element should certainly not unite into a stable bond.[109] This is one of the reasons, for example, that Berzelius took what would otherwise look like such a strange view concerning Gay-Lussac's law. He rejected Avogadro largely because of his 'dualistic theory', involving many of the electrical problems mentioned above.[110] On the other hand, he _partly_ accepted Gay-Lussac's law as a way of helping to derive chemical formulae, holding that equal volumes of _elemental_ gases contained equal numbers of atoms, but not that equal volumes of compound gases did.[111] He also thought that chemical compounds exhibiting similar properties should also have similar chemical formulae ('isomerism').[112]

What resulted was several different estimations of tables of atomic weights, so that instead of converging, potentially independent ways of determining atomic weights pulled chemists in different directions. Berzelius, for example, utilized something like Dalton's (RGS), chemical intuition, a primitive formulation of isomerism, and a modified version of Gay-Lussac's law of constant composition by volume to help determine chemical formulae. Due to the relative paucity of experimental results available at the time, and some tension between these and other methods, Berzelius constructed different tables in 1814, 1818, and 1826. Furthermore, Thomson, Dalton, and Berzelius, using different methods, obtained different chemical formulae for many substances.[113]

The formula for water, for example, was characterized as 'HO' (using modern symbolism, introduced by Berzelius) by Dalton and Thomson, and 'H_2O' by Berzelius, Davy and Wollaston.[114] This and other discrepancies (all concerning the determinateness of the relevant crucial quantities), led many chemists to lose confidence in the fruitfulness of the atomic theory, so that, by 1813-1815, Berthollet, Bostock and Wollaston retreated to what they took to be the theory-neutral empirical concept--'chemical equivalents',[115] standing for the fact that a certain quantity of one substance 'neutralized' the same weight of another. Consequently, while some of the early criticisms of the atomic theory (including the disaffections of some previous atomists) may have been due to general philosophical perspectives like favoring phenomenological descriptions over 'hypotheses', straightforward methodological problems regarding the crucial quantities in atomic theory both fueled this inductivist tendency where it was already present, and provided independent reasons for having second thoughts about the fruitfulness of the theory. Also, as I've already repeatedly stressed, the ensuing disagreements were not primarily concerned with which virtues were relevant in accepting or rejecting atomic theory, nor over what these virtues mean, but rather over whether or not the theory achieved a sufficient level of these to justify faith that it would eventually overcome these problems (whether or not the 'promissory note' of making the quantities determinate would be cashable).

In 1826, J. B. A. Dumas, in an attempt to use a new procedure that he hoped would render chemical formulae and relative atomic weights more determinate, instead obtained results that not only made this new procedure seem questionable, but greatly extended the growing scepticism regarding the atomic theory.[116] Dumas developed a procedure for determining the vapor densities of solid and liquid (at normal temperatures and pressures) substances. His basic procedure was to heat the substance in a glass bulb within a bath of water, for example, to a high enough temperature to vaporize it. When the substance vaporized, the neck of the glass bulb was sealed off with a torch, so that its temperature and pressure could be measured. When the bulb was cooled, the vaporized substance would condense and could be weighed, while the volume of the bulb was measurable in terms of the quantity of water it could hold.[117] This procedure should have greatly extended the determinateness of the crucial quantities we've been discussing, since only a few substances known at the time occurred naturally in the gaseous state. Furthermore, Dumas, at that time, favored atomism and was one of the few chemists who accepted Avogadro's hypothesis (which he attributed to Ampère). Using the idea that equal volumes of a gaseous substance contained equal number of atoms, his procedure of measuring vapor densities should have provided yet another method for determining relative atomic weights, and for substances previously inaccessible. His anomalous results, therefore, not only further weakened chemists' confidence in Avogadro's hypothesis, but, more generally, led to even more disaffection with the atomic theory.

The main problem was not Dumas' new procedure, but, once again, (in spite of his appreciation of Avogadro/Ampère), an ambiguous treatment of atoms and molecules. The vapor density method is valid for determining molecular weights of elements, but not directly for their atomic weights. Consequently, the atomic weights and chemical formulae suggested by Dumas' work (revised and extended by Mitscherlich) were often in direct conflict with those established by Berzelius' via chemical analogy. His value for sulphur, for example, was three times that allotted to it by Berzelius, while his value for mercury was one half that of Berzelius, and his value for phosphorus (and Mitscherlich's for arsenic) was twice that of Berzelius, etc.[118] As Freund points out, such apparently anomalous results could hardly fail to add fuel to anti-atomist sentiments.

> Hence a gallant attempt to rescue Avogadro's hypothesis was unsuccessful, and indeed led to contradictions calculated to bring discredit on the course advocated; and it is no wonder that considering the complex and involved nature of Berzelius' arguments in choosing the values for atomic weights, and the failure of Dumas' attempt 'to replace the arbitrary data by definite conceptions', the desire to do without any such hypothetical quantities should in many quarters have been strong.[119]

So far then, Dalton's atomic theory, while undoubtedly initially successful (virtuous), was also failing over a period of time to make its crucial quantities more determinate, and to exploit the increased generality it would have enjoyed had it been formulated in such a way as to be compatible with Gay-Lussac's, Avogadro's, and Dumas' results. While it would still be rational to accept it, given its initial success (and, it was still the most modest way to explain the law of multiple proportions, etc.), it was also rational, given the above apparently anomalous results, to suspend belief. In other words, compatable with diachronic realism, there was a rather lengthy period in the early-mid 19th century when Dalton's theory, while on the right track, was not (given all the available evidence), virtuous enough to subdue anti-realist objections.

What was needed was a classification of the concepts of 'atom' and 'molecule', and theoretical-empirical advances in several branches of chemistry (e.g., organic chemistry), to render the crucial quantities of relative atomic weights and chemical formulae more determinate. Besides the apparently anomalous results already discussed, other apparently disparate data such as electro-chemical results, crystallography, etc., eventually converged within a reformulation of atomic theory. In the 1840's and 50's, for example, Gerhardt and Laurent helped consolidate chemical formulae within organic chemistry, by concentrating more accurately on chemical analogy, and obtained results compatible with many of the above methods, and also used a clearer distinction between atoms and molecules.[120] Another approach that could have been used to make the relevant quantities more determinate was (especially Faraday's) work in electrolysis. Though Faraday himself preferred 'equivalents' to the 'hypothesis' of atoms, his electrolysis experiments potentially provided yet another independent method for determining the proportion by weight of elements separated from compounds by electrical means (and, hence, chemical formulae).

> A very valuable use of electro-chemical equivalents will be to decide, in cases of doubt, what is the true chemical equivalent or definite proportional, or atomic number of a body; for I have such conviction that the power which governs electro decomposition and ordinary chemical attractions is the same; and such confidence in the overruling influence of those natural laws which render the former definite, as to feel no hesitation in believing that the latter must submit to them also.[121]

Faraday, however, failed to pursue this any further, and Berzelius failed to extend it because of his distrust of Faraday's laws.[122] While Faraday's laws and empirical results eventually proved to be useful for chemistry, the theory of the electrical combination of chemical elements had to await the structured atom of 20th century physics.

Within chemistry, most of the above anomalies were satisfactorily handled in 1858, with the publication of Cannizzarro's "Sketch of a Course of Chemical Philosophy."[123] Though widely distributed after the Karlsruhe Congress of 1860, the importance of this work was not appreciated by many chemists for some time. By now, the anti-atomist-anti-hypothesis sentiment was strong enough to keep many chemists from recognizing what might have earlier been seen to have solved most of the anomalies plagueing the atomic theory since at least 1815. Using both the progress made in several branches of chemistry since Dumas' Lecons, and (finally) a clear distinction between atoms and molecules, Cannizzarro was in a good position to bring the previous anomalies into a unified perspective.

> I believe that the progress of science made in these last years has confirmed the hypothesis of Avogadro, of Ampère, and of Dumas on the similar constitution of substances in the gaseous state; that is, that equal volumes of these substances, whether simple or compound,

> contain an equal number of molecules: not however an equal number of atoms, since the molecules of the different substances, or those of the same substance in its different states, may contain a different number of atoms, whether of the same or of diverse nature.[124]

Returning to Dumas' and later chemists' work on vapor densities, Cannizzaro argues that the molecular weights of substances are proportional to their density in the gaseous state. Taking half of a molecule of hydrogen as unity, and referring the densities of other gaseous substances to a molecule of hydrogen (=2), he obtained a set of molecular weights consistent with chemical facts.

By comparing the data gained by using this assumption, relative atomic weights of a substance can be calculated by comparing the proportional molecular weights of "all or the greater part of the molecules in which it is contained, and their composition."[125] The weight of one volume of hydrochloric acid, for example, is 36.5 (taking 1/2 volume of hydrogen as unity), consisting of a weight of 35.5 of Chlorine, and 1 of hydrogen. One volume of water weighs 18, consisting of a weight of 16 for oxygen, and 2 for hydrogen. One volume of ammonia has a weight of 17, consisting of a weight of 14 for nitrogen, and 3 for hydrogen, etc.[126] From these results, consisting of many more chemical compounds than were available in the 30's, Cannizzaro reasoned that "the different quantities of the same element contained in different molecules are all whole multiples of one and the same quantity, which always being entire, has the right to be called an atom."[127] Consequently, a hydrogen molecule can be said to consist of two hydrogen atoms, hydrochloric acid contains one hydrogen atom, water contains two, ammonia three, etc. Also, for Cannizzaro, this indirect determination of atomic weights from molecular weights is not hypothetical. Since, if anyone were to compare the composition of equal volumes of substances in the gaseous state, "he will not be able to escape the following law: the various quantities of the same element contained in equal volumes either of the free element or of its compounds are all whole multiples of one and the same quantity."[128] Furthermore, this assumption can be independently tested by multiplying the atomic weights calculated as above by the specific heats of the substances involved (the amount of heat required to raise the temperature of the substance one degree). The resulting product turns out to be very nearly a constant (if the atomic weight value is correct).[129]

Finally, an indirect method is also available for determining the relative atomic weights of substances whose vapor densities cannot be determined. The constitution of the bromides, chlorides, and iodides of potassium, sodium, and silver, for example, could not be determined directly, because the vapor densities of these compounds was not known. However, by <u>analogy</u> with the protochloride of mercury (HgCl), and of copper (CuCl), and the specific heats of these free metals, one can assume that one atom of sodium, etc., combines with one atom of the relevant halogen in these compounds as well.[130] Combining results using vapor densities, specific heats, and chemical analogy, then, Cannizzaro was able to make a wide range of chemical formulae and relative atomic weights determinable, on a revised version of Dalton's atomic theory linked (finally) with Avogadro's hypothesis.

Cannizzaro succeeded, then, in establishing procedures for determining relative atomic weights that was no longer arbitrary--i.e., for some chemical compounds it was now possible to check the results of one method against those of others and obtain consistent results. Computations based on molecular weights taking 1/2 hydrogen molecule as unity, for example, could sometimes be independently checked using specific heat values, etc. Consequently, most of the previous anomalies concerning different atomic weight tables could now be resolved. Had this occurred earlier, it may certainly have quelled many of the anti-atomist objections concerning the determinateness of the crucial quantities assumed by the atomic theory.[131] At the time, however, the atomic-molecular theory of gases was being extended into the physical study of heat and energy by the development of the kinetic theory of gases. On the one hand, this extension greatly enhanced the generality of the atomic theory, and many physicists and chemists saw this as good evidence for the molecular (if not the atomic) theory of gases.[132] On the other hand, this extension of range also led to new empirical and conceptual problems. What, for example, happened to the heat energy absorbed by the molecules? If this absorbed energy was completely handled by the translation of the molecule, how could these molecules be 'halved', as Cannizzaro claimed?[133] Also, later spectroscopic studies indicated the necessity for a complex, structured atom, as opposed to the indivisible ultimate particles that had been assumed by all atomists since antiquity.[134] There were attempts to handle these objections with vibratory atoms, etc.,[135] but these problems as well as the problem of the electrical bonding of atoms could not be fully handled until the structured atom. Furthermore, phenomenological thermodynamics was soon to offer a serious competitor to the molecular theory of heat, which also led to new anti-atomist objections.

By the early 20th century, all of these objections could finally be met by an atomic theory that had given up the notion of indivisible atoms. Furthermore, the determinateness of the crucial quantities was established by the actual measurement of the actual (as opposed to relative) mass of atoms and their constituents, as well as Perrin's experimental work on Brownian motion.[136] As I claimed earlier, at this point, the atomic theory became modest, general, and determinate enough to warrant rational belief, in spite of the subsequent problems involving the nature of sub-atomic entities. I have also tried to show in the above account, that all of the meanderings of the atomic theory throughout the 19th century were primarily due to constitutive components involving the determinateness of the crucial quantities involved. It was primarily the proliferation and interpretations of experimental results that led chemists and physicists to their realist or anti-realist views concerning atoms.[137] If this is so, not only do extreme non-cumulative interpretations fail, but so do synchronic interpretations of realist/anti-realist debates. What evidence was or would be relevant was not very controversial throughout this period.[138] Whether the atomic theory had at various stages of its development sufficiently satisfied these evidential demands was controversial, at least until the early 20th century. This is exactly what should be expected if my treatment of diachronic realism is correct, and if my realist arguments for the scientific virtues were convincing, I can now claim that by the early 20th century, atomic theory was virtuous enough to warrant rational belief. Hence, Dalton's atomic theory, while not itself sufficiently virtuous to warrant belief, (from our perspective), was on the right track in that the success it <u>did</u> enjoy was due to the parts of Dalton's account that match or nearly match those of 20th century atomism.

The development of the atomic theory has received a great deal of attention by both historians and philosophers of science. Furthermore, Gardner has interpreted this development in a similar manner to diachronic realism, and Glymour has interpreted it in a manner which agrees with my contention that there was pronounced agreement concerning relevant evidence on the part of both proponents and opponents of the atomic theory, and that it progressively became better tested. The main innovations in my treatment of this case involve stressing methodological factors more than Gardner, and realist factors more than Glymour (diachronically interpreted), as well as my interpretation of Dalton's theory being on the right track. Atomic theory, however, is, in Putnam's and Laudan's terminology, a 'mature' theory, whose central theoretical terms may be said to have referred (via the causal theory of denotation), and which, therefore, <u>may</u> be open to a revised (CER) account. Consequently, while diachronic realism can be supported by the case of atomic theory, an older, less quantified theory might provide a better elucidation of being on the right track, and demonstrate how my interpretation of the 'success' of some past theories can be best explained by diachronic realism, and is more general and more adequate than any (CER) account.

Franklin's theory of static electricity provides a case that does not seem accessible to a notion of approximate truth (since it was not only non-quantitative, but many of its central theoretical terms failed to refer, unlike 'atom'). Still, it was, on any reasonable account of the term, 'successful' enough to provide a good test case for my claim that some levels of success are best explainable by the 'truth-likeness' of the theory, in terms of being on the right track. Consequently, I will now provide a brief account of the formulation and development of Franklin's theory as the final historical evidence for my version of diachronic realism.

Historical Cases: Franklin's Theory of Static Electricity

Franklin began his investigations of electricity in the mid 18th century as (is often the case) somewhat of an outsider, having become acquainted with only some of the previous European discoveries, and very little of the predominant contemporary electrical theories.[139] Hence, the researches of the Philadelphia electricians contained some inadvertent repetitions of previous experiments, but with a new theoretical framework which contained the germ of a fresh approach that was to prove very fruitful for the subsequent development of electrical science. The most prevalent theories of electricity at the time were in England, those of Watson and Wilson, and in France, that of Nollet. All were various forms of 'effluvial' theories of electricity, and all, as will be shown, encountered serious difficulties with the discovery of the Leyden Jar. It is quite possible that Franklin's superior theory of the phenomena of the Leyden Jar was to a certain extent due to the fresh theoretical start the Philadelphia electricians were forced to make. Before describing Franklin's new approach, it will be worthwhile to briefly review the effluvial theories that were his chief competitors.

After the _very_ general effluvial explanations of electrical phenomena towards the end of the 17th century,[140] the first important further developments were the experiments of Francis Hauksbee at the beginning of the 18th century. He was the first electrician, for example, to clearly recognize electrical repulsion as a genuine electrical phenomenon, and to support this with fairly well designed experiments.[141] By a more careful observation of the motions of small pieces of leaf brass in the proximity of a rubbed tube of glass, he noticed that:

> Sometimes the Bodies Attracted would adhere to the Tube, and there remained quiet. Sometimes would be thrown violently from it to good Distances: Sometimes in their Motions towards, and sometimes even touching it, they would suddenly be Repell'd back to a distance of 4 or 5 Inches, repeating the same several times with great Velocity in a very surprising manner.[142]

The violence of the bodies thrown off indicated that this phenomenon was not simply a case of their falling, but rather illustrated a real repulsive force that increased their velocity.[143]

Further experiments led to what would prove to be an even more important discovery, concerning what we would now call the direction of the electric field around an electrified glass globe. In one experiment, a wire hoop with loose threads attached to it was held over a glass cylinder or sphere so that the threads hung about an inch from the glass. When the glass was electrified, the threads all pointed radially inwards towards the glass.[144] However, if one's finger (or another object) was interposed between the glass and the threads, the threads lose this characteristic, but return to their original radial extension when the object is removed. Apparently, the 'effluvia' could not penetrate solid objects. If the threads were attached in the center of a hollow glass hemisphere, the threads again arranged themselves radially if the glass was rubbed. Again, when a finger was brought near, the threads lost this characteristic, "despite the fact that there was now a layer of glass between finger and thread."[145] Apparently, then, the effluvia could penetrate at least one solid object--glass. Further experiments seemed to indicate that glass was unique in this regard (even a thin piece of paper, or muslin, for example, would prevent the passage of the effluvia).

Despite the apparent transparency of glass, however, the fact that interposing material objects interrupted the effluvial flow provided one of Hauksbee's reasons for asserting the physical reality of the effluvia. Why else, for example, would it be interrupted by the interposition of a barrier? Furthermore, it could be felt upon one's face if a rubbed tube was held near, or seen and heard when a glass tube were rubbed in the dark.[146] Finally, the effluvia seemed to move in straight lines towards the glass, on both the hoop-thread experiment and the brass leaves experiment. This was to pose a problem for the earlier, general effluvial theories that viewed the effluvia as emanating because of friction exerted by rubbing. Boyle, for example, likened it to the emanation of smell produced by rubbing a stick of sulfur. At first Hauksbee too used such an account to explain the transparency of glass to the effluvia.

> The Effluvia which are provok'd from the glass seem to be, and are nothing else but part of the same Body exerted from it by rubbing; therefore (I think) can be no Impedimant to the Motion of its own Effluvia, . . .[147]

Then, however, he discovered that rubbed sealing wax could also influence threads or pieces of leaf brass through glass. How then could the effluvia of sealing wax penetrate glass? Only, on this analogy, if the parts of sealing wax and glass are "much alike."

Within two years, however, Hauksbee seems to have changed his mind about the nature of effluvia. Now instead of the 'stiff body' analogy utilized so far, he takes the effluvia to be more like a 'subtle fluid'. Still, however, some type of matter is emitted from the glass because of the rubbing, which remains the crux of the effluvium theory.[148] The fluid image seems to have originated because of Hauksbee's view that air was necessary for electrical effects, and that the electrical effluvia (and the heat of the friction caused by rubbing) caused changes in the density of the air around and inside the glass tubes, which needed to be equalized.

> If the Electrical Matter be emitted in Physical Lines, every where diverging from the Center of that Circle in which the Attrition is made . . . towards the Circumference of the same Circle; then by the Rarefaction of the Medium contiguous to the Glass, and the necessary Pressure of the more remote and dense Medium, into the Plane of that same Circle, with directions contrary to those in which the Effluvia are emitted: by this means (I say) the Threads may be regularly directed to the Center of that Circle, in whose Plane the Hoop to which they are fix'd is plac'd.[149]

That is, while electrical matter moves out of the tube, air moves in, accounting for the hoop-thread experiment, and small bodies being carried by the air towards a rubbed tube. This account handled a fair amount of the electrical phenomena known at the time, and its relative success may account for Hauksbee's not specifying how he handled Boyle's results that electrical effects could be created in a vacuum. Still, Hauksbee's experiments and theoretical accounts were the most general thus far, and were not really expanded upon or challenged for 20 years, until the work of Stephen Gray and Charles-Francois De Cisternai Dufay. Even then and later, the effluvial theories which preceded Franklin all shared aspects of Hauksbee's account, and the direct mechanical action of the effluvia, the transparency of glass to the effluvia, and the idea that the density of the surrounding medium accounted for electrical effects, remained cornerstones of the Effluvial theories.

Gray's experiments are noted chiefly because their results drew attention to electrical conduction, or the transmission of 'electrick vertue' through bodies--most of which we would now ascribe to electrostatic induction.[150] Prior to this, most effluviasts were convinced that the emanated effluvia must return to the parent body (e.g., the rubbed glass), and some used this as an explanation of attraction. After all, if the effluvia did not return, there should eventually be a marked diminishing of the mass of the parent body[151] (interestingly, Franklin used similar reasoning to discount Newton's corpuscular theory of light--the sun's mass should therefore diminish, etc.). Gray's experiments, on the other hand, showed that the effluvia need not return, by demonstrating that it could pass from one body to another, sometimes by contact, and sometimes by merely bringing a rubbed object close to another body (if it was a 'conductor').[152] His experiments led to the view that all bodies were either conductors or nonconductors, though he neither expressed this as a general law, nor used the terminology of conductor and nonconductor. He like Gay-Lussac, was much more of an experimenter than a theorist, and retained the vague notion of the transmission of the effluvia. In one striking experiment that attracted much attention, a small child was suspended by silk threads above (insulated) containers of small objects and then a rubbed glass tube was made to approach the child. The small objects were thereby made to dance back and forth between the child's face and hands and the containers, a spectacular instance of communication already demonstrated in a milder way with rubbed glass, cork and a feather, as well as with other objects.[153] He also showed (following Boyle, and against Hauksbee) that the attractive virtue was communicated in vacuo, as well as by the air. The results of his experiments were to prove increasingly troublesome for the effluvia approach. The communicated effluvia, for example, could be conducted through threads and (with difficulty) through air, but not at all through silk, glass, or resin, while the earlier experiments seemed to indicate that the effluvia could penetrate glass but not muslin, etc. Though this discrepancy did not yet cause mischief, such phenomena were eventually to prove embarrassing for the effluvia theory.

Dufay's work on electricity began by verifying and expanding Gray's work on conduction. More importantly, Dufay's experiments led him to propose that there were _two_ electricities, 'vitreous' and 'resinous' (later corresponding more or less to 'plus' and 'minus' electrification), the first exemplified by rubbed glass, the second by rubbed amber, though these did not correspond, for Dufay, to two distinct _substances_[154] (though later it would sometimes be interpreted in this way). Dufay also made the important discovery that bodies with different 'electricities' attracted one another, while like bodies repelled each other. Though also primarily an experimenter in the manner of Gray, Dufay did allow himself some tentative opinions as to the cause(s) of electrical phenomena. He apparently believed that bodies emanate electrical matter when approached by, for example, a rubbed glass tube, and that this electrical matter is retained around the body by envelopes or 'atmospheres', though he did not speculate concerning the nature of these atmospheres. Also, he made some use of electrical _tourbillons_ or 'vortices', without specifying their nature or their precise role in producing electrical effects.[155] Still, this last (Cartesian) element of his effluvial theory was to be picked up in more detail by one of Franklin's chief competitors--Nollet.

Hence, while many discoveries were made twenty years after Hauksbee's work, electrical theory was still quite rudimentary. Those that ventured to speculate at all continued to believe that there were many irregularities or 'caprices' contained in the subject. Consequently, while sharing the general effluvium research tradition of Boyle, Newton and Hauksbee (and in a sense, Descartes), none of these briefly articulated 'mini-theories' could account for all electrical phenomena, though each could claim superiority for their account of some phenomena.[156] Kuhn would call the state of electrical science prior to the 1740's a 'pre-paradigm' stage. In the terminology I've developed, there was so far an insufficient level of scientific virtues attained by any of these theories to establish any of them as a really successful theory, or at least to help choose between minimally successful theories. As we've seen, these theories, in so far as they were articulated at all, were not precise enough to lead to any clear cut commitments (determinateness), nor was any formulation general enough to adequately cover all of the data being discovered. Hence, at this stage of electrical theory, allegiance to effluvia, electrical particles, or whatever, was primarily philosophical, based on an allegiance to earlier corpuscular philosophy, and Newton's speculations in the "Queries" of his Optics. Methodological reasons for faith in a particular approach were as yet too few in number, and the theories themselves were too vague to allow the constitutive components I've argued are predominant in cases of theory choice to come fully into play. This was to happen later, as the various approaches became articulated enough to obtain clear commitments that could be verified or falsified by experiment, and by the level of virtues obtained by the theories.

One other factor that needs to be discussed before Franklin's chief competitors, is the (relative) popularization of electrical phenomena, caused primarily by the experiments of German electricians, some of whose experiments were communicated to Franklin. Bose, for example, immediately prior to the 1740's developed a more sophisticated electrical machine for producing electrical effects. Consisting of a bicycle-type apparatus which could whirl a glass globe or cylinder at great speed for a considerable length of time, and a 'prime conductor' (a metal rod suspended by silk, insulating, threads), which could be electrified by the whirling glass much more effectively and powerfully than in the older rubbing techniques. With such a machine, Gray's and Dufay's experiments could be conducted more quickly, easily, and with much more powerful effects, in a more consistent manner.[157] He also conducted his own experiments, some of which were little more than 'party tricks' in the genre of the 'whoopy cushion',[158] but which did succeed in popularizing (De gustibus non disputandum est) electricity. Also, one of his particular tricks was a near-predecessor to the Leyden Jar. Knowing that water could draw sparks from an electrified object, he conducted an experiment to see if the converse were also true, and succeeded in drawing sparks from an electrified glass of water.[159] Had he held the glass while he tried to draw a spark from the prime conductor, . . . Again, however, the level of available theory was quite low, and was directed by, in Heilbron's terms, "rough, amateurish, superficially plausible systems."[160] Consequently, it was not at a higher level than the theories discussed in the previous paragraph.

In France, Jean Antoine Nollet, an assistant of Dufay in 1731 or 1732, developed the first more determinate electrical theory, after hearing of Bose's experiments in 1745, and himself constructed a "great wheel, fully equipped with globes." Equipped with this means of producing more powerful and consistent electrical effects, Nollet soon invented some two dozen new demonstrations, and a new theory to go along with them.[161] First, Nollet affirmed the existence of 'electrical atmospheres' surrounding electrified bodies, and maintained that <u>all</u> bodies contained electrical matter, which can be expelled from them when their parts are sufficiently agitated.[162] The expelled electrical matter takes the form of an 'effluent stream', issuing from certain 'pores' of the electrified body in divergent jets, which can be made <u>visible</u>, for Nollet, by spreading fine dust on the prime conductor and watching some of it leap up from the conductor at various points like "so many water jets."[163] Some of the dust, however, remained on the conductor, which for Nollet, illustrated the existence of a second 'affluent flow' back to the conductor.[164] These 'effluent' and 'affluent' streams of electrical matter, together with respectively different 'pores' in the body, were the cornerstones of Nollet's system,[165] and were articulated in such a way as to make his system vastly superior to the one's already discussed.

A problem for all previous theories, for example, was that objects seemed to first be attracted by an electrified body, then, after contact with it, were repulsed (ACR). By varying (besides the direction of the two streams) the velocities and spatial arrangements of the streams (currents), Nollet was able to provide a plausible explanation for (ACR) phenomena.

> Consider an electrified body of cylindrical shape surrounded by small objects E F G immersed in a moderate flow, well removed from the strong divergent effluent currents: nothing impedes the 'attraction', the drift of these mites towards the excited electric. When the small body touches or nears the electric, it is stimulated to emit its own effluent jets, to which Nollet assignes properties similar to those of Hauksbee's stiff effluvia; the charging of the small object proceeds like the unfolding of the tentacles of an octopus, giving purchase to effluent spurts that 'repel' or drive it away.[166]

Why does a suspended leaf fly to a finger presented to it? Well, since the electrical matter 'flows' less readily through air than through (non-resinous) denser bodies, the interposed finger decreases the density (so to speak) of the affluent flow of the leaf on the side of the finger, and hence the increased affluent flow on the far side pushes it towards the finger, while the effluent flow issuing from the leaf readily permeated the finger (and hence is not repelled from it). This attraction does not take place if a wax rod (resinous) replaces the finger, because, being more resistant to the electrical flow, the consequent added repulsion between the leaf and the rod caused by the leaf's effluent flow on the side of the finger <u>neutralizes</u> the increased affluent flow on the far side (brought about, again, by the interposed object increasing the <u>density</u> on the other side).[167] Hauksbee's important hoop-thread experiment can be explained in a similar way, again on the assumption that glass is transparent to electrical matter (common to all effluviasts).

Nollet's system could apparently handle virtually all of the electrical phenomena discovered up to that time (all thought associated with (ACR)). Furthermore, its more precise formulation of the nature of electrical phenomena (unlike Dufay's), rendered it more determinate (e.g., glass _must_ be penetrable, there must be both effluent and affluent streams of the electrical matter, resinous bodies block the flow while non-resinous bodies do not impede it, etc., any of which if shown wrong, topples Nollet's system, while Dufay's vague speculations seem to have no particular commitments that could not be amended if the need arose). Nollet was also an able experimenter, who traveled throughout Europe demonstrating his theory to almost everyone's satisfaction. Given this, and the results of his visible demonstration of the streams by putting fine dust on the prime conductor, there was good reason (at last) to have a certain amount of _faith_ in an electrical theory, _given the level of available evidence_. Before the Leyden Jar, and Franklin's clear formulation of an alternative theory, Nollet's theory was the first that was virtuous enough to be taken as a standard. Thus, again, the convergence of opinion regarding the 'success' of Nollet's system was due to its 'virtuousness', and between 1745-1752, it enjoyed the widest assent yet received by any electrical theory.[168]

William Watson began his systematic study of electricity in 1745. One of his experiments, which was later adequately explained by Franklin, consisted in having two persons stand on wax cakes (insulating them), and electrifying one of them. If this electrified person (A), touches the other (B), he loses 'almost all of his electricity', and B receives it in one sensible 'snap'. If this is repeated a number of times, the 'snapping' progressively diminishes until it becomes insensible when B is 'impregnated with electricity'.[169] Another of his experiments brought him close to the problem of the penetrability of glass by the electric matter. Placing some books atop an inverted wine glass containing some leaf brass, he brought a rubbed tube above the glass. Eventually, the brass leaves responded to the motion of the tube, but not, for Watson, until 'the electricity has fully impregnated the books'. Then, after the glass first hinders the flow (so that the books can become 'impregnated'), the electric matter eventually permeates the glass and the brass leaves respond. Watson, therefore, claimed that glass was 'semitransparent' to the electric matter (why?--he does not say), and that the effluvial flow must attain a certain strength before it could penetrate the glass.[170] Once again, these types of explanations would later have trouble with accounting for how a charged Leyden Jar could retain its charge.

In 1746, Watson presented a 'Sequel' to the Royal Society, which supplied his account of the electrical fire, or effluvia. Based on the surprise he experienced when an insulated operator failed to provide more electricity (on the assumption that the electricity originated in the rubbed glass, less should 'escape' through the operator if she were insulated), he, and others familiar with this effect, began to conjecture that the electricity emanated not from the rubbed glass tube, but from the operator, and, ultimately, from the ground she stood on, so that the tube acted as a kind of 'pump'.[171] Consequently, Watson came to see most electrical phenomena as cases of the electric aether trying to restore its equilibrium (similar to Franklin), but in taking the 'fluid' analogy too literally, he identified this in terms of the differing densities of the electrical matter brought about by electrical experiments, parallelling Nollet.

> The equilibrating aether flow is the agent of attraction and repulsion. To save the phenomena, Watson must make the current of effluvia run initially in a direction opposite to that demanded by the theory. It is to flow towards electrified objects, towards objects already clothed with atmospheres, from the naked conductors about them: 'The blast of electrical aether constantly sets in from the nearest unexcited non-electrical towards those excited, carries with it whatever light objects be in its course'. That is attraction. On approaching an excited object the light bodies encounter the equilibrating aether blast the original theory promised, and are repelled.[172]

On the other hand, like all English electricians, Watson interpreted electrical phenomena as the conduction of electrical matter, or a change of electrical state, rather than the French model, which interprets electrical phenomena as the propagation of a state, or the stimulation of a flow of effluvia.[173] Hence, despite his density interpretation and tendency towards Nollet's theory, Watson was even closer to Franklin's account.

Benjamin Wilson was Watson's chief competitor for the title of 'chief electrician' in England. Using electrical machines (like Bose's) rather than a simple rubbed glass tube, he quickly noticed that in the phenomena of sparks passing between a grounded metal plate and a continually electrified one, the sparks appeared to originate from both plates, rather than only from the one which possessed a surplus of effluvia.[174] From this he inferred that excited and unelectrified bodies differed in degree rather than kind (consistent with Watson's 'equilibrium' view), but when he conducted the experiment of insulating the operator, he expected a diminishing rather than an enhancement of the electrification.[175] Wilson postulated that the electrical matter was Newton's gravitationaloptical aether (from the "Queries" to the Optics), mixed with luminous and sulphurous particles. Hence:

> The (electrical) machine works by 'vibrating' the aether naturally in or around the hand or cushion; the vibration projects electrical matter onto the glass, which, being an electric substance and highly 'elastic' besides, drives it onto the prime conductor, the closest object more apt to receive it. The process can continue if a reservoir of electrical matter, like the ground, replenishes the particles lost by the rubber.[176]

Consequently, if the operator is insulated, little or no electrification can occur. Of course, identifying the aether of electricity with gravitational aether seemed absurd on the face of it. Gravitational attraction, after all, depends on the mass of the attracting objects, while electrical phenomena do not, and there seems nothing comparable in gravitational theory to the vitreous and resinous duality. Still, this theory too had its (brief) day, but was soon (with the others so far mentioned) to experience the 'shock' of the Leyden Jar.

In 1746, Pieter van Musschenbroek, reported a phenomenon to Reamur that had been accidentally discovered by Andreas Canaeus, a lawyer trying to reproduce some of Musschenbroek's experiments, and then investigated by himself.[177] If a wire is linked to a prime conductor of an electrical machine and a glass of water, if a person holds the glass of water and tries to draw a spark from the machine, she will receive a violent shock, but only if the same person holds the glass and approaches the machine.[178] How was it possible to put such a fantastic charge into a glass of water only if the experimenter held the glass and drew the spark? There were now frank reports of puzzlement on all sides, the most important probably coming from Nollet. He admitted that the natural explanation of the large shock received would be to assume that the glass somehow retained and built up a charge. On the other hand, like all effluviasts, the penetrability of the glass to the electric matter played a crucial role in his explanation of many other electrical phenomena. Still, repeated experiments affirmed the paradoxical results so that Nollet was forced to treat the jar as an anomaly, not explainable by contemporary electrical theory. As Heilbron reports:

> The feebleness, imprecision, and incompleteness of the theories of Nollet, Winkler and Watson, their failure to explain or even to mention characteristic features of the condenser (Leyden Jar), require no comment. They offered no guidance or stimulation. . . . A new approach was needed.[179]

This new approach was to come, from (of all places!) the colony of Pennsylvania, and the electrical theory of Benjamin Franklin.

In one of his first communications concerning electricity, Franklin took up the 'men on cake wax' experiment of Watson, with a few additions of his own, as well as an <u>explanation</u> of the effects, involving his new terminology of 'plus' and 'minus' electrification.[180] Again, two persons (A and B) are standing on wax, one (A) rubbing the tube and the other (B) drawing the charge. As long as they do not touch one another, they will both seem electrified to a third person (C), who is not insulated (i.e., C will exchange a spark with either A or B). If they touch one another during the rubbing of the tube, however, they will not appear electrified to C. On the other hand, if A and B touch one another <u>after</u> the tube is rubbed, there will be a stronger spark between them than between either of them and C. After this 'discharge', neither of them will appear electrified to C. Like the English Electricians, this experiment suggested to Franklin that the rubbed glass acts as a 'pump',[181] but unlike them, his interpretation is much more detailed.[182]

Franklin supposes that there is one 'electrical fire' (later called by him 'electrical matter', and then 'electrical fluid'), of which all three persons in the above experiment have their equal share. A (who rubs the glass) communicates some of his share to the glass, and since he is insulated by the wax, this cannot be replenished from the ground. B (who received the charge) accumulates an extra amount of electrical fire, and because he is also insulated, must retain this surplus. Both appear electrified to C, who, standing on the ground, retains his normal share and, hence, communicates some of this to A (whose share is deficient), and takes some from B (who has a surplus). The spark is greater if A and B approach one another after the glass is rubbed because there is more of a difference between A and B than between either of them and C. If A and B approach while the glass is being rubbed, on the other hand, the electrical fire merely circulates, and the equality is not destroyed, hence, no spark results.

> Hence have arisen some new terms among us: we say B, (and bodies like circumstanced) is electrised _positively_; A _negatively_. Or rather, B is electrised _plus_; A _minus_. . . . To electrise plus or minus, no more needs to be known than this, that the parts of the tube or sphere that are rubbed, do, on the instant of the friction, attract the electrical fire, and therefore take it from the thing rubbing: the same parts immediately, as the friction upon them ceases, are disposed to give the fire they have received, to any body that has less.[183]

Hence, _a la_ the English electricians, the electrical matter can be communicated, and loss of equilibrium of this matter results in electrical phenomena (at least charging and discharging). Even here, however, Franklin takes exception to Watson's interpretation of the direction of this communication.[184] Also, the terminology of 'plus' and 'minus' already anticipates Franklin's insistence on the conservation of electrical charge, crucial to Franklin's theory, and an experimental law that still stands.[185]

This concept also provided the 'new approach' necessary for understanding the Leyden Jar. Three months after the above letter to Collinson, Franklin wrote him another, in which he specifically discusses "M. Musschenbroek's wonderful bottle."[186] As the glass is charged by the wire leading to the prime conductor it retains this surplus, <u>since glass is impermeable to the electrical fire</u>. Since, because of the conservation law, a surplus of electrical fire in one place requires a surfeit in another, the outer coating of the glass <u>gives off</u> equal quantities of electrical fire to that being added to the inside via the prime conductor, through the man holding it and into the ground. The glass can therefore accumulate a great difference between 'plus' and 'minus' electrical fire on its inside and outside, until the latter has no more electrical fire to communicate. Equilibrium can only be reached (as with the experiment with the three persons standing on wax above) if the inner and outer coatings can communicate with one another (via the person holding the glass and drawing a spark from the wire). Hence, the <u>violent</u> shock if the same person holds the glass and tries to draw the spark. Also, a spark can only be drawn out of the top, if one can also enter the outer coating, so that if the glass is placed on an insulator there will be no spark. Hence, Franklin's concepts of plus and minus electrifications and the conservation of electric charge, together with the assumption that glass is impenetrable to the electric matter, allows Franklin to modestly account for a phenomenon that baffled the leading electricians in Europe. He also produced eleven elegant experiments to support this view.[187]

Some historians would argue that it was <u>because</u> Franklin early concentrated upon Leyden Jar phenomena, while Nollet and others concentrated on (ACR) phenomena, that Franklin's theory constituted a new approach (note: there is no mention of attraction or repulsion in the above accounts by Franklin). To a certain extent, this is probably true, particularly together with the fact that, being relatively isolated in the new world, Franklin was only partially aware of previous theories when he began his electrical investigations. It is going too far, however, to further claim that this resulted in the incommensurability of the two approaches, and that Franklin and (for example) Nollet didn't even agree on what constituted a successful electrical theory. While some non-methodological considerations no doubt played a role in the subsequent theoretical arguments, it has already been argued (in the last chapter) that Nollet also appealed to experimental facts, and many of the later arguments between proponents of Nollet's and Franklin's theories revolved around data that their individual theories could best explain ((ACR) phenomena for Nollet, Leyden Jar phenomena, and, partially, induction phenomena for Franklin). As will be seen, Franklin eventually ran into trouble with (ACR) phenomena, particularly the mutual repulsion of two negatively charged bodies. Hence, the debates were primarily over which theory explained the most, and the most <u>important</u> facts, not over what it means to explain experimental facts, nor over the importance of experimental verification. Eventually, I want to argue, it was the greater variety of phenomena exhibited by the Leyden Jar, and so, explained by Franklin, and the increased determinateness engendered by his experiments, that led many electricians to side with him, and not conflicting 'world views'. As Priestley points out: "Dr. Franklin's new theory of charging the Leyden Jar led him to observe a greater number of facts, relating both to charging and discharging it, than other philosophers had attended to."[188] In other words, from my perspective, it was the virtues that I have been arguing for that played the dominant role in this case of theory choice, as I've argued they have for others.

So far then, Franklin has provided a very modest, general, and (as we shall see) relatively determinate account of the Leyden Jar. Still, 'plus' and 'minus' electrification, the conservation of charge, and the impenetrability of glass, however suggestive and nicely documented, are closer to empirical laws than to a theoretical account of electricity.[189] After more experiments, especially on Leyden Jar phenomena, Franklin first articulated his theory of electricity in a formal paper, addressed to Peter Collinson, July 29, 1750.[190] It will be useful at first to borrow a succinct statement of the general outline of this theory from Heilbron, and then discuss the explanations it provides of a wide range of electrical phenomena.

> The phenomena of electricity, he says, depends upon the interactions of two types of matter, the common and the electrical. Elements of the former attract, those of the latter repel one another; while between a particle of the common, and one of the electrical matter a strong attraction obtains. Because of these forces, and the great subtlety of electrical fire, neutral matter is a 'kind of sponge' crammed with as much of the fire as it can hold. Between two neutral bodies there is no net electrical interaction. For 'electrical signs' to appear--attractions, repulsions, sparks, shocks--electrical matter must be accumulated. The accumulation, or excess beyond the amount required to saturate a body, 'lies without upon the surface, and forms what we call an electrical atmosphere'. We know that, in general bodies contain nearly their just

> quantities of electrical matter, for a small increment usually confers an atmosphere; and we know that, since bodies with atmospheres repel one another, common matter does not normally contain more of the electric matter than it can absorb.[191]

Franklin reports some nice experiments to support his postulation of atmospheres, and some which seem, to him, to render them visible, using smoke, etc. (much like Nollet's demonstration of his effluent jets using fine dust on the prime conductor).[192] He also speculates concerning the ease with which pointed objects more easily receive and transmit electric sparks than blunted objects (as he did in his first paper), though he later came to be less sure of this.

Most important, perhaps, are his detailed account of the impenetrability of glass to explain the Leyden Jar and his explanation of older experiments (like the inverted wine glass experiment, and Hauksbee's hoop-threads on the *inside* of a glass cylinder) that seem to indicate that glass *is* penetrable.[193]

> *We cannot by any means we are yet acquainted with force the electric fluid through glass.* I know it is commonly thought that it easily pervades glass, and the experiment of a feather suspended by a thread, in a bottle hermetically sealed, yet moved by bringing a rubbed tube near the outside of the bottle is alleged to prove it. But, if the electrical fluid can easily permeate glass, how does the vial become *charged* (as we term it) when we hold it in our hands? Would not the fire thrown in by the wire, pass through to our hands, and then escape into the floor?[194]

In fact, for Franklin, if there is a crack in the glass, the bottle will not charge, which certainly seems to further support his view. Also, an experiment that Franklin feels might convince a "slight observor" that glass was penetrable, on closer examination proves the contrary. If a Leyden Jar is placed on a glass stand beneath the prime conductor, and a bullet is suspended by a chain from the prime conductor to within 1/4 inch over the wire of the bottle, and one places her knuckle on the glass stand 1/4 inch from the jar, a spark will fly from the bullet to the wire as the globe is turned, and another spark will be seen (and felt) at the same instant passing to the knuckle. Does this not demonstrate that the same electrical fluid that enters the wire, passes through the glass of the jar and onto the knuckle? No!, according to Franklin, since the jar will be charged by this procedure, which would be impossible if it were the same spark that entered and left the jar (i.e., the total electrical fluid contained in the jar would be the same as before the turning of the globe if the same spark entered and left, or for that matter, if glass permitted the electrical fluid to pass through). Rather, for Franklin, this experiment provides an elegant support for his account of the jar; that for every surplus charge forced into the inside, an equal quantity will be discharged by the outside. If this were not the case, there is no way the jar could charge.[195]

One of the most fruitful parts of Franklin's theory, and the most troublesome given his model for it, is his near anticipation of electrostatic induction. Franklin's postulation of 'electrical atmospheres', as in most of the previous effluvial theories, was an attempt to provide a mechanical (non action at a distance) account of (ACR).[196] Unlike earlier theorists, however, he did not take his fluid analogy so literally as to think that these atmospheres 'mixed', or that there was an actual transfer of electrical fluid in electrical phenomena. Rather, the atmospheres reacted to each other in a static way. As Heilbron claims:

> Somehow an atmosphere causes its host and neighboring neutral bodies to come together, and two atmospheres, without mixing, force their (positively) charged possessors to move apart. Whether the

> atmospheres rotate, gyrate, pulsate, or vegetate does not appear; compared to earlier models they seem lifeless, even vegetal.[197]

Besides the lack of articulation concerning atmospheres, his brief account of them seemed contradictory; the 'air' both being transparent to them, and, sometimes, the mechanism which retained them around the bodies they surrounded.[198] Furthermore, of course, such a model could provide no explanation of the repulsion between two negatively charged bodies.[199] Still, his confusing, static atmospheres were a strength as well as a weakness of his theory. Not being able to surrender mechanical models (and not being mathematically sophisticated enough to investigate action at a distance alternatives--reminiscent of the 'bag of marbles' account of atoms in Dalton's model for his law of partial pressures), he was also, due to the cornerstones of his new approach, unable to fully embrace the _literal_ mechanical views of the effluviasts. Consequently, what he _said_ was confused by his impotent atmosphere model, but what he _almost said_ in several places comes very close to action at a distance, which would be successfully used by mathematical electricians in the near future.[200]

Consider, for example, Watson's intermediate position between Franklin and Nollet. He shared, again, the English conception of the imbalance of electrical equilibrium, and Nollet's Cartesian concept of varying _densities_ of the electrical matter, the surrounding air, etc. Though Watson at first thought his position similar to Franklin's, his literal mechanical-dynamical view of bodies moving in response to two streams of fluid was in fact closer to Nollet than to Franklin's ambiguous account of 'atmospheres',[201] (although his equilibrium view was, as was claimed earlier, more like Franklin's). While his followers cannot be blamed for their misinterpretation of Franklin's troublesome model, which also provided grounds for Nollet's counterattack,[202] his disclaimers concerning their interpretations of his system demonstrate that he was in principle, if not in fact, theoretically beyond the mechanical constraints of earlier theorists, and this struggling with new concepts is one of the reasons his theory was on the right track.

John Canton's experiments on what we would call electrostatic induction[203] provide a good example for showing Franklin's theoretical acumen, hampered as it was by his atmosphere model. If two cork balls, for example, are suspended from the ceiling by 8-9 inch linen threads so that they hang in contact with one another, and a rubbed glass tube is made to approach them, the balls will separate when the tube is three to four feet away, and will separate farther as the tube is brought nearer. If the balls are suspended by dry <u>silk</u> threads, however, the tube must be brought within 18 inches before they repel one another, and they will continue to repel one another after the tube is withdrawn! In the first instance, according to Canton, since the balls are not insulated (due to the linen threads), they are not electrified (since the surplus electricity passes through the threads to the ceiling), but the atmosphere of the approaching tube causes them to "attract" and "condense" the electrical fluid about them, and separate because of the repulsion of the electrical particles.[204] Since they are insulated in the second experiment (due to the silk threads), the tube must approach nearer because a denser (!) part of the atmosphere of the tube is necessary for them to repel one another. The dynamics involved here in Canton's account can be made clearer by considering another one of his experiments.[205]

If a tin tube 4-5 feet in length and two inches in diameter is hung from the ceiling by silk threads, and cork balls are suspended from one end of it by linen threads, and the tin tube is electrified by bringing an excited glass tube near the opposite end from the cork balls, the balls will diverge from one another to hang one and a half inches apart. As the glass tube approaches closer, they will lose their repelling power and converge, if still closer they will again repel one another and to the same distance as before. As the glass tube is withdrawn, they will reverse this process. If the tin tube is electrified instead by the approach of a rubbed <u>wax</u> tube (negatively charged), the balls will behave in a similar manner. On the other hand, if the tin tube is electrified by glass and approached by wax (or vice versa), their repulsion will be increased. Why? For Canton, the glass tube electrifies the tin tube positively, adding to the electric matter it contained, so the balls will attain a surplus of electric matter through the linen threads and will repel each other. But as the glass approaches closer, emitting electric fluid, the discharge of it from the balls will be diminished, a part of it will be driven back and they will converge.[206] And:

> If the tube be held at such a distance from the balls, that the excess of the density of the fluid round about them, above the common quantity in air, be equal to the excess of the density of that within them, above the common quantity contained in cork; their repulsion will be quite destroyed.[207]

If the tube is brought closer, however, the fluid without will again be more dense than that within the balls; they will attract it, and recede from one another again. The increased distance of the repulsion if the tin tube is electrified positively and approached by a negatively charged rod (or vice versa), is due to the increased difference in density between the balls and the fluid surrounding them. This is, of course, a hodge podge, consisting of parts of Franklin's static atmospheres, older density views of effluvia, and new facts dictated by these experiments. Still, his experiments were elegant and suggestive, despite the crudity of his explanations.

Franklin's response to Canton's work shows his ability to clarify experimental results, and give a good indication of his non-mechanical tendencies.[208] He begins his account with a restatement of his distinction between (what we would call) inductive effects, and those involving actual transference of electrical matter, again, by denying that atmospheres 'mix' (i.e., his static/dynamic, density account).

> Electric atmospheres, that flow round non electric bodies, being brought near each other, do not readily mix and unite into one atmosphere, but remain separate, and repel each other.[209]

Furthermore, atmospheres not only repel one another, but also the electric matter contained in the substance of a body, "and without joining or mixing with it, force it to other parts of the body that contained it."[210] This is, of course, to implicitly accept action at a distance, and was to later be utilized by more mathematically inclined electricians, until it was used quite fruitfully by Coulomb in the first really adequate mathematical account of static electricity.[211] That Franklin's account came close to later action at a distance treatments was noted by Priestley, who first provides Franklin's response to Canton and praises it for its simplicity (modesty), and then goes on to argue that the phenomena could also be accounted for by action at a distance models, introducing the subsequent work of Wilcke and Aepinas, and the eventual overthrow of the entire 'atmosphere' approach.[212]

In summary, Franklin's theory, though at times an odd mixture of old and new concepts, provided the first adequate explanation of the multi-varied experimental phenomena engendered by the Leyden Jar, and suggested (while still hamstrung by an inadequate model), fruitful approaches to electrostatic induction that were to destroy mechanical explanations of static electricity until the field theories of Faraday and Maxwell in the 19th century. Furthermore, his terminology of 'plus' and 'minus' electrification remains a useful device to this day, and his utilization of the law of the conservation of electric charge remains an undisputed physical law. There are indeed no 'atmospheres' and no 'electrical fluid', but Franklin's struggle with these inadequate mechanical concepts is much to his credit, since unlike his contemporaries and predecessors, he was never willing to sacrifice experimental demonstrations to contemporary mechanical models. While not true or approximately true, therefore, Franklin's approach was on the right track as can be illustrated by the easy translation of the salient elements of Franklin's theory into the later concept of fields of force, and the electron theory of the 20th century.

> We have been able to measure the masses associated with given charges of electricity in gases at low pressures, and it has been found that the mass associated with a positive charge is immensely greater than that associated with a negative one. This difference is what we should expect on Franklin's one-fluid theory, if that theory were modified by making the electric fluid correspond to negative instead of positive electricity, while we have no reason to anticipate so great a difference on the two-fluid theory. We shall, I am sure, be struck between the similarity between some of

> the views that we are led to take by the result of the most recent researches with those enumerated by Franklin in the very infancy of the subject.[213]

Hence, on my account, we can talk about the 'truthlikeness' of Franklin's theory, in terms of his nearness to views that are more nearly correct (from our perspective--benign chauvinism). Also, I will try to further justify this claim by showing that the increased success of Franklin's theory at the time it was developed was because of this truthlikeness. That is, his theory succeeded where his competitors did not because of its anticipation of part of the correct account of electrostatics.

Franklin's use of the law of the conservation of charge, for example, was one of the main reasons his theory provided a better account of the Leyden Jar than earlier effluvial approaches, and this law is an empirical law. Consequently, while his terminology and models often diverge from our treatment, in so far as he remained faithful to this law, one would expect his theory to work. Furthermore, while 'atmospheres' did not (and could not) provide a correct account of many electrical phenomena, especially electrostatic induction, his confused, static, interpretation of these did not lead him into the same mechanical pitfalls experienced by his competitors. It was his allegiance to the conservation of charge, and his 'plus' and 'minus' terminology (as well as 'charge' and 'discharge') that confused his contemporaries, and separated his theory from the sterile 'density' interpretations of Watson, Wilson, and Canton. In other words, from my perspective, while there were previous 'successful' theories of electricity, Franklin's achieved a greater level of the virtues, and proved fruitful for later electrical work because he had tapped into notions that are either correct, or directly led to notions that were correct. His theory was thereby on the right track, and the level of virtues it achieved it achieved _in so far as_ it was on the right track (i.e., it was 'truthlike', though not true or approximately true, given the non-denotative status of many of its central theoretical terms).

Also, while there were problems with his account of (ACR) phenomena (and hence Nollet and others were not automatically 'stuck in a competing paradigm' or 'stubborn' in their refusal to accept his approach), his Leyden Jar experiments and explanations were considerably more virtuous than his competitors' accounts. Furthermore, since Leyden Jar phenomena eventually proved to encompass more data than traditional (ACR) data, and eventually proved more fruitful for action at a distance models, and electrostatic induction, his theory was more virtuous given the entire range of data. Hence, even the later competing 'two-fluid' theory remained closer to Franklin than to (for example) Nollet's views, which quietly died out. In other words, while there were some good reasons for rejecting Franklin's account, and even more good reasons (e.g., the equivocal treatment of 'atmospheres') for reserving judgment, Franklin's theory proved to be _more_ virtuous (even at the time), and hence the gradual converging of opinion concerning (at least) Franklin-like approaches, over older effluvial accounts. Franklin's theory never achieved a level of virtues to warrant rational belief, but it was still an impressively successful theory. On my account of diachronic realism and truthlikeness, the relative success of all of the above early electrical theories was due to their being right about _something_. Franklin's theory, however, because it _directly_ led to more adequate accounts, and because of the relative ease with which it can be 'translated' into current accounts, was also on the right track. Hence it was more 'truth-like' and consequently more virtuous than its competitors _because_ it comes closer to later true accounts. I think that my explanation of the success of Franklin's theory succeeds (while (CER) accounts fail) in fulfilling the realist desideratum that theoretical success is best explained by a realist account of (some) scientific theories. Consequently, I think that this chapter provides reason to hope that diachronic realism is historically adequate, as well as conceptually adequate.

NOTES

[1]Larry Laudan, for example, has attacked realist positions involving a variety of these desiderata, including the view that cognitive progress or growth is associated with the cumulative retention of explanatory success, the translatability of one theory into another, and what he calls 'convergent epistemological realism'. For the first two, see Larry Laudan, "Two Dogmas of Methodology," Philosophy of Science, vol. 43 (1976). For the last, see Larry Laudan, "A Confutation of Convergent Realism," Philosophy of Science, vol. 48 (1981).

[2]Larry Laudan, Progress and Its Problems: Towards a Theory of Scientific Growth (Berkeley: University of California Press, 1978), p. 15.

[3]Ibid., p. 17. One can also add to this list cases such as attempts to measure the aether drift in the late 19th century, and attempts to determine the specific properties of the caloric envelope surrounding atoms.

[4]Ibid., "The only reliable guide to the problems relevant to a particular theory is an examination of the problems which predecessor--and competing--theories in that domain (including the theory itself) have already solved," p. 21.

[5]Larry Laudan, "The Sources of Modern Methodology: Two Models of Change," in Science and Hypotheses: Historical Essays on Scientific Methodology (Dordrecht: Reidel, 1981). "We need to realize that succesful theories not only inspire new theories of methodology, they also--in a curious sense--serve to justify those methodologies. For instance, the success of Newton's physics was thought to sanction Newton's rules of reasoning; Lyell's geological theory was cited as grounds for accepting methodological uniformitarianism; the kinetic theory of gases and Brownian motion were thought to legitimate epistemological realism; these are but a few examples of a very common phenomena," p. 16.

[6]Progress and Its Problems, p. 22.

[7]Clark Glymour seems to be of the same opinion. See, Clark Glymour, Theory and Evidence (Princeton: University of Princeton Press, 1980), pp. 100-101.

[8]Thomas Kuhn, "The Function of Measurement in Modern Physical Science," in Harry Woolf, ed., Quantification: A History of the Meaning of Measurement in the Natural and Social Sciences (New York: Bobbs Merrill, 1961), p. 42.

[9]Galileo Galilei, Dialogues Concerning Two New Sciences, Henry Crew and Alfonso de Salvo, trans. (New York: Dover, 1954/1638), p. 179.

[10]Kuhn, "Function of Measurement in Modern Physical Science," p. 42.

[11]Ibid., p. 43.

[12]Ibid.

[13]Admittedly, one can find inductivist-sounding claims in Galileo, as one can in Newton, Priestley, et al. See, for example,

Galileo Galilei "Letter to Madame Christina of Lorraine, Grand Duchess of Tuscany," in Stillman Drake, trans. and ed., Discoveries and Opinions of Galileo (Garden City: Anchor, 1957/1615), p. 175. Though the 'obviousness' of these discoveries has been disputed by Paul Feyerabend, who claims that Galileo had theoretical reasons for preferring the telescopic observation to naked eye observations, and some of his opponents' objections seemed empirically sound. See Paul K. Feyerabend, "Problems of Empiricism, Part II," in Robert G. Colodny, ed., The Nature and Function of Scientific Theories: Essays in Contemporary Science and Philosophy (Pittsburgh: University of Pittsburgh Press, 1970), pp. 281-89.

[14]See Feyerabend, op. cit., pp. 280-81.

[15]Galileo Galilei, Dialogues Concerning the Two Chief World Systems: Ptolemaic and Copernican, Stillman Drake, trans. (Berkeley: University of California Press, 1967/1632), p. 328.

[16]Ibid., p. 339.

[17]Glymour, op. cit., pp. 110-290.

[18]Ida Freund, The Study of Chemical Composition: An Account of Its Method and Historical Development (New York: Dover, 1968/1904). "It may be that two or more hypotheses will all stand the test of a first set of inferences; when this happens the two hypotheses must be tested further and further, until an inference is drawn, which according to the one, would give a result different and easily distinguished from that yielded by the other. An experiment, in this cases called a crucial experiment . . . , is called upon to decide," p. 18. Karl R. Popper, The Logic of Scientific Discovery (New York: Harper and Row, 1968/1934).

"I mean by a crucial experiment one that is designed to refute a theory (if possible) and more especially one which is designed to bring about a decision between two competing theories by refuting (at least) one of them--without, of course, proving the other," p. 277 (f.n.).

[19]Antoine Lavoisier, Elements of Chemistry: In a New Systematic Order Containing All the Modern Discoveries, Robert Kerr, trans. (Philadelphia: Mathew Carey, 1799/1789), pp. 81-85. See also Freund's discussion of this as a crucial experiment, The Study of Chemical Composition: An Account of Its Method and Historical Development, op. cit., pp. 46-56.

[20]One of the places where this case is discussed is in Carl G. Hempel, Philosophy of Natural Science (Englewood Cliffs: Prentice Hall, 1966), p. 26. A bit more detail on the experimental apparatus involved (and an interpretation of the experiment as being a crucial one) is provided in Francis Weston Sears, Optics (Reading: Addison-Wesley, 1958/1949), p. 13.

[21]See, for example, the passage in Hempel mentioned in the last footnote. Also, Morris R. Cohen and Ernest Nagel, An Introduction to Logic and Scientific Method (New York: Harcourt, Brace, and World, 1934), pp. 219-21, and Clark Glymour, op. cit., pp. 5-6, 39, 43-45, 48-55.

[22]Thomas Kuhn, The Structure of Scientific Revolutions (Chicago: University of Chicago Press, 1970/1962), p. 12.

[23]Ibid., p. 94.

[24]Max Planck, Scientific Autobiography and Other Papers (New York: Philosophical Library, 1949). "A new scientific truth does not

triumph by convincing the opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it," pp. 33-34. This is, in fact, Nollet's chief fear; not that established electricians would be seduced by Franklin's upstart theory, but that young, inexperienced investigators might be swayed. See J. L. Heilbron, Electricity in the 17th and 18th Centuries: A Study of Early Modern Physics (Berkeley: University of California Press, 1979), pp. 354-55.

[25]Mary Jo Nye, Molecular Reality: A Perspective on the Scientific Work of Jean Perrin (New York: Elsewer, 1972), pp. 143-45, 150-52, 156-57.

[26]Jean Baptiste Andre Dumas, Lecons sur la Philosophie Chimique (1878/1836). "Que l'on admette, si l'on veut, dans les gaz des groupes moléculaires ou des groupes atomiques en nombres égaux, à volume égal, on contentera tout le monde; mais on ne donnera rien d'utile à personne jusqu' à présent. Ce ne sera après tout qu'une hypothèse, et sur ce sujet on n'en a déjà que trop fait," p. 293. ("One can admit, if one likes, in gases of equal volume, equal numbers of molecular or atomic groups, one will (thereby) satisfy everyone; but one will not (thereby) provide anything useful at present. This would be, after all, only a hypothesis, and on this subject too many of these have already been made.")

[27]Stanislao Cannizzaro, "Sketch of a Course of Chemical Philosophy" (Edinburgh: Alembic Club Reprint #18, 1947/1858).

[28]Stephen Toulmin, "Crucial Experiments: Priestley and Lavoisier," Journal of the History of Ideas, vol. 18, no. 2 (1957).

[29]Ibid., p. 207.

[30]Ibid., p. 208.

[31]Priestley to Franklin, June 24, 1782, in Robert E. Schofield, ed., _A Scientific Autobiography of Joseph Priestley, 1733-1804: Selected Scientific Correspondence With Commentary_ (Cambridge: MIT Press, 1966), p. 209.

[32]Freund, _op. cit._, pp. 30-35.

[33]See again Lavoisier's account of his own experiment and Freund's discussion of it in the works cited in footnote 15.

[34]For an account of the identification of phlogiston with hydrogen, and the inherent difficulties in such an identification, see Freund, _op. cit._, pp. 35, 40-41. Two years later (1785), Priestley was to abandon this identification, but still retained his phlogiston explanation. See Toulmin, _op. cit._, p. 211.

[35]Toulmin, p. 210.

[36]Ibid. As Toulmin expresses it, Priestley takes the equation to be "Minium + Hydrogen → Lead," while current chemists would interpret it as "$Pb_3O_4 + 4H_2 \rightarrow 4H_2O$."

[37]Ibid., pp. 210-11.

[38]Joseph Priestley, _Experiments and Observations on Different Kinds of Air: And Other Branches of Natural Philosophy Connected with the Subject_ (Birmingham: Thomas Pearson, 1790), vol. III, p. 540.

[39]Ibid., "According to Stahl, phlogiston is a real substance, capable of being transferred from one body to another, its presence or absence making a remarkable difference in the properties of bodies, whether it add to their weight, or not," p. 540. Compare this with

Lavoisier's claims for 'caloric'. Antoine Lavoisier, Elements of Chemistry, op. cit., "During the calcination of mercury, air is decomposed, and the base of its respirable part is fixed and combined with the mercury, it follows from the principles already established, that caloric and light must be disengaged during the process," p. 86. Lavoisier includes both caloric and light as elements, and even though they are involved in the above chemical process, they do not contribute to weight gain or loss any more than phlogiston does for most phlogiston theorists!! See, also ibid., pp. 49-54.

[40] Priestley, Experiments and Observations on Different Kinds of Air: and Other Branches of Natural Philosophy Connected with the Subject (Birmingham: Thomas Pearson, 1790), vol. III, p. 543.

[41] Toulmin, op. cit., p. 212.

[42] See Ernest Nagel, The Structure of Science: Problems in the Logic of Scientific Explanation (New York: Harcourt, Brace, and World, 1961), chapter 11, pp. 336-97.

[43] See, for example, Thomas Kuhn, The Structure of Scientific Revolutions, op. cit., esp. pp. 101-102, and Paul K. Feyerabend, "Explanation, Reduction, and Empiricism" in Herbert Feigl and Grover Maxwell, eds., Scientific Explanation, Space, and Time (Minneapolis: University of Minnesota Press, 1971/1962).

[44] Larry Laudan, "Two Dogmas of Methodology," op. cit., p. 587.

[45] Ibid.

[46] Ibid., pp. 587-88.

[47] See Albert Einstein, Relativity: The Special and General Theory, Robert W. Lawson, trans. (New York: Crown, 1961/1916), pp. 44-48.

[48]"Two Dogmas of Methodology," p. 588.

[49]Ibid., pp. 589-91.

[50]Ibid., p. 591.

[51]See Larry Laudan, Progress and Its Problems, especially chapters 1 and 2.

[52]"A Confutation of Convergent Realism," op. cit.

[53]Ibid., pp. 20-21.

[54]Hilary Putnam, "What is Realism?" Proceedings of the Aristotelian Society, vol. 76 (1976), pp. 177-78.

[55]Ibid., p. 179.

[56]"A Confutation of Convergent Realism," p. 23.

[57]Ibid., p. 24.

[58]Laudan discusses a number of successful, nonreferential theories in ibid., pp. 26-27, and lists many others on p. 33.

[59]Ibid., p. 25.

[60]Ibid., p. 26.

[61]Ibid., p. 32.

[62]Ibid.

[63]Ibid., p. 33.

[64]Ibid.

[65]Ibid.

[66]Ibid., pp. 35-36.

[67]William Whewell, History of the Inductive Sciences, cited in The Study of Chemical Composition, op. cit., pp. 56-57.

[68]This example (again) will be discussed in more detail later. For now, see Roderick W. Home, "Franklin's Electrical Atmospheres,"

British Journal for the History of Science, vol. 6, no. 2 (1972).

[69]Benjamin Franklin, Letter to Peter Collinson, April 29, 1749, in I. Bernard Cohen, ed., Benjamin Franklin's Experiments: A New Edition of Franklin's Experiments and Observations on Electricity (Cambridge: Harvard University Press, 1944/1774), p. 199.

[70]See Mary Jo Nye, "The 19th Century Atomic Debates and the Dilemma of an 'Indifferent Hypothesis'," Studies in History and Philosophy of Science, vol. 7, no. 6 (1976), pp. 266-68.

[71]Clyde L. Hardin and Alexander Rosenberg, "In Defense of Convergent Realism," Philosophy of Science, vol. 49, no. 4 (1982).

[72]Ibid., pp. 604-605.

[73]Ibid., p. 607.

[74]Ibid., p. 606.

[75]See again, Hartry Field, "Theory Change and the Indeterminacy of Reference," Journal of Philosophy, vol. 70, no. 14 (1973) and my account of it at the end of the second chapter.

[76]See, for example, Marie Boas, "Structure of Matter and Chemical Theory in the 17th and 18th Centuries," in Marshal Claggett, ed., Critical Problems in the History of Science (Madison: University of Wisconsin Press, 1959), Henry Guerlac, "Quantification in Chemistry," in Woolf, op. cit., Richard Westfall, The Construction of Modern Science (Cambridge: Cambridge University Press, 1977/1971), pp. 65-81.

[77]Sir Isaac Newton, Optics (New York: Dover, 1979/1730), p. 400.

[78]Marie Boas, "Structure of Matter and Chemical Theory in the 17th and 18th Centuries," op. cit., p. 512.

[79]See, for example, Aaron J. Ihde, The Development of Modern

Chemistry (New York: Harper and Row, 1964), pp. 103-105, the very brief account given in Bernard Jaffe, Crucibles: The Story of Chemistry from Ancient Alchemy to Nuclear Fission (New York: Dover, 1976/1930), pp. 85-90, Gerald Holton and Duane H. D. Roller, Foundations of Modern Physical Science (Reading: Addison-Wesley, 1958), pp. 367-80.

[80] Ihde, op. cit., pp. 98-101.

[81] Ibid., p. 98.

[82] Ibid.

[83] Ibid., pp. 99-100.

[84] Ibid., p. 101.

[85] Freund, op. cit., "If, as did happen, Dalton saw the regularity where Proust did not, this is not strange . . . , considering the fundamental difference between their ways of looking at nature. Dalton, a theorist before everything, full of speculations concerning the hidden nature of phenomena, was the man from whom, with the material available, the discovery of such a regularity could be expected; Proust was not. And this is borne out by the view . . . that it was not the available data which led to an empirical law, but that in the empirical numbers Dalton found a verification of his theoretical speculations," p. 153.

[86] See, again, Kuhn, "The Function of Measurement in Modern Physical Science," op. cit.

[87] John Dalton (1805), Alembic Club Reprint #2, cited in Freund, p. 155.

[88] Ibid., p. 156.

[89] Ibid.

[90] Ibid., p. 157.

[91]Ihde, p. 109, see also Clark Glymour, *Theory and Evidence*, p. 226, and Freund, pp. 288-89.

[92]The following analysis is taken from Freund, p. 290.

[93]For another treatment of this analysis, see Holton and Roller, *op. cit.*, p. 383.

[94]See Michael Gardner, "Realism and Instrumentalism in 19th Century Atomism," *Philosophy of Science*, vol. 46, no. 1 (1979), esp. pp. 14-23.

[95]John Dalton, *New System of Chemical Philosophy* (1808) cited in Freund, p. 291.

[96]Dalton, of course, did not use this symbolism, but represented 'HO' as '⊙O', with '⊙' representing an atom of hydrogen, and 'O' one of oxygen. See Freund, pp. 292-93.

[97]Ibid.

[98]Ibid., p. 291.

[99]See Glymour, pp. 258-63, and Gardner, pp. 14-22.

[100]Ihde, pp. 116-18; Freund, pp. 302-307; Holton and Roller, pp. 391-92.

[101]Freund, pp. 303-305.

[102]Holton and Roller, pp. 378-80.

[103]John Dalton, *New System of Chemical Philosophy*, cited in Freund, p. 310.

[104]Ibid., "The truth is, I believe, that gases do not unite in equal or exact measures in any one substance; when they appear to do so, it is owing to the inaccuracy of our experiments. In no case perhaps is there a nearer approach to mathematical exactness, than in that of one

measure of oxygen to two of hydrogen; but here the most exact experiments I have ever made gave 1.97 hydrogen to 1 oxygen," p. 311.

[105]Ihde, p. 120.

[106]This would be the modern formula, '$2H + O_2 \rightarrow 2HO$'. '$2H_3 + O_2 \rightarrow 2H_3O$', '$2H + O_4 \rightarrow 2HO_2$', etc., would also agree with Gay-Lussac's law without splitting atoms. Put another way, distinguishing between molecules and atoms avoids the problem of splitting allegedly indivisible atoms, but does not, in itself, provide for determinate formulae, only innumerable ways to balance chemical equations. For a lengthy excerpt from Avogadro's original paper, see Freud, pp. 318-20.

[107]Ibid., p. 320.

[108]Ibid., p. 331.

[109]Holton and Roller, p. 399, and Ihde, pp. 124-33.

[110]Ihde, pp. 121, 131-33, Freund, pp. 334-35.

[111]Ihde, p. 121.

[112]Freund, pp. 545-53, Ihde, pp. 170-73.

[113]Glymour, pp. 229-39, Ihde, pp. 141-45.

[114]Glymour, p. 238.

[115]Ibid., p. 239. See also, Ihde, p. 144, Freund, pp. 171-86, Gardner, pp. 16-20, Nye, "The 19th Century Atomic Debates and the Dilemma of an 'Indifferent Hypothesis'," pp. 248-49.

[116]Ihde, pp. 149-53; Glymour, pp. 251-55; Freund, pp. 335-40; Gardner, pp. 19-20; Gerd Buchdahl, "Sources of Scepticism in Atomic Theory," British Journal for the Philosophy of Science, vol. 10, no. 38 (1959), pp. 125-27.

[117]Ihde, pp. 150-51.

[118]Freund provides a useful comparative table of Dumas' and Mittischlich's atomic weights contrasted with Berzelius' on p. 338.

[119]For Dumas' own presentation of these problems, 10 years after his original experiments when he had abandoned the atomic theory, see J. B. A. Dumas, Lecons sur la Philosophie Chimique, op. cit., chapter 7, esp. pp. 296-303.

[120]Freund, pp. 340-55, Ihde, pp. 204-208. Gerhardt, however, still retained Gmelin's 'equivalents' in 1853. Showing that the chemical community was not quite ready to completely return their allegiance to atomic weights.

[121]Michael Faraday, Experimental Researches in Electricity (New York: E. P. Hutton, 1912/1839-55), p. 162.

[122]See Ihde, pp. 133-39. "Faraday's electrochemical equivalents should have assisted the chemists of that day in solving their big problem--atomic weight values. However, chemists were far from agreement as to the reality of atoms, and . . . there was a great deal of confusion regarding atoms, equivalents, and molecules. Davy and Faraday had never found it necessary to believe in atoms, though Davy used equivalent weights. Faraday had never had to deal with chemical quantities until he encountered electrochemical equivalents, and had no interest in extending his work in the direction of atomic weight estimations," pp. 137-38.

[123]Stanislao Cannizzaro, "Sketch of a Course of Chemical Philosophy," op. cit.

[124]Ibid., p. 1. See also, Holton and Roller, pp. 400-402; Gardner, pp. 9, 20-21; Ihde, pp. 226-29; Freund, pp. 334-59; Glymour,

pp. 260-63.

[125]Cannizzaro, p. 12.

[126]See the table Cannizzaro provides on p. 9.

[127]Ibid., p. 11.

[128]Ibid., p. 12.

[129]Ibid., table on page 22.

[130]Ibid., table on page 28.

[131]Gardner, pp. 20-21, Glymour, p. 263.

[132]See Freund, pp. 322-29. James Clerk Maxwell, "On the Dynamical Evidence of the Molecular Constitution of Bodies," Scientific Papers of James Clerk Maxwell, vol. II (Cambridge: Oxford University Press, 1890), pp. 422-30.

[133]Nye, p. 252, Maxwell, pp. 430-33.

[134]Nye, p. 253, Maxwell, p. 433.

[135]Maxwell, ibid., and "Atom" in Scientific Papers of James Clerk Maxwell, esp. pp. 462-72.

[136]Nye, Molecular Reality, p. 143-45.

[137]See Nye, "The 19th Century Atomic Debates and the Dilemma of an 'Indifferent Hypothesis'," pp. 266-68 and Molecular Reality, pp. 156-57.

[138]See Glymour, p. 263.

[139]See Heilbron, Electricity in the 17th and 18th Centuries: A Study of Early Modern Physics, pp. 324-26.

[140]I. Bernard Cohen, Franklin and Newton: An Inquiry into Speculative Newtonian Experimental Science and Franklin's Work in Electricity as an Example Thereof (Philadelphia: American Philosophical Association,

1956), pp. 98-103 on Boyle's general corpuscular effluvial theory. See also Heilbron, pp. 202-205.

[141]Roderick W. Home, "Francis Hauksbee's Theory of Electricity," Archive for the History of the Exact Sciences, vol. 4, no. 3 (1967), pp. 204-206, Heilbron, pp. 232-33, 237-39.

[142]Francis Hauksbee, "An Account of an Experiment made before the Royal Society at Gresham College, touching the Extraordinary Elistricity (sic!) of Glass, producible on a smart Attrition of it, with a Continuation of Experiments on the Same Subject, and other Phenomena," Philosophical Transactions of the Royal Society, London, 25 (1706), cited in Home, p. 206.

[143]Ibid.

[144]Ibid., p. 207.

[145]Ibid., see also Heilbron, p. 234.

[146]Home, p. 209.

[147]Hauksbee, Philosophical Transactions of the Royal Society, 1708, cited in Home, p. 211.

[148]Home, p. 213.

[149]Hauksbee, Physico-Mechanical Experiments on Various Subjects, containing an Account of several Surprizing Phenomena touching Light and Electricity, producible on the Attrition of Bodies. With many other Remarkable Appearances, not before observ'd (London: R. Brugis, 1709), cited in Home, p. 214.

[150]Cohen, pp. 368-70, Heilbron, pp. 245-49.

[151]See Priestley, The History and Present State of Electricity: With Original Experiments, vol. II (New York: Johnson Reprint, 1966/

1767), p. 17.

[152] Cohen, p. 369.

[153] Heilbron, pp. 245-47.

[154] Cohen, p. 372, Heilbron, pp. 255-60.

[155] Cohen, p. 376.

[156] Ibid. See also, Priestley, p. 17.

[157] Heilbron, p. 265.

[158] Here is one reported by Heilbron. "Insulate a large dinner table and one of its chairs, which you will occupy; run a wire from a concealed prime conductor to within reach of the special seat; set the machine in motion, grasp the wire, touch the table, and watch your guests jump when the sparks fly from their forks," p. 267.

[159] Ibid., p. 269.

[160] Ibid., p. 275.

[161] Heilbron, p. 282.

[162] Ibid., pp. 282-83.

[163] Ibid., p. 283.

[164] Ibid., p. 284.

[165] Ibid. See also, Cohen, pp. 388-90, Priestley, pp. 22-23.

[166] Heilbron, p. 284.

[167] Ibid., pp. 285-86.

[168] Ibid., p. 287.

[169] Heilbron, p. 297, Cohen, p. 398.

[170] Heilbron, p. 298, Cohen, pp. 396-97.

[171] Heilbron, p. 299, Cohen, p. 403.

[172] Heilbron, p. 300, Cohen, p. 407.

[173] Heilbron, pp. 300-301.

[174] Ibid., p. 302.

[175] Ibid.

[176] Ibid., p. 303.

[177] Ibid., pp. 313-16, Cohen, pp. 385-86.

[178] Cohen, p. 385. Much of Musschenbroek's actual report is reprinted in Heilbron, pp. 313-14.

[179] Heilbron, p. 323.

[180] Letter to Peter Collinson, July 11, 1747, in I. Bernard Cohen, ed., _Benjamin Franklin's Experiments: A New Addition of Franklin's Experiments and Observations on Electricity_ (Cambridge: Harvard University Press, 1941/1774), pp. 174-76. See also, Cohen, pp. 438-39, Heilbron, pp. 328-29, E. T. Whittaker, _A History of the Theories of Aether and Electricity_ (New York: Philosophical Library, 1951/1910), p. 47, Priestley, vol. 1, pp. 194-95.

[181] Heilbron, p. 328.

[182] What follows is a paraphrase of Franklin's own account, _Benjamin Franklin's Experiments_, pp. 175-76.

[183] Ibid.

[184] Ibid., p. 176. Cohen, pp. 440-41.

[185] Cohen, p. 450, Heilbron, p. 330. See also, I. Bernard Cohen, "Conservation and the Concept of Electric Charge: An Aspect of Philosophy in Relation to Physics in the 19th Century," in Claggett, ed., _Critical Problems in the History of Science_, _op. cit._, pp. 357-83.

[186] Letter to Peter Collinson, Sept. 1, 1747, in _Benjamin Franklin's Experiments_, pp. 179-186. The following is a paraphrase of

parts of this letter. See also, Heilbron, pp. 330-34; Cohen, Franklin and Newton, pp. 452-63; Whittaker, pp. 46-50; Priestley, vol. 1, pp. 195-98.

[187]Letter to Collinson, Sept. 1, 1747, op. cit., pp. 182-86.

[188]Priestley, vol. 1, p. 199.

[189]Heilbron, pp. 334-35.

[190]"Opinions and Conjectures, Concerning the Properties and Effects of the electrical Matter, arising from Experiments and Observations made at Philadelphia, 1749, Paper to Peter Collinson," July 29, 1750, in Benjamin Franklin's Experiments, pp. 213-36. See also, Heilbron, pp. 334-43, Franklin and Newton, pp. 467-78, Roderick W. Home, "Franklin's Electrical Atmosphere's," British Journal for the History of Science, vol. 6, no. 22 (1972).

[191]Heilbron, p. 335.

[192]"Opinions and Conjectures," pp. 215-18, the experiment rendering atmospheres 'visible', p. 216.

[193]Ibid., pp. 227-36.

[194]Ibid., p. 227. Emphasis in original.

[195]Ibid., p. 228. See also, Franklin and Newton, pp. 473-76.

[196]Heilbron, p. 337.

[197]Ibid.

[198]Home, "Franklin's Electrical Atmospheres," pp. 135-37.

[199]Ibid.

[200]For a brief account of how the non-mathematical electricians of Franklin's day could not appreciate the bold step taken by Aepinas, see Roderick W. Home, "Aepinas and the British Electricians: The

Dissemination of a Scientific Theory," Isis, vol. 63, no. 217 (1972).

[201]Home, "Franklin's Electrical Atmospheres," pp. 140-42.

[202]Ibid., p. 145, Heilbron, pp. 352-62.

[203]John Canton, "Electrical Experiments, with an Attempt to Account for their several Phenomena: Together with Some Observations on the Positive and Negative electrical State of the Clouds," Dec. 6, 1753, in Benjamin Franklin's Experiments, pp. 293-99. See also, Heilbron, pp. 373-76; Franklin and Newton, pp. 516-24; Home, "Franklin's Electrical Atmospheres," pp. 142-43; Priestley, vol. 1, pp. 386-93. The following account is a paraphrase of Canton.

[204]Canton, "Electrical Experiments, with an Attempt to Account for their several Phenomena: Together with Some Observations on the Positive and Negative electrical State of the Clouds," p. 294.

[205]Ibid.

[206]Ibid., p. 295.

[207]Ibid.

[208]"Electrical Experiments made in Pursuance of those made by M. Canton, dated Dec. 6, 1753," March 14, 1755, in Benjamin Franklin's Experiments, pp. 302-306.

[209]Ibid., p. 302. See also, Heilbron, pp. 376-77; Franklin and Newton, pp. 526-28; Priestley, vol. 1, pp. 293-94.

[210]Benjamin Franklin's Experiments, p. 302.

[211]George Gamov, Biography of Physics (New York: Harper Torchbooks, 1961), pp. 128-30, Whittaker, pp. 57-60.

[212]Priestley, vol. 1, pp. 293-303, Whittaker, pp. 48-53.

[213]J. J. Thomson, Electricity and Matter (New York: Charles

Scribner's Sons, 1905), p. 6. See also, his later account of the atomic structure of electricity, and the easy translation he provides of Franklin's theory into his own, pp. 88-89.

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