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MODELS AND SCIENTIFIC REALISM

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ABSTRACT

The aim of this dissertation is to argue for a model-supported scientific realism. The use of physical models is a crucial factor in explicating different aspects of the controversy between realism and instrumentalism in science.

An analysis of the notion of a scientific explanation is offered. The main point of this analysis is that scientific theories explain only insofar as they provide intelligible interpretations of the mechanisms and processes associated with the theoretical structure of theories. Two features of models are distinguished. A model has both descriptive and formal aspects. The key to intelligible theories is shown to be the employment of models with descriptive aspects. These descriptive aspects are conceived of as being attributed to theoretical entities by analogy with antecedently understood, familiar characteristics of (ultimately) observable phenomena.

It is argued that theories are capable of being predictive although they may not be intelligible in the sense that the theoretical models may have a minimum of descriptive content. This leads to a consideration of the status of theoretical entities.

Several instrumentalistic views on the status of theoretical entities are discussed. These views are all rejected on the grounds that they neglect the function of

theories to provide intelligible descriptions of both observable and theoretical phenomena.

At this point, the controversy between realism and instrumentalism is recast in the light of an examination of Carnap's notion of alternative linguistic frameworks. The problem of delimiting the "real" is seen to be relative to the choice of a particular framework. Since frameworks are constituted in terms of certain aims and purposes, it is argued that what is counted as real would be relative to those aims and purposes. Two aims, predictivity and intelligibility, were singled out as being central to the realist-instrumentalist controversy. The instrumentalist is characterized as one who holds that predictivity is the central aim of scientific theorizing; the realist, as one who holds that both predictivity and intelligibility are central aims of scientific theorizing.

It is argued that the crucial move for maintaining a distinction between realism and instrumentalism is the distinction drawn between the concept of existence and the concept of reality. To say of scientific entities that they really exist, is to be able to provide a physical model which confers intelligibility onto those theoretical entities which our well-confirmed theories assert exist.

Some "realist" views of theoretical terms are examined and found wanting, chiefly because they fail to make a clear cut distinction between those theoretical elements which are real and those theoretical elements which are

only instrumental. I argue that the key to such a distinction is the availability and deployment of physical models.

Some difficulties with the model supported realism are pointed out and discussed with the result that the realist-instrumentalist controversy is seen to divide into three distinct issues. First, it is pointed out that all conceptual frameworks and all theories within the framework of science are only means instrumental to certain ends. This does not preclude us from making distinctions within these frameworks between real and instrumental theories. Secondly, it is argued that the progress of science is best characterized, in part, as a move to replace instrumental theories by realistic theories which are interpreted through intelligible models. Thirdly, the plausibility of distinguishing what is instrumental from what is real within the context of a well-confirmed realistic theory is left an open question with several possible solutions tentatively offered.

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CHAPTER I

Introduction

The role of models in science has been the subject of much contemporary discussion in the philosophy of science. One of the most influential and thought provoking recent discussions is provided by Mary Hesse in Models and Analogies in Science. ([44])[1] At the very outset, Hesse inquires

> If a scientific theory is to give an "explanation" of experimental data, is it necessary for the theory to be understood in terms of some model or some analogy with events or objects already familiar? Does "explanation" imply an account of the new and unfamiliar in terms of the familiar and intelligible, or does it involve only a correlation of data according to some other criteria, such as mathematical economy or elegance? ([44], p. 1)

The aim of this dissertation is to provide an answer to these questions. With respect to the first question, it is argued that scientific theories are explanatory only insofar as the theoretical entities or processes postulated by the theory are understood or interpreted in terms of some model. However, the point of view here adopted differs significantly from that of Hesse. Although she argues that models are necessary in order that a theory be explanatory, she argues that this is the case because otherwise the theory would not be strongly predictive. On the contrary, I argue that models are not at all necessary for a theory

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[1]References in square brackets refer to the bibliography at the end of the dissertation.

to be predictive, but that they are necessary if the theory is to provide us with any scientific understanding. The interrelationship between predictivity, understanding, and explanation in science is explored in the next chapter.

As for the second issue raised by Hesse, it is argued that theories are explanatory only insofar as they do provide "an account of the new and unfamiliar in terms of the familiar and intelligible." As it stands this answer is open to immediate objections, since it is clearly often the case that scientific explanations of what is relatively familiar, e.g., that bread nourishes the human body, are accounts in terms of the relatively unfamiliar, e.g., notions of atoms, molecules and physiochemical reactions. However, properly qualified, theories without intelligible models are not explanatory, although they may indeed be predictive. The exact nature of these qualifications is discussed in the next chapter.

The subsequent chapters are devoted to showing the significance of models for the predictivity of theories and for the interpretation of theoretical terms in science.

The problem of how to interpret theoretical terms in science leads us to consider the dispute between realist and instrumentalist interpretations of scientific theories. Basically, an instrumentalist is one who holds that theories, and theoretical entities, are only instruments for the prediction and classification of observational data. The realist is one who holds that scientific theories, or, at

least, well-confirmed scientific theories, are descriptive of reality, and that, therefore, the theoretical entities and processes are as real as any of the other basic "furniture of the earth."

The fundamental difference between instrumentalism and realism is seen to turn upon different conceptions of the aims and purposes of science. It is argued that the basic aim and purpose of science is to provide an intelligible understanding of the world. In this sense, and insofar as physical models can be employed in this task, the direction of this essay is distinctly realistic.

One note of caution must be voiced about the concept of a model. The recent literature on the subject has been immense. Only a fraction of this literature is cited in the bibliography at the end of the dissertation. The result of this discussion has been that instead of coming to a consensus on how the term is used, there has been a proliferation of different senses of the term. Thus we have normative models, model cities, model homes, mechanical models, conceptual models, analogue models, basic models, isomorphic models, theoretical models and so on. This proliferation has resulted in what Marx Wartofsky has called the "model muddle." ([90]) The concept of a physical model which is employed in this dissertation agrees most closely with the concept of a theoretical model as employed by Peter Achinstein ([1],[3]) and Henry Byerly ([13]). A physical model for a theory, as that notion is here employed,

is to be identified with the theoretical entities and processes which are postulated by the theory.

CHAPTER II

On Explaining

There are at least three related concepts which lie at the heart of the scientific enterprise, and are therefore the prime investigating targets for the philosopher of science. These three are "understanding," "explanation," and "prediction." A scientific theory, if it is to be counted a good one, must offer us some understanding of something or other, must explain something or other and must allow us to make predictions about what it is that we understand and have explained.

These notions are fundamental and interdependent. Some would claim that at least two (explanation and prediction) stand or fall together and that the third, insofar as it does not reduce to the others, reduces to nothing. Others would agree that understanding is a holdover from the old days, and best forgotten, but would want to carefully distinguish the other two. It seems to me that some light may be thrown on these concepts and their interrelationships through the use of another conceptual tool--the model. In the process, it is hoped that some light will be thrown on the scientific enterprise in general and more particularly on some of the issues which are confronting contemporary philosophers of science as they attempt to answer the basic questions which they are facing: what is science, what is it all about and what is it up to?

In this section I want to state a thesis about the role of models in scientific explanation and then, through a series of criticisms and defenses, refine the thesis to the point where the strategic role of models in science becomes clear.

(1) Thesis: There is a need for models in any attempt to give a scientific explanation. Because in giving an explanation we are trying to understand what is happening; we desire not only control over the phenomena (via predictability) but also comprehension. We want to make the phenomena intelligible to us.

(2) This view is rejected by those who either identify explanability with predictability <u>or</u> reject "explanation" in the above sense as unattainable. This latter view restricts scientific theorizing to the producing of useful predictors. Some grounds for rejecting (1) are the following.

(2a) The basic notions of "understanding," "comprehension" and "intelligible" are vague and ill-defined.

(2b) If we understand "intelligible" as equivalent to "what is familiar" our gain is slight because the notion of familiarity is also vague, or is at least as vague as "intelligible." To consider explanations as providing intelligibility by reducing the unfamiliar to the familiar is to relativize the notion of scientific explanation in at least two <u>prime facie</u> undesirable ways.

What would count as a scientific explanation of a

given phenomena would vary from individual to individual because what is familiar to Jones may not be familiar to Smith. Even if we eliminate reference to individuals by considering the familiar to be what is generally accepted by the scientific community, scientific explanations would still be culturally relative in the following sense. Smith and Jones, despite individual idiosyncratic differences, may share a common cultural heritage, yet this heritage may differ from that of two comparable individuals sharing a different heritage, or from a different culture entirely. Hence, an explanation for the one group would not in general count as an explanation for the other. Thus, to take a concrete example, if we identify explaining with reducing to the familiar, then we cannot say that Azande accounts of sickness and disease in terms of magic and witchcraft are any more or less scientific than more "modern" explanations in terms of molecular biology. Yet we want to say, it seems, that the molecular biologists are more or less right in what they say about disease, and the Azande are dead wrong. In the light of (1) it is hard to see how we can do this.

(2c) (1) is also rejected on the grounds that if we did explain the unfamiliar in terms of the familiar, then how do we account for the fact that we "explain" familiar phenomena in terms of relatively unfamiliar concepts? To take Hume's classic example of the nourishing power of bread, we take the familiar fact that bread nourishes human beings and explain it in terms of the relatively

unfamiliar concepts of atomic and chemical theory. Related to this point is the observation that what we consider to be familiar changes, and hence, what is unfamiliar at first, gradually becomes more and more familiar, and in the light of (1), less and less in need of explanation. A simple example of this is the postulate of special relativity theory, that the speed of light is constant with respect to all inertial frames. Relative to Newtonian mechanics, this postulate is very non-intuitive and unfamiliar. With the gradual acceptance of the special theory of relativity, the strangeness wears off, and the postulate becomes accepted as familiar. The unfamiliar postulates, entities and processes are eventually just accepted as familiar and we use them to explain other, as yet, unfamiliar phenomena.

(2d) A final point seems to be that (1) is taken as maintaining that what is familiar is not in need of any explanation, and this is precisely what is not the case. When science progresses it does so, in many cases, by providing explanations or accounts of what is most familiar to us, as when we seek to explain the transition of the seasons, climatic changes and other familiar phenomena such as the fact that what goes up usually comes down.

Having stated some criticisms of thesis (1) we may offer a defense of it. Taking the criticisms in reverse order to their presentation, the following can be said.

(3a) In (2d) it is suggested that (1) implies that what is familiar to us is not in need of any explanation,

and that this is manifestly false. It follows that thesis (1) is false. This criticism seems to me to neglect a fundamental distinction between our power to predict the course of phenomena and our ability to understand the phenomena. It might be objected that there is no such distinction and that our understanding of a phenomena is reflected fully in our ability to make successful predictions about the course of the phenomena. This view is simply false. There are many instances where we may be able to make accurate predictions and yet have absolutely no understanding of how or why these predictions are fulfilled. A simple example would be the prediction of tidal effects based on regularities which are discernible in tide tables. The mere fact that we are able to predict the future course of the tides through the use of such tables, which thereby gives us a certain amount of control over our environment, in no way engenders any understanding on our part. Now the tides are among the most familiar of phenomena, at least for those who live by the sea, and yet its familiarity does not preclude the fact that there are many things we may learn about it. The tables may reveal regularities undreamed of, and yet, noticing these regularities and using them to our advantage does not make either the tides or the regularities intelligible to us.

In short, predictivity does not entail intelligibility, nor does intelligibility (i.e., familiarity) entail predictivity. A phenomena may be intelligible to us (in

one important sense) because it is familiar and commonplace, and yet we may be able to make only cursory predictions about its behavior (e.g., the tides come in regularly twice a day). We certainly do want to be able to make better predictions about phenomena with which we are most familiar, and it is this fact that is captured in criticism (2d). However, this fact is not denied by defenders of thesis (1). All they deny is that, better (wider and more accurate) predictivity, ipso facto, means greater intelligibility. Understanding and intelligibility rest on our ability to account for our new found predictivity on the basis of a mechanism or process (a model) which can be interpreted (in a way to be specified below) in terms of concepts which are familiar to us.

(3b) In (2c), the criticism was that in explaining we often seem to replace the familiar by the obscure, as when, in the case of the nourishing of bread, we introduce relatively obscure notions of atoms and chemical reactions. This is exemplified in more recent times by the enormously unintuitive complications which seem to be introduced into physical theory by quantum theorists. This objection, I maintain, also conflates understanding with predictivity and may be met in the following way. It may be true that the formalism we introduce in order to explain some phenomena P is, initially, at least, strange and unfamiliar. However, the formalism itself, that is the laws and equations, serves merely as a predictive tool. If we cannot interpret the

terms of the formalism using notions with which we are familiar, then, despite our increase in predictive power, we cannot really say that we have made the phenomena any more intelligible than they were originally. Similarly, being able to use the formalism does not in any way make the how or why of the processes involved in the phenomena any more intelligible either. To make the discussion more concrete, consider the tried but true example of the explanation of the behavior of gases in terms of the atomic-molecular theory. What we do when we consider a gas as composed of molecules in motion is to offer a redescription of the phenomena in terms of a different set of concepts. Thus, we account for the behavior of a gas, in particular, say, the interdependence of the pressure, volume and temperature (macroscopic variables) in terms of the mechanical behavior of small particles (molecules) in constant motion. We redescribe the gas as a collection of molecules. In this case, the redescription is in terms of micro-entities, and it is certainly true, I think, that many scientific explanations involve the redescription of phenomena originally described in macroscopic terms through the use of such micro-entities. The reduction of the macro to the micro level is not, however, what I consider to be essentially explanatory in this instance. I do think that ultimately explanations do involve redescribing macroscopic phenomena in terms of micro or submicro phenomena (or in general, the phenomena at one level are redescribed and explained in terms of phenomena at

a lower level) but I do not wish to argue that thesis here. What is crucial to the explanatory process as I wish to characterize it is the notion of redescription. Every explanation is at least a redescription. Whether it is a redescription in terms of some preferred set of concepts is another question. Now, by a redescription I do not merely mean the replacement of one set of equations (relating macro-variables, e.g., PV = nRT) by a set of equations relating microvariables. Both, as such, are at best predictive tools. In order to make the phenomena intelligible to us we must provide some model, in this case, in terms of micro-entities and micro-processes. The equations themselves do not provide the requisite model, since they lack descriptive content. In other words, we want not only to replace one set of equations by another, but also provide, by imputation, some descriptive content to the phenomena which is formally described by the equations.

In this case, at least to a first approximation, the model for molecular behavior is based on <u>analogies</u> drawn from the behavior of macroscopic billiard ball motion (or for that matter, any other similar phenomena, bouncing basketballs, ping-pong balls, pebbles, or what have you).

The explanation of gas phenomena in terms of a molecular model, and the ultimate intelligibility of the model, rests on the fact that the molecular model is patterned after familiar, observable, macro-phenomena. What is familiar to us is not any of the particular analogues as such,

but rather what is exemplified by all of them, e.g., "rebounding," "transfer of momentum," etc. Thus, we are not saying that molecules are tiny billiard balls (or tiny ping-pong balls for that matter) but that they are analogous to billiard balls or the like. Here (and hopefully everywhere throughout) I am using the term "model" to designate the purported theoretical entities or processes, and the term "analogue" to designate the familiar entities or processes in terms of which we try to understand the unfamiliar. The basic idea is that we are able to extend our understanding and make what is strange, intelligible, only insofar as we can extend our familiar concepts to unfamiliar (and, perhaps, unobservable) areas. We extend by analogy, and where our analogies bear no fruit, the phenomena remain unintelligible, although not necessarily unpredictable.

The reduction of the unfamiliar to the familiar consists in extending, by analogy, descriptive predicates from the ordinary familiar everyday world to the extraordinary, unobserved (or unobservable) world. As we delve further into the unknown, into the realm of the very small, or for that matter, the very large, we may formulate theories which require models which seem to have no observable counterparts. But, insofar as the model and, hence, the phenomena is to be intelligible to us, we must employ descriptions in terms of concepts or constructs which are ultimately derived from those with which we are most familiar, i.e., those we use to describe what is observable. The end results, e.g.,

models of the very small may not be exemplified in the form they take at the sub-micro level at any other level. Yet they will be analogous to phenomena at larger levels, and these in turn will be analogous to phenomena at the everyday level. To put the point the other way around, as we construct a molecular model analogous to, say, billiard balls and their behavior, gradually the molecular model becomes part of what we consider the familiar. Then when we go on to describe sub-molecular behavior, we may construct models of sub-molecular phenomena by using molecules as analogues. These sub-molecular models will, in general, differ from molecular models in much the same sense, although not necessarily in the same respects, as the molecular model differs from its observable billiard ball analogue. One result would be that the more models we construct in this manner, the greater the likelihood that the next model will be unlike the original observable analogues from which we started. We may conceive this process to result in a series of models beginning from some observable phenomena to serve as the initial analogue and proceeding, O_1, M_1, M_2, M_3, . . ., M_n, . . ., in such a way that some features of other observable analogues O_i, which were not used in constructing M_1, may be used to construct some model M_i. The result may well be that for some models in this sequence there is no <u>one</u> observable analogue from which it is directly descended. Thus, if one were to directly compare such a model, say M_j, with any observable analogue O_j, the former would seem to

be strange and unfamiliar; this would hold for any given observable state of affairs. The appearance is that our theories (theoretical entities and processes) are becoming stranger and more unfamiliar, and that we are explaining or accounting for the familiar in terms of the unfamiliar. This, however, would be an illusion. In fact, M_j would be intelligible only insofar as its descriptive content could be shown to derive, perhaps piecemeal, from more familiar (and ultimately from observable) phenomena. (See [94], [95])

Criticism (2c) is rejected on the following grounds. While it is true that the predictive apparatus we employ in developing a theory T about phenomena P may be strange and unfamiliar, that predictive apparatus alone does not provide an explanation of P, i.e., does not make P intelligible to us. P becomes intelligible to us only if we can provide in conjunction with T, a descriptive model M for P which employs concepts which are analogous extensions of concepts employed in describing observable phenomena.

Implicit in this defense of thesis (1) is the assumption that to explain some phenomena P is basically to offer a redescription of P in terms of intelligible concepts (usually, although not necessarily, macrophenomena are explained (redescribed) by means of models of microphenomenal behavior). We extend the range of concepts from what is observed to what is unobserved or unobservable by arguments by analogy. In at least one sense, then, analogies and models are indispensable for theoretical understanding, for

without analogies and models the phenomena P remains unintelligible to us, because we would not be able to connect up our theoretical account with what is familiar to us.

(3c) It should be clear by now that thesis (1) is to be defended as entailing the indispensability of models and analogies in scientific explanations. It may be possible to defend thesis (1) without this entailment but I do not see how. Henceforth, then, we may assume that the defender of thesis (1) is a modellist of some sort, where we take as a model something which has both formal (or logical) and material (or descriptive) content. ([13], pp. 135-44) Furthermore, the descriptive content of our models is such that it is attributed via arguments by analogy from more familiar (and ultimately, observable) phenomena.

In contrast, those who oppose thesis (1), I will call formalists, since their basic premise seems to be that the essential characteristic of a scientific theory is its predictive power. If we are able to associate an intelligible model of the phenomena with the theory so much the better, but if we are not so able, so much the worse for intelligibility.

In the light of this, what can we make of the charge that thesis (1) engenders a certain undesirable relativity into what constitutes a scientific explanation? The formalist, it seems, would want to say that molecular biology offers better explanations of disease than those offered by Azande witchcraft. There must be some criteria whereby we

judge that the one is, in fact, a better theory than the other. Otherwise we seem to be left with the relativistic view that either is acceptable, and that there can be no comparison between them. The Azande view is just dead wrong. But how is the formalist to argue this point? Since he eschews intelligibility in terms of models, he cannot, it seems, argue that one model is truer than another. He must distinguish the two theories on formal grounds alone. Thus, he may argue that we reject Azande witchcraft as an explanation of disease because it does not offer us predictivity, or at best it offers us a very limited predictivity. Molecular biology provides a much wider scope of predictivity than the Azande magic view, and, in fact, solves more problems than the latter. Hence, molecular biology is judged to offer a more scientific account and a more scientific explanation of disease because it is a better predictor.

But, here the question arises as to what constitutes a legitimate scientific problem. Within its own sphere, Azande witchcraft works tolerably well. It provides an intelligible account of the situations it is intended to cover, and the questions that arise from the molecular-biological view do not arise for the Azande.

Given these preliminaries the following dialogue might ensue.

Formalist: Your last point emphasizes what I have been saying all along. The notion of intelligibility in terms of the familiar must be given up. The justification

of a theory must be in terms of its predictive power, not in terms of the possibility of finding an intelligible model for it.

Modelist: Then I must point out that the same relativity you attribute to us follows from your own view. If all that matters is formal structure and predictive power, it can easily be shown that the terms of your theory can be interpreted by an infinite number of distinct models. The significance of what the theory <u>says</u> about the world is clouded, and one has no reason to choose one model over another as representing what the theory says. Relativity all over again.

Formalist: I could not agree more. Everything you have said reinforces my conviction that there is no essential point to talking about models at all. We can do perfectly well by sticking to the formalism and the predictivity of the theory. What the theory <u>says</u>, in essence, is determined and completely circumscribed by the observable predictions that can be generated.

Modelist: But you must employ some models, or some interpretation, otherwise the symbols in your theory remain uninterpreted and, hence, useless for prediction, since we must be able to interpret initial data which is observational, in terms of the theory, in order to be able to use the theory for prediction.

Formalist: Yes, but the models and interpretations we employ are arbitrary. Any model or interpretation that

works and gives good predictive results is acceptable. The justifying of the theory is in the predicting.

Modelist: But the point of having theories is not only to predict, but to explain, and these functions are just not identical as you seem to insist. Look, on your view, if there existed an oracle which, on given an amount of initial data, would come up with predictions about anything under the sun, to any degree of precision required, then we would have to say that by going to the oracle we were not only able to come up with accurate predictions but that we also had explained the phenomena in question. That is simply an absurd view.

Put another way, a theory must contain both descriptive and formal content. Even if predictivity is reduced to the formal condition of deducibility from the theory, the theory will not be explanatory unless the theoretical concepts have some descriptive content as well.

Formalist: First, let me say that your example of the oracle is unfair. Clearly, there is no such being, although if there were I would admit that no intelligibility is conferred on the phenomena by his predictive powers. But, then, so much for the notion of intelligibility. And as far as it goes, you have not shown that the use of models is essential in any way for a theory. If you insist on making a distinction between descriptive and formal properties, I fail to see how you escape the basic charge of relativism which is at issue. In your attempt to specify some essential

descriptive nature to theoretical terms by falling back on arguments from analogy, you are forced to rely on the notion of "reduction to the familiar" which does not avoid the charge of relativism. For me, it is true, theories are relative, but only in the innocuous sense that formalisms may have alternative models. But nothing seems to be gained by opting for any one model over another, hence, they are nonessential and superfluous.

Modelist: Since you have given up the notion of intelligibility, or descriptive content for its own sake, there remains nothing to be said except that you have also given up a classical sense of explaining X which is taken to be, among other things, making X intelligible. It remains to be seen whether the claim that theories without intelligible models can be said to be strongly predictive (in a sense to be defined in Chapter III, Models and Scientific Predictivity).

Formalist: I don't deny that the theory must be interpreted, at least partially, but the interpretation of the so-called theoretical terms is arbitrary. To demand that there be some analogy between the theoretical concepts and some familiar or observable phenomena as a condition of our confidence in the theory is just pointless.

It seems pointless at this stage to continue the dialogue. The modelist, in his attempt to answer the formalist charge that there is an undesirable relativity introduced into science by the attempt to explicate "explanation"

in terms of "intelligibility" has come to an impasse. The impasse is reached by the failure of the two parties to agree to the importance of a distinction basic to the modelist position. This is the distinction between descriptive and formal content. What the formalist does is deny that any particular descriptive content is essential to the theoretical concepts of a given theory. The whole content of the theoretical terms is given by the formal content of the terms. The distinction between descriptive and formal content is basically the same distinction that is drawn between semantic and syntactic "content." The formalist is not saying that the content of theoretical terms is completely specified by syntactical rules of the calculus in which the theory is written. Actually, in order to make the theory empirical, i.e., relevant to our experiences, there must be some links with observable phenomena. This is provided by linking the theoretical terms of the theory via so-called correspondence rules with certain other terms, the so-called observational terms, which do have descriptive content. The descriptive content of observation terms is supplied by semantic rules which confer some ostensive or operational meaning onto the observation terms. The theoretical terms under such a system are said to be partially interpreted via the correspondence rules. They are not directly linked to any experience by any semantic rules.

The modelist, in insisting that theoretical terms also have descriptive content, is saying, in effect, that there be semantic rules for theoretical terms. Since the theoretical terms, for the most part, designate unobserved or unobservable entities or processes, there is no way that we may directly specify semantic rules for these terms. We must attribute descriptive properties to them, if at all, by analogy. The ascribing of descriptive properties to theoretical terms in order to confer intelligibility onto the theory is done by extending, through analogies, properties which are observable to entities which are (perhaps in principle) unobservable. ([13], pp. 140ff.)

There are two theses concerning the relation of the descriptive and formal content of theoretical terms which the modelist may wish to advance against the formalist. We may call them the Predictive thesis and the Intelligibility thesis respectively.

Predictive thesis: Unless the theoretical terms of a theory have descriptive content via analogical semantic rules, then the theory will fail to be strongly predictive in a sense that needs to be specified further. This is basically the thesis maintained by Hesse and others that models are essential for the predictivity of theories. The importance and centrality of the thesis for the modelist position is such as to warrant the discussion of the thesis a separate chapter (Chapter III).

Intelligibility thesis: This thesis does not claim

that models and analogies are essential in order for a theory to be predictive. It merely states that models based on analogies drawn from familiar phenomena are essential in making the theory intelligible. This intelligibility is essential to the explanatory function of theorizing. It cannot be done away with by being identified with predictivity. To explain is not only to predict but to make intelligible, and theories which have uninterpreted or partially interpreted theoretical terms are unintelligible. The balance of this chapter will be concerned, more or less, with this second thesis.

Let us return to the main argument, which at this stage is the modelist attempt to defend thesis (1) against criticism (2b). In the light of the objections by the formalist, the modelist argues that scientific explanations <u>are</u> relative but that this, in itself, is not a bad thing. An explanation, after all, consists in redescribing the unfamiliar in terms of the familiar and models constructed by analogies with familiar phenomena provide such a means. If, to return to the case of disease, witchcraft and magic is more familiar to the Azande than molecular biology, then theories constructed in terms of the former will be explanatory while theories constructed in terms of the latter will not.

Predictivity, on the other hand, is something else again. It would be granted that molecular biology is a better predictor than Azande witchcraft. Given a familiarity

with both models, Azande witchcraft and molecular biology, the molecular theory would be preferable because of its better predictivity. But without some intelligible model, the molecular theory would be merely a convenient tool for calculation and not at all explanatory.

At this point the formalist reopens the dialogue. We see, he would claim, that imputation of models not only leads us to accept as scientific, theories which are possibly noninformative (i.e., nonpredictive) but the imputation of models is also a possible hinderance to the advance of science since we may be satisfied with models which are intelligible to us (i.e., perhaps a witchcraft model) yet which are fundamentally nonpredictive. Thus, the insistence on intelligible models that we may visualize or depict in terms of familiar concepts is a reactionary viewpoint which very likely will lead to the stultification of scientific imagination--exactly the opposite of what we want to achieve.

Now it is always open to the modelist to counter by challenging the importance of predictivity for scientific theories. If it came to a showdown between predictivity and intelligibility, then the modelist may always opt for intelligibility. No doubt some philosophers may argue this way, but only at the price of giving up science. I do not want to argue that point of view. The modelism I wish to defend would not deny the essential importance of predictivity for a scientific theory, it would only deny that

predictivity is all there is to scientific theories. It would argue that models are an essential and valid element in the scientific enterprise. The first thing to be said against the latest formalist criticism is that there is an ambiguity lurking in the concept "informative." The formalist explicitly identifies "informative" with "predictive" whereas the modelist would want to argue that "informative" also encompasses "intelligible." Thus, for the modelist, a theory which does not provide descriptive content for theoretical terms is at best only partially informative.

As to the conservative nature of model building, one can only answer that models which are intelligible, but nonpredictive, will in the course of the conflict of ideas, die out and be superseded by intelligible models with greater predictive power. It is not at all clear how this struggle ought to be characterized, but I think it can be understood from a realistic point of view (a view I will develop more fully in Chapters IV and VII) about our scientific theories. Although all theories are, in a sense, instrumental towards certain ends, it is possible to distinguish between those theories which are merely instrumental and those which can be interpreted realistically, on the grounds that the former afford us predictivity alone, while the latter provide both predictivity and intelligibility. It remains to be seen whether any rational grounds

can be given for this belief.[1]

(3d) It remains to rebut (2a) which criticized thesis (1) on the grounds that the concepts of "understanding" and "intelligible" are vague and ill-defined. The standard contemporary move by the formalists to deal with concepts they consider vague and ill-defined, is to replace them by "rational reconstructions" or "explications" which are more precise than the original. The explication of a term singles out one of more paradigmatic aspects implicit in the original and redefines the imprecise original so as to include the reconstructed part(s). The formalist move with respect to the concepts of "understanding" and "intelligibility" is to ignore them or deny that they are central to the notion of explanatory understanding in the sciences.

In the first place, insofar as intelligibility and prediction are distinct functions of inquiry, no satisfactory explication of "explanatory understanding" is achieved by

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[1]No one, I think, would argue that the sole criterion for deciding on the acceptability of a theory T is its predictivity. Other relevant factors are the general level of scientific knowledge, factors of simplicity and comprehensiveness, the availability of alternative theories, etc. It is clear that these are not purely formal notions, i.e., that they cannot be analyzed and explicated simply from the point of view of the structure of a theory. These issues are important and merit attention. However, in this thesis I am concerned mainly with the explanatory role of physical models in science, and from this point of view, the difference between the formalist position and the modelist position is best seen in terms of how the predictivity aspect of theories is viewed. This distinction between formalists and modelists parallels, to a degree, the distinction drawn below between instrumentalism and realism.

replacing it by predictivity alone. To do so is to completely ignore the aspect of intelligibility implicit in the notion of an explanation. As such the formalist reconstruction does such violence to the original that the proposed explication is just downright unacceptable. Such violence in itself is not necessarily grounds for rejecting a proposed explication. It might be tragic but inevitable, were there no alternative explication available. But, in this case there is such an alternative.

In the second place, then, we have an alternative explication available which does do justice to the distinct functions of the original and which comes much closer to capturing the original meaning associated with the phrase "explanatory understanding." This alternative is the view, partially developed, that theories are modelled and that two distinct features of models are apparent. These two features are, on the one hand, the formal or logical content, and on the other hand, their descriptive content. The formal aspects of models and their formal relations begin to adequately explicate the notion of predictivity. The material or descriptive aspects of models begin to explicate quite adequately the notion of intelligibility necessary for an adequate explanation. In the light of this viable alternative, objection (2a) is rejected.

The net result is that the basic truth of thesis (1) with important qualifications is maintained. It remains to spell out these qualifications with respect to the notion of

predictivity and intelligibility in order that we may firmly establish the relevance of models to scientific theorizing.

CHAPTER III

Models and Scientific Predictivity

The program of this chapter is to investigate the claims that have been made by various contemporary philosophers to the effect that a theory which does not have a model in some sense does not have the predictive power of a theory which does have a model. We will examine then a series of related theses on this issue to determine (1) what sense of "model" is employed and (2) whether or not the thesis is sound. Along the way I hope to be able to reinfoce some of our other findings on the role of intelligibility for scientific theories and the relation it bears to the predictive function of theories.

1. Mary Hesse is a staunch advocate of the view that models are essential for the predictivity of theories. In her book, Models and Analogies in Science, she distinguishes between three levels of predictivity. ([44], Ch. I) These are associated with three kinds of theory, which she labels G, A, and B.

The weakest kind of theory is the G-type. A G-theory is little more than a correlation of data in some formula. Its sole predictive power lies in its ability to predict that the observational results which it collates will, given the identical experimental circumstances, be re-observed. The limitation of such theories, although they hardly deserve the name, is that they do not "point"

beyond the initial set of data upon which they are based. As Hesse says, a theory must do more than predict that the same observation statements that have been confirmed in the past will, in sufficiently similar circumstances, be confirmed in the future. ([44], p. 36) The limiting characteristic of such theories is that they give us no predictive ability with respect to either new relations between the variables of the original correlation or new relations between new variables. A comprehensive scientific theory could not be based on G-type theories.

The two types A and B represent more interesting and more predictively fruitful theory types which allow for the prediction of new testable observations. A-type theories allow predictivity in a weak sense, in which only new correlations involving the original observational predicates are predictable. B-type theories allow predictivity in a strong sense, in which new correlations involving new observational predicates as well as the original observational predicates are predictable. The following notation may help to make the distinction between these two theory types clearer.

Let: Observational Predicates: $O_1, \ldots, O_j$

$P_1, \ldots, P_k$

Theoretical Predicates: $T_1, \ldots, T_m$

$\{S_i\}$ = set of observation sentences containing either O's or P's or both which are accepted as true.

Consider a theory T, initially designed to account for observation reports containing only the O's. Call R the observational consequence class of T, i.e., $R = \{S_j \mid S_j$ is deducible from T and appropriate other conditions, and only O's occur in the S_j's$\}$.

There are two possibilities for the sentences of T:

case (1) A-type theory: no P's occur in any sentence of T

case (2) B-type theory: some P's occur in some sentences of T.

Consider A-type theories (case 1) first. In this case, none of the S_i's which contain any P's will be deducible from T. Hence, T will not enable us to predict any observational consequences concerning the P's. There are two kinds of prediction possible.

(a) Formal tests of theory: These would consist merely in establishing the scope of the experimental laws which follow from the theory T. Thus, suppose Charles' Law was initially established on the basis of observations made in a certain temperature range, $t_0 < t < t_1$. Then the theory, in the absence of other considerations, would predict that the same law should hold outside the range, i.e., for $t < t_0$ or $t > t_1$. The testing of Charles' law under such conditions constitutes a weak predictive test of the theory. Similarly, testing the law within the range $t_0 - t_1$, but at a $\underline{t}$ which had never before been experimentally examined would constitute a similar test.

The difference between this case and the G-type predictivity discussed above is simply that, in the former case, the generalization (Charles' law) is conceived of as restricted to the finite set of observations which formed its experimental basis. Thus, if $p \propto t$ were established on the basis of a finite set of pressure-temperature observations ($p_1 t_1 \ldots p_n t_n$) then a G-type prediction would be of the form, given some p_i of this set, in the future, expect t_i.

(b) In addition to the formal tests, the theory might predict a hitherto unknown or unnoticed observational correlation between the O's. This would come about through the combining of two experimental laws hitherto distinct. Let the set of relevant O's be "p," "v," and "t," the variables for the thermodynamic gas laws. If, for example, Charles' law states that $p \propto t$, and Boyle's law states that $p \propto 1/v$, then we are led to formulate the general gas law, pv/t = constant. From this we may derive what, we suppose for the example, had been an undiscovered correlation between v and t, i.e., that at constant pressure, $v \propto t$. This could then be tested and added to R.

Case (2): In this case, some of the S_i's which contain P's may be deducible from the theory T. (We consider only this possibility. The other possibility that, although there are sentences of T which contain P's, there are no observational consequences containing P's which can be deduced from T, is of no interest to us here.)

Hence, T will be able to predict observational consequences containing the P's. This will amount to an extension of the original theory, insofar as it was designed initially, by hypothesis, to account only for S_i's containing only O's.

The question at this point is how the sentences containing P's (P-statements) were introduced into the theory T. The theory was originally designed to account only for observation reports containing the O's. What leads us, in other words, to introduce sentences with P's into the theory? The P-statements, in effect, are correspondence rules that connect some of the T_i's with the P_k's. Since the theory was originally designed to account for observation reports described only by the O's, it would seem that correspondence rules for the P's would not be necessary. Hesse considers three possibilities to account for the introduction of these P-statements into the theory. ([44], p. 39)

(1) The P-statements are introduced because of observed relationships between the P's. This possibility is rejected since the theory originally only accounted for the O's. The situation is as follows. We accept the standard analysis of a theory as an interpreted calculus. Consider a simple observation sentence S_j correlating two O-predicates, O_1 and O_2. Call this sentence $S(O_1,O_2)$. This sentence is now correlated to a sentence $S(m_1,m_2)$ which is deduced directly from T. "m_1" and "m_2" are symbols of the calculus

for T which are to be identified via semantic rules with O_1 and O_2, respectively. The theory T will consist of sentences which relate T-predicates with the m's. One such sentence may be $S(T_1,m_1)$. The semantic rules which identify M_1 with O_1 will induce a transformation on $S(T_1,m_1)$ yielding $S(T_1,O_1)$. This sentence will be a correspondence rule of the theory which partially interprets T_1 in terms of O_1.

In the case under consideration here, we are examining sentences of the theory of the form $S(T_i,P_k)$ which are the interpreted forms of sentences (of the calculus) of the form $S(T_i,m_k)$. We are, in effect, asking for the source of the transformation $\mathcal{T}_p$: $S(T_i,m_k) \longrightarrow S(T_i,P_k)$. According to the conditions of the problem, there are no sentences of the form $S(P_k)$ which serve (initially) as part of the observational base of the theory. Hence, $\mathcal{T}_p$ cannot be induced as in the case of the O's.

(2) The second possibility is that the P-statements are introduced arbitrarily. This possibility is rejected by Hesse because "such a theory could not be taken seriously as a predictive theory." ([44], p. 39) Now clearly Hesse is not arguing that we could extend a theory T by some arbitrary transformation $\mathcal{T}$, or by what amounts to the same thing, the arbitrary introduction of new correspondence rules into the theory linking up the T-predicates with sentences containing P-predicates. She is arguing that such a new theory would not be scientifically acceptable. Why not?

Suppose one were to make such an arbitrary extension, grind out some predictions and test them. If the predictions turn out to be verified, such a case would be fortuitous indeed, but what is to prevent us from accepting this new theory as scientifically respectable? What Hesse claims is that, in such a situation, one has no reason for preferring one $\mathcal{T}$ over another. This is true, it may be admitted, but so what? Choose a $\mathcal{T}$ by whatever means come to mind; if it leads to successful predictions fine, if it does not, discard it and choose another $\mathcal{T}$, and so on. Hesse claims that science does not usually make theoretical advances in this manner. Thus, admitting arbitrary extensions would provide an inadequate historical account of the genesis of theories. This is, of course, a highly controversial issue. Many philosophers of science, especially the Popperians, would argue that the shotgun approach is the best method for generating scientific progress. It also appears that Hesse may be conflating two important but distinct issues--a historical issue concerning the actual genesis of a theory, with a logical issue concerning the justification of a theory (or theory extension) after the fact.

Some light may be thrown on this question by considering what it might mean to be a reason for the introduction of new correspondence rules into the theory. We may do this in a quite general way by distinguishing reasons which explain from reasons which justify. If someone asks Jones why he killed his neighbor, Jones may give two different

sorts of replies. He may say "I killed him because he annoyed me" or he may say "I killed him because he was an evil man" or he may say both. In the first instance, he has given a reason that explains his action by indicating what led him to commit his act. In the second instance, he has given (or has attempted to give) a reason which justifies his act, in an attempt to rationalize his behavior.

In our particular case, to say that a correspondence rule was arbitrarily added to a theory would be the same, it seems, as saying that there was no acceptable explanatory reason for doing so. No one, in other words, would be motivated to add one rule rather than another. This is what Hesse may mean when she says that "there would be no reason why any particular statement should be introduced rather than any other . . . ." ([44], p. 39) On the other hand, she may be (and most probably is) claiming that there is no justification for introducing one rule rather than any other. Here, her critic replies that "the proof of the pudding is in the eating." If a particular transformation $\mathcal{T}$ leads to predictions which are then verified, that is justification enough for introducing $\mathcal{T}$ into the theory. To this Hesse replies that there must be some prior justification for introducing one $\mathcal{T}$ over another. If there is a model for the theory, then the model does provide some prior justification for the introduction of some particular $\mathcal{T}$. The model also explains why we choose some one $\mathcal{T}$ over another. We do so because the model "naturally"

suggests extensions in some directions rather than in others. The function of the model as motivation to change the theory in certain ways is, of course, distinct from the model's function as justifying such a move.

(3) The third possibility, then, is that the P-statements are introduced on the basis of some model or analogies. To give an indication of what is at stake here, it would be helpful to think of the development of the molecular theory of gases. We assume that gases are composed of small particles called molecules, which are in constant motion. These small particles are conceived of as analogous to little billiard balls in motion. If we make the limiting assumption that the molecules are massive point-particles (radius = 0) we can derive, with the help of analogies between particle dynamical pressure and volume and the phenomenological measures of the pressure and volume of a gas, the ideal gas law, pv = nRt. Experimentally, it is found that actual gases only obey this law approximately and only under certain conditions (of relatively high temperatures and low pressures). On the basis of our molecular model this is readily understandable. At high temperatures and low pressures, the particles of the gas are relatively far apart and do not interact appreciably with one another. At low temperatures (or high pressures), other factors being equal, the volume of the gas is less. This means that the particles of the gas are "cramped" together. If we turn to our analogue, we see that it would

correspond to our billiard balls colliding together in a smaller space. Now it is clear, from the macroscopic analogue, that the smaller the space, the greater the interference to the motion of any given billiard ball due to the presence of the other billiard balls. Under these conditions, so many collisions with the walls will take place per second, etc., and we should expect the relations between the motion of these balls and the macroscopic properties of pressure and average velocity to be affected. This suggests that something similar is taking place in the case of our theoretical particles. Our original supposition was that the particles were dimensionless. If they are to be closer analogues to billiard balls, it would be more reasonable to assume that they had some dimension. When we make this correction, we arrive at the van der Waal's equation which approximates the experimental situation much better than the ideal gas laws. If we, in addition, assume that the particles attract one another (that they are electrical in nature, etc.), then the agreement gets still better.

Having the theoretical model for molecular motion based on an analogy with macroscopic particle behavior, we are thus naturally <u>motivated</u> to extend the theory in a certain way, by incorporating factors into the theory which designate the finite radii of the molecules. We may see how the model provides <u>prior justification</u> for this move in the following way.

Clearly, some bright physicist, noticing the discrepancy between the behavior of ideal and real gases, could have decided to try to give the theory a better experimental fit by introducing an arbitrary factor ρ. Having introduced this factor and juggled the equations around in order to gain experimental agreement, we are faced with the following situation. From a logical point of view, that is, strictly as a matter of deduction, the new theory T $+\rho$ offers a good predictive fit with the experimental data. However, there is a certain ad hoc quality about the manner in which the theory has been extended. This ad hoc quality would generally tend to throw some suspicion on the degree of explanation achieved. The introduction of ρ under the auspices of the model tends to give credence to the amended theory. We would be much more willing to accept the new theory with the accompanying model as an explanation of the factual evidence. Some justification for this increased confidence can be given as follows. The initial modelled theory presumably was an adequate explanation of some aspects of the phenomena under consideration. By extending the theory, using an already established model as base, all the credence of the original theory is transferred to the new theory. The new theory also presumably gives a better empirical fit and thereby has greater factual support than the old theory. Consider, however, if the new theory had been constructed completely ad hoc, or had resulted from the introduction of an arbitrary factor which had no

immediate interpretation in terms of the established model. Then, in spite of the factual support for the new theory, the credibility attached to the old theory in virtue of its model would not be automatically transferred to the amended theory.

It is clear, I think, that Hesse argues that such prior justification is necessary, but the question still remains, in what sense necessary? ([44], pp. 39, 43) Suppose a scientist were to make an arbitary extension of the theory T as suggested above. A colleague might object that such an extension was unwarranted and unjustifiable. The first scientist chooses to ignore his colleague and proceeds to make predictions on the basis of his new theory. He tests them. They turn out to fit the experimental data better than the old theory. The question now is: do these positive results justify the original extension? The answer is both yes and no. Yes, in the sense that for a scientific theory, the results justify the means. No, in the sense that even though the extension proved fruitful, the extension before the proof of its fruitfulness was unjustified. It should be obvious by now that we not only must distinguish between motivating reasons and justifying reasons but that there are at least two distinct senses of justification at work here. One has to do with the predictive feature of a theory and the other with the intelligibility feature of a theory. I think that one factor that clouds Hesse's analysis of models and theories is a failure

to keep these two features distinct.

In order to see this, consider that it might be argued that an ad hoc or arbitrary extension of a theory is unjustified even if it subsequently proves fruitful. There is a sense in which it is perfectly reasonable for the scientist's colleague to accept the new theory, after it has proved to give a good experimental fit, which he initially rejected, and still maintain that the original extension was unreasonable (unjustified). Hesse is not arguing, I should hope, that arbitrarily extended theories cannot turn out to be predictively successful. She is arguing that such extensions are unjustified and would not be taken seriously by scientists. This is not an unusual sense of "justify." Suppose I were to push a button without any justification (on a whim) and thereby knowingly blow up a manned space station. It is then discovered (something unknown to me) that the men on the space station were planning to destroy the world. This discovery (and that I have saved the world) does not, at least in one sense, justify my original action. I would think that it would still be considered unjustifiable, yet very fortuitous.

I suggest that this sense of justification or reasonable does not have to do with the consequences of my action but rather with its intelligibility. I suggest it is the same with respect to theory extensions. Without some model, theoretical modifications in the face of recalcitrant data will appear unintelligible, but it does not

seem to follow from that, that the theory cannot be predictive. In short, Hesse is blurring the distinction that this thesis strives to maintain between theories as predictive tools and theories as offering intelligible understanding of the world.

Hesse's thesis about the necessity of prior justification for theory extensions in terms of models seems indefensible. What she does seem to be right about is that the introduction of new correspondence rules into a theory T would not seem intelligible unless it were done in accordance with a particular theoretical model. But, as her thesis is stated, she seems either to confuse the notion of intelligibility with that of predictivity <u>or</u> to be claiming that, in some sense, a theory must be intelligible in order to afford predictivity. This latter thesis we have already argued is false.

2. In an article, "Models in the Empirical Sciences," R. B. Braithwaite takes issue with the view that models are predictive in a sense that the theory is not; i.e., that the model will yield new testable generalizations that the theory does not. ([10]) Braithwaite distinguishes four ways in which this claim can be understood. He supposes we are given a theory T with observable terms $A_1, \ldots, A_n$, and some terms $C_1, \ldots, C_m$, which are theoretical. We also postulate a model M of this theory (i.e., an alternate interpretation of the same calculus, such that terms $B_1, \ldots, B_n$ correspond to the A's, and $L_1, \ldots, L_m$ correspond to the C's.

(1) First type predictive novelty.

In this case, the initial propositions of the model lead to new generalizations about the observable terms of the model (the B's) which correspond to new generalizations about the A's which were not members of the original set of generalizations for which T was constructed. This type of predictive novelty is characterized by no change being made in the initial hypotheses. Braithwaite points out that, in this case, the model is no more predictive than the theory, since the new generalizations about the A's could just as well have been deduced from the theory by itself.

(2) Second type predictive novelty.

In this case, the L's may suggest a new property B_{n+1} which might be included in modified form of the generalizations involving the old B's. We should then look for a new A_{n+1}, corresponding to B_{n+1}, which is observable, and also look for new generalizations about the A's. This case is characterized by the addition of new hypotheses connecting the old L's to a new (model) observable property, B_{n+1}. According to Braithwaite the model is little help here since the difficult problem is in finding an appropriate A_{n+1}. The problem is not in (once having found an appropriate A_{n+1}) connecting it with the old theoretical concepts, which seems to be the function of the model here.

For Braithwaite, the real problem only begins <u>after</u> we have found a B_{n+1} and wish to find an appropriate A_{n+1}. His claim is that the model is of no help in this search.

Hesse might counter by claiming that the B_{n+1} "suggests" which A_{n+1} to choose <u>and</u>, in addition, gives us a reason for choosing some particular A_{n+1}. Otherwise, she would claim, we would be at a loss to justify the extension of our theory to account for the introduction of some one new A over any other. The difficulty here, as we have seen, is in understanding what it means to be a reason for trying one extension rather than another. A Duhemist would object that if the extension works, i.e., provides true predictions, then the fitting of the new A into the deductive structure is reason enough to justify its inclusion.

(3) Third type predictive novelty.

In this case, the old L's may suggest new propositions about the old B's to be added to the initial hypotheses. Here the theory is extended to incorporate new hypotheses about the old terms.

(4) Fourth type predictive novelty.

In this case, the model may suggest the addition of new L's. This, in turn, may suggest new generalizations about the old B's. It may also suggest adding new generalizations involving new B's. Here the theory is extended to incorporate new hypotheses involving new theoretical terms.

According to Braithwaite, predictive novelty of types (3) and (4) provide the best case for arguing that the use of model might be useful. Firstly, the model terms are "familiar" and thereby might suggest extensions of the theory along the lines outlined.

Secondly, it might be argued that the model points to some particular extension of the theory. This is so especially in the case that the model is sound (i.e., its initial hypotheses are true). This pointing is taken to be an argument by analogy, "for inferring from the known features of the model to unknown features of the theory." ([10], p. 230)

> . . . it is sometimes held that analogy provides a good reason for believing that any feature of the extended model will be reproduced in the correspondingly extended theory, until an empirically testable generalization, which is a consequence of supposing that the extended theory has this feature, is refuted by experience. ([10], p. 230)

Braithwaite denies that the analogy does provide a good reason for believing that some feature of the extended model will be reproduced in the extended theory. He says:

> Analogy can provide no more than suggestions of how the theory might be extended . . . . Whether the theory will bear extension in its initial hypotheses will have to be decided by testing against experience the testable generalizations deduced in the extended theory, and in this testing a model is of no use whatever. The thesis that a modelled theory has, ipso facto, greater predictive power than the bare theory cannot be sustained. ([10], p. 230)

Braithwaite notes that the model advocates reply to his objections with a series of questions all more or less aimed at the problem of the meaning of theoretical terms. Their claim is that models enable us to understand the meaning of theoretical terms while a lack of models prevents us from doing so. Braithwaite rests his case on the adequacy of the "indirect meaning" thesis he has proposed along with Carnap, Hempel and others. This thesis is criticized in

some detail in the next chapter ("Models and Theoretical Terms: Some Instrumental Views"), where it is argued that models are essential to the intelligibility of the theory and the theoretical entities it postulates.

The core thesis of Braithwaite's position on models is that models and theories are merely alternate interpretations of a common uninterpreted formal calculus. The structural isomorphism that is postulated to exist as a necessary condition of two systems being related to one another as model to theory precludes the possibility that the models could prove to be more predictive than the theories.

From Braithwaite's point of view, where a model and a theory are just alternative interpretations of a common uninterpreted formal calculus, it is a grave error to confuse the model with the theory.[1] This concept of model is at variance with the one being developed in this thesis. From our point of view, the model and the theory are one. This is to be understood in the following sense. The theoretical objects, i.e., the entities and processes described by the theoretical predicates are identified with the model objects. Thus, in terms of Braithwaite's analysis, we would say that the various interpretations of a single calculus were all <u>distinct</u> theories. Not all of these may be accepted as physically plausible. It is widely

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[1]See a similar criticism of Braithwaite's views by M. Spector in [83].

recognized that a formal calculus may have an infinite number of interpretations. Some of these, i.e., those which postulate numbers or classes of numbers as theoretical entities are rejected out of hand. In order to be candidates for physical models, the interpretations must ascribe some descriptive predicates to the theoretical entities. This is in line with our requirement that a physical theory offer an intelligible explanation of phenomena in terms of observational predicates or predicates derived by analogy from observational predicates. The partial-interpretation thesis, to which Braithwaite subscribes, is too weak to exclude interpretations which do not possess this characteristic. ([92]) A viable alternative seems to be the interpretation of theoretical entities directly via suitable analogical arguments.[2] It is clear, however, that such direct interpretations only confer a certain intelligibility onto the theory. There is no method whereby we may guarantee that such an intelligible theory will lead to any fruitful predictive consequences.

Now, in a trivial sense, no uninterpreted theory can yield predictions which are testable, merely because, no matter what deductions are carried through in the formalism, there will be no correspondence rules to indicate the empirical significance of any deductive conclusions. Thus, in our sense of model, where a model provides the

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[2]See Chapter II.

interpretation for theoretical terms, and thereby confers intelligibility onto the theory, intelligibility is a prerequisite for predictivity. What is clearly not the case though is that any given model which confers intelligibility onto the theoretical aspects necessarily leads to any fruitful predictions. As we have seen, not only is it possible to construct an intelligible theory with no experimental consequences, but even if some intelligible theory did have observational consequences, the mere fact that the theory is intelligible does not guarantee that all experimental predictions which follow from the theory will fit the data. In short, intelligibility does not imply fruitful predictivity.

3. In a paper, "Constructions and Inferred Entities," L. W. Beck examines Russell's thesis that "Wherever possible logical constructions are to be substituted for inferred entities." ([6], p. 74) With respect to the question of theoretical terms, the question is: need we infer that theoretical entities have any existence other than the systemic existence guaranteed by their role in a given theory? Beck assigns three functions to "inferred entities" in science and goes on to argue that constructs may function in the first two ways but not in the third. ([6], p. 77) Hence, Russell's thesis must be rejected.

The three functions for theoretical entities (I use this term rather than Beck's "inferred entities" so as not to prejudge the case against constructs) are roughly as

follows:

(1) Theoretical entities serve to summarize observed facts.

(2) Theoretical entities serve as objects of scientific research, as e.g., the atom, the gene or the electron. For example, the atom postulated as an unobservable entity may spur on research in order to develop techniques by which it will become observable.

(3) Theoretical entities are the basis for further predictions and development of the theory.

Now, Beck concedes, and so shall we, that both constructs and inferred entities can fulfill functions (1) and (2).[3] The bone of contention is function (3). Beck argues that inferred entities function as predictive bases in a sense that constructs do not. In order to evaluate this claim, and point out what I feel is a basic ambiguity in the notion of a construct, we must pay closer attention to a precise specification of these terms.

By an inferred entity, Beck understands a "supposed real existent whose existence is inferred if and only if a given substantive hypothetical proposition about it is

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[3]In the case of function (2) there may be some question as to how a mere construct, postulated as unobservable, can become observable. The answer is easy enough, in Russell's case at least. The atom, as construct, is a construct out of sense data. The "observability" of the atom only turns on the availability of sense-data, which was unsuspected at the time that atoms were postulated. The fact that what started out as an un-observable construct has become observable does not affect in any way the fact that the atom is still only a construct out of sense data.

confirmed." ([6], p. 75) This definition requires unpacking. By a "substantive hypothetical proposition," Beck understands a hypothesis which is capable of yielding predictions which may be empirically tested. ([6], p. 75) Thus, if on the basis of hypothesizing that gases are composed of molecular particles obeying classical dynamics, and this leads us to predict that pv = nRt, then the hypothesis is substantive and we have reason to infer the existence of these molecular particles. We are entitled to think that they really exist (have "real existence") if we think that it is likely that we will be able to make other inferences which are testable on the basis of assuming that they exist. ([6], p. 75)

A construct, on the other hand, does not have real existence in Beck's sense, but only "systemic existence." "A construct," he says, "is that entity whose systemic existence is affirmed by the confirmation of the relevant hypotheses." ([6], p. 75) Where by "systemic existence" he understands "the mode of existence of an entity all descriptions of which are analytic within a system of propositions." ([6], p. 75)

The ambiguity in Beck's notion of a construct, to which I alluded earlier, lurks in the term "analytic." Now, in one sense Braithwaite, in his book Scientific Explanation, attacks and solves this very same problem by showing that "if the theoretical terms of theory are logically constructed out of observable entities, the theory will

be incapable of being modified to explain new sorts of facts." ([11]) Thus, Braithwaite confirms Beck's analysis, to the extent that "construct" is construed in the sense implicit in the just cited quotation from Braithwaite.

However, as we have noted earlier, Braithwaite goes on to state and defend the view that theoretical terms are implicitly (rather than explicitly, as for the case of Russell's constructs) defined in terms of the observational consequences of the theory. Theoretical terms which are implicitly defined in virtue of their place in a deductive theory, and have no other empirical content whatsoever, have a systemic existence in just the same sense as do Beck's constructs. ([11], p. 85) In other words, without the addition of new correspondence rules to the theory, the theories, from the point of view of the partial interpretation thesis, are just as impotent as theories wherein the theoretical entities are logical constructs in Russell's sense. (In a very important sense, the heart of the debate between the modelists and the formalists turns on the question of the introduction of new correspondence rules into a given theory.)

It is quite true that theoretical entities in Braithwaite's sense are extendable in a sense that Russell's constructs are not. But this is simply due to the fact that theoretical entities which are implicitly defined are not completely specifiable by any set of correspondence rules. One can always introduce more correspondence rules and still

say that the entities remain the same, only modified. In the case of the constructs, which are, in effect, explicitly defined by a finite set of correspondence rules, the addition of a new correspondence rule would result in a completely different theory about completely different entities. But, surely if ever there was merely a verbal difference, this is the case. The case for claiming that the difference is only verbal is strengthened by the fact that in neither case, construct or implicitly defined entity, is there any systematic method for extending a theory through the introduction of new correspondence rules. Indeed, a corollary of the partial interpretation thesis seems to be the idea that there is no systematic method for adding correspondence rules to the theory.

Thus, for all practical purposes there is no difference between constructs and implicitly defined entities. In particular, they are both "analytically defined," the implicitly defined entities admittedly in a looser sense than the constructs.

The important point for Beck's thesis is that the implicitly defined terms enable the theory to be extended, allowing for the predictivity denied to theories containing only constructs. Further, the fact that the theoretical entities are implicitly defined rather than constructs does not give them, ipso facto, any predictive advantage. At best they possess a verbal advantage. It is clear, I think, from Beck's subsequent discussion of the supposed

advantages of inferred entities over constructs, that his inferred entities are no better off than implicitly defined entities. Certainly, they do not afford any greater indication of the direction of theory growth.

4. In a recent article, "Correspondence Rules," Kenneth Schaffner argues that the use of models and analogies is not necessary for the interpretation of theoretical terms. ([69]) He suggests instead that this function may be taken over by a special kind of correspondence rule which draws on "antecedently understood theoretical meaning." ([69, p. 283) Schaffner subscribes to the view that models and analogies are merely psychological sources for theories and are not to be confused with what is "logically relevant," i.e., "antecedent theoretical meaning." Thus, he argues that "to claim that a model or analogy is required [to give relevance to the theoretical terms of a theory] is to conflate a psychological source with a logical requirement." ([69], p. 288)

Schaffner's view derives from the fact that he accepts the Braithwaitean analysis that a model is to be distinguished from the theory and that the model objects are in no sense to be identified with theoretical objects. It is this thesis which allows him to speak of conflation. However, from the point of view of this thesis of this chapter, Schaffner himself is guilty of conflating models with the analogues from which they are derived. In fact, however, if we carefully distinguish the analogue (or

analogies)--the psychological sources--from the models which are developed, it is clear that the models are to be identified with the theoretical objects. The conflation of which Schaffner speaks arises from his own mis-identification of the analogues with the theoretical objects (or models) which are described by the theory.

In order to see that this is the case, we need only inquire how theoretical entities get their "antecedent theoretical meaning." It seems that they must get their antecedent theoretical meaning either arbitrarily or because, in fact, the theoretical objects are conceived of as model objects. In fact, it is possible to show, I think, that what Schaffner understands by "antecedent theoretical meaning" is just what we mean by models as theoretical objects.

Schaffner himself speaks this way, e.g.,

> In the usual way of speaking one would say that Lorentz [compare an earlier citation on p. 282 of Schaffner's paper] was providing his readers with an <u>analogy</u> or <u>model</u> of the electron. I would like to re-interpret this however, and suggest that Lorentz is doing more than providing an analogy for a theoretical entity--rather . . . he is <u>creating the meaning</u> of the term "electron" <u>by drawing on antecedently understood notions, and putting them together in a radically new way</u>. ([69], p. 283)

It is clear from this passage, especially the italicized section, that Schaffner is describing what we have called a model object, and the only reason he forbears to call it a model is that he identifies models with the analogues upon which they are based. ([69], pp. 283-84)

The "antecedent theoretical meaning" serves two functions in science. First, it serves to make a scientific theory intelligible in our sense of the word. Schaffner says that it makes theories "meaningful per se." ([69], p. 284) But, in addition to making the theory meaningful or intelligible, it serves as a guide for establishing correspondence rules whereby the theory will yield predictions that can be made subject to empirical test. ([69], p. 284) We see that Schaffner's "antecedent theoretical meaning" is really the ascription to theoretical entities of an intelligibility and a significance that is not exhausted by the experimental implications of the theory. ([69], p. 283)

The reason that theoretical entities must possess some antecedent theoretical meaning is that this meaning "justifies us in saying these entities behave in one way rather than another, and allows us to use borrowed theories to infer observational consequences of this behavior." ([69], p. 288) Let us examine this claim piece by piece. In the first place, we are advised that the antecedent theoretical meaning "justifies us in saying [that] these entities behave in one way rather than another." We have already met some ambiguity in the notion of "justify" in this kind of situation.[4] What Schaffner means to say, I should guess, is that the antecedent theoretical meaning

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[4]Compare above, pp. 40ff.

makes the behavior of these theoretical entities intelligible to us. We are justified in saying they behave the way they do because it makes sense, given our model, that they behave the way they do. Thus, our assertion that the pressure of a gas is caused by the incessant pounding of the molecules on the sides of the gas container is justified in this sense, because we can intelligibly understand how and why they would do so, given our molecular model.

The antecedent theoretical meaning "allows us to use borrowed theories" in order to make predictions. Now it is well known that theories rarely, if ever, stand alone. In order to make predictions and to test them we explicitly or implicitly assume many other theories which we are not explicitly testing. Almost every experiment in which an observer actually sees something, be it a meter reading or what have you, presumes as a matter of course a theory of optics. That this does not distort Schaffner's sense of "borrowed theory" can be seen from the example of a causal sequence he gives on page 287. Again, however, I suggest that there is an ambiguity in the notion of "allow." Clearly, regardless of any antecedent meaning which makes the behavior of the theoretical entities under investigation intelligible to us, we may assign any kind of correspondence (or semantic) rules in order to account for a particular experimental result. The only requirement would seem that under similar experimental conditions we must assign the same or similar correspondence rules. If we did not do

this, then it is hard to see how the experimental results could be significant in an unambiguous way. What confers legitimacy on this assignment, in one sense of "allow," is that such assignments prove predictively fruitful. Hence, in this sense, the sense in which a theory is predictive, antecedent theoretical meaning is of no significance.

However, there is a sense in which the assignment of these correspondence rules may be unintelligible to us, that is, we may not have any model to enable us to "see" that one rule is "reasonable" whereas another is not. This again points to the function of antecedent theoretical meaning (or a model) as conferring intelligibility onto the phenomena.

Finally, to clinch the case, Schaffner himself, in a footnote, refers to Maxwell's "crobes" example by remarking that "Part of the function of the antecedent theoretical meaning is to tell us what the entity [hitherto unobserved, but now observed] <u>should</u> look like." ([69], p. 284) I submit that the rest of the function of antecedent theoretical meaning is no more and no less than making intelligible the assignment of certain correspondence rules which connect up one domain of theoretical inquiry with others. This function obviously has important implications for making the predictive results of a given theory <u>intelligible</u> to us, but it certainly has no more significance. Schaffner himself, of course, does not imply that such intelligibility does any more than "guide" the scientist.

([69], p. 284)

The final objection that Schaffner has to identifying his "antecedent theoretical meaning" with a model entity is, among other things, that the use of models becomes involved with "conflicting analogies," of which he has in mind, in particular, the wave-particle duality in quantum mechanics, while his notion avoids whatever complications are produced by this "morass." ([69], p. 288) I can only say that I do not see how this is so, nor does Schaffner anywhere in his paper, to my reading at least, show it to be so.

5. This concludes our discussion of the role of models in relation to the predictivity of scientific theories. Our conclusion is that the major function of models is to confer intelligibility onto the theoretical phenomena. We have earlier argued that intelligibility and predictivity are two more or less distinct components of an adequate scientific theory and adequate scientific explanations. In the course of this chapter, we have examined several contentions to the effect that intelligibility is a necessary prerequisite for predictivity in a scientific theory. Except in the trivial sense that intelligibility means that the theoretical terms are given some interpretation or another, this claim has been rejected.

CHAPTER IV

Models and Theoretical Terms: Some Instrumental Views

Any science, insofar as it is the least bit sophisticated, involves an appeal to entities or processes which are unobservable. This tendency raises special problems concerning the interpretation to be placed on terms which designate what is unobservable. One strategy devised in recent times has been the attempt to avoid referring to these unobservables. In general, programs designed to eliminate reference to unobservables have met an ignominious end. The sensationalism of Mach, and the efforts of the early positivists and empiricists of this century to do away with concepts which could not be eliminated in favor of some completely observational equivalent have all ended in failure. Recent attempts to sidestep the issues raised by unobservables by appealing to results from the realm of logic have met with only limited success. While we seemingly can avoid being disturbed by the nature (the <u>what</u>) of unobservable entities or processes, these logical moves do not avoid the necessity of saying <u>that</u> these unobservables exist.

A simple example should make the situation clearer. Consider a black bottle which is completely sealed and from which an annoying and persistent intermittant buzzing appears to be emanating. We can see the bottle (although not inside it) and we can hear the buzzing. From our past

experience, bottles in and of themselves do not buzz. We conjecture <u>that</u> something is in the bottle producing the buzzing. The question is, <u>what</u>? In the best of all possible worlds, we need only smash the bottle and look inside. We are confident of finding something; perhaps a fly, perhaps some artificial mechanism, perhaps some peculiarity in the construction of the bottle. In this world, however, everything is not always so easy. Suppose we attempt to smash the bottle but find we cannot. We are denied, in effect, direct accessibility to see what is going on. We must resort to tests which will, at best, allow us to construct an indirect picture of what is going on. Our first test is to attempt to find out if anything is in the bottle, by taking X-ray pictures, for example. When we do this, suppose we are lucky and discover a spot on the film which appears first at one place on the film, now at another. We conjecture that something is moving inside the bottle and reasonably suppose this something to be the source of the buzzings. We further observe, let us suppose, that the thing, call it U, sometimes appears to move on the inside surface of the bottle and sometimes moves in the space inside the bottle. We also note that when U is on the bottle, no buzzing occurs. When U moves off the bottle, the buzzing begins and continues until U is on the bottle again. Conjecturing about what U might be, we note that it exhibits certain typical insect-like behavior. This suggests that U itself is an insect of some sort. Insects we know require

sources of food and air and so we proceed to conduct tests in an effort to discover any such sources. Finding that there are such sources would confirm our hunch that U is an insect-like thing. However, unlike an ordinary insect, we never see U, we only infer what it is on the basis of the observable information that we have. Do we have any justification or warrant for believing U to be an insect of some sort? We may ask what further purpose it serves for us to think of U as insect-like, other than to assert that U exhibits, under specified conditions, some observable properties which are characteristic of insect behavior.

In short, U is an unobservable which we can use in formulating hypotheses and making predictions about the structure of the black bottle system. What sort of status ought we accord to these unobservables? This question is usually discussed under the heading: the ontological status of theoretical entities. I have hesitated to introduce the term "theoretical" because I have always felt uncomfortable about the glib identification of "theoretical" with "unobservable" and the tendency to shift back and forth from one characterization to the other in the course of philosophical discussions. The tendency to speak of the problem of theoretical terms obscures the fact that there are several issues at stake. ([64]) Here I want to distinguish two issues concerning, on the one hand, the epistemological status of theoretical terms, and on the other hand, the logical status of theoretical terms. The question of the

ontological status of theoretical terms is reserved for subsequent consideration.

Consider first the epistemological status of these terms or, if you prefer, of the entities which they may or may not designate. Reconsider the term "U" introduced to explain the phenomenon of the buzzing bottle. We have seen that although we may conjecture U to be insect-like and gather more and more evidence to support that view, we can never be sure that U designates an unobservable insect. The implication of this point is that, compared with the information we may gather about ordinary observable insects, we are somehow in an inferior position with respect to our information about U. Someone may object to this conclusion as follows. Are we really worse off with respect to our knowledge about U than we are with respect to our knowledge about some observable insect, say the fly which has been intermittantly bothering me as I write this draft? Our objector says no. Clearly, we may gather more information about the fly which is bothering me now than we can about U, but it always remains possible that we are mistaken in thinking that this buzzing observable object is a fly. It may turn out, on closer inspection, to be a mechanical bug. Even if it doesn't, and still seems to be a fly no matter what tests we perform, we are still only relatively better off. The possibility remains that someday the "fly" (if that is what it is) inside the bottle will become observable, in the same way that microbes and large molecules have

become observable. From an epistemological point of view, there seems to be a continuous spectrum of varying degrees of observability. There is no epistemological reason for distinguishing theoretical from observational terms. This idea of a continuous spectrum from observable to unobservable will be useful later when we come to discuss the ontological status of theoretical entities.

Consider now the question of the logical status of theoretical terms. It is convenient (we are told) to think of a theory as an uninterpreted calculus plus an interpretation. We may consider the calculus to contain a set of nonlogical primitive terms which we may divide into two classes, T-terms and O-terms. The O-terms are directly interpreted as various observable properties which are characteristic of the explanatory domain of the theory. It is assumed that the meaning of each O-term is relatively stable, in that specific rules and procedures are given for definitely deciding whether a given property is designated by an O-term or not. For the sake of convenience, we may assume that the O-terms are those which a native speaker with no special training in the sciences could use and understand with ease. ([19], p. 40)

Certain of the T-terms are now correlated with the O-terms by means of correspondence rules or operational definitions of some sort. Unlike the O-terms, there is a certain arbitrariness about the identification of the T-terms. In the first place, we do not require that every

T-term be correlated with a set of O-terms. These theoretical terms remain uninterpreted. A standard example of such a term is the ψ-function in quantum theory. The function $|\psi|^2$ is generally interpreted as a probability measure. If we choose a particular ψ-function, say of an electron in a potential well, then the values of $|\psi|^2$ are said to yield the probability that the electron is in a particular state. The set of all such values yields a probability distribution. The ψ-function itself, however, although in some way associated with the electron, is not given any interpretation. Various attempts in the early days of quantum mechanics to provide interpretations for the ψ-function proved to be unfruitful. ([51], p. 281)

In the second place, those T-terms which are related to O-terms via correspondence rules are not, in general, explicitly definable in terms of the O-terms. For all practical purposes this has two major consequences. First, the T-terms cannot, in general, be eliminated by replacing their occurrence in any sentence containing them by an appropriate expression containing only O-terms. Included among these are the so-called dispositional terms, e.g., "is soluble," "is magnetic," etc., of which science abounds.[1] Secondly, the fact that these terms are only

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[1]In Testability and Meaning, Rudolf Carnap proposed to treat dispositional terms by means of reduction sentences, which were designed to specify necessary and sufficient observational conditions for the applicability of dispositional concepts. This method failed, and in "The Methodological Character of Theoretical Concepts" (pp. 62-69), Carnap proposes to treat them as theoretical concepts.

implicitly defined gives the theories in which they occur the "open-texturedness" which is essential for their growth. If a given theory is to "say more" than the original observational phenomena on which it is based, i.e., if it is to be an adequate theory, this "open-texturedness" is essential. ([11], p. 53)

The meaning of the T-terms has a certain instability which the O-terms presumably lack. Whereas the O-terms are thought of as being stable with respect to their meaning and reference, the T-terms are not. The T-terms are said to derive their meaning via their role in a theory, that is, by virtue of the theoretical laws in which they occur and the correspondence rules by which they are related to observational statements. The T-terms are said to possess "indirect meaning" by virtue of their place in a nomological network. An electron, for example, is just what our electron theory claims for it and no more. The thrust of this view is that there is held to be a logical distinction between the T-terms and the O-terms. One advocate of this view is R. B. Braithwaite. ([11])

Professor Braithwaite adheres to the view that a scientific theory can be thought of as an abstract calculus which is given an interpretation in the following way. There are two basic sets of nonlogical symbols in the calculus. One set, our O-terms, is correlated with observable entities. This correlation, however it is made, confers a "direct meaning" onto these symbols. Such correlation

corresponds to what Carnap and Hempel call "semantic rules." The second set, our T-terms, acquire their meaning indirectly via their role in the deductive apparatus of the theory.

These two different sets of terms give rise to three kinds of formulas which can occur in the calculus (or three kinds of sentences which can occur in the theory). One kind consists of those formulas which contain only O-terms. These correspond to the observation sentences of the theory; i.e., those empirical generalizations for which the theory was designed to explain.

A second kind consists of those formulas which contain both O-terms and T-terms. These correspond to what we have previously called correspondence rules. The third kind of formulae consists of those which contain only T-terms. In the interpreted theory they correspond to theoretical laws which do not contain any observable terms.

What, then, in Braithwaite's view is the problem of the T-terms? The problem, as he sees it, is in determining what these terms mean. Assuming that the meaning of the O-terms is well defined in that we can see or point out what we mean when we use such terms, what about the meaning of the T-terms? By hypothesis the entities or processes designated by such terms cannot be seen or pointed out, for if they could be defined in such a way they would designate observables and, hence, be O-terms. Part of the basic characteristic of these T-terms is that what they designate (if anything) is unobservable.

Let us turn to Braithwaite's analysis of the meaning of T-terms. He says that they have "indirect meaning," in contrast to the "direct meaning" of the O-terms. The T-terms derive their indirect meaning by virtue of their role in the deductive structure of the interpreted calculus which is the theory.

Braithwaite considers and rejects Russell's view that T-terms are to be logically constructed out of O-terms. ([11], p. 53) This view amounts to defining T-terms of a theory, e.g., "electron," explicitly in terms of the set of observations which are used to detect the presence of the "theoretical" entity, in this case, the electron. In practice one explains a given observable state of affairs by deducing a proposition containing O-terms (which describes the situation) from statements of a theory some of which contain the problematic T-terms. Russell's idea was that in order to find out what a particular T-term meant, it was necessary to reverse this procedure and arrive at definitions of the T-term in terms of the O-terms which occurred in the sentences which they helped to deduce. Such a task would, in general, be enormously difficult but, in principle, it would be necessary to carry through in order to determine the meaning of a given T-term.

This view was criticized by Ramsey and Braithwaite as conceiving of theories in a manner too restricted to enable them to be extended in a natural way to explain new data. ([11], p. 53) Braithwaite constructs two simple

theories, his "factor" theories, to exhibit the restrictiveness of satisfying Russell's demands. ([11], pp. 54ff.) He shows that if the "factor" theories are modified to allow the T-terms to be explicitly definable in terms of the O-terms, then the result is that the theoretical postulates of the theory would be logically equivalent to the empirical generalizations they were designed to explain. In such a case, the theoretical postulates would no longer "explain" the generalizations in any acceptable sense of the word "explain." They would, in fact, be mere reformulations of those generalizations.

Braithwaite goes on to show how if some new data arose in the form of empirical generalizations such that it was deemed desirable to extend the "factor" theory by adding new factors, the resulting expanded theory would be restricted in undesirable ways. ([11], p. 76) Hence the project of determining the meaning of T-terms directly by constructing them out of observables must be abandoned.

Braithwaite's solution is not to abandon the idea of defining the T-terms in terms of the O-terms, but rather to adopt a less restrictive procedure for doing so. The technical details need not detain us here. ([11], pp. 76ff.) The general idea in terms of his simple "factor" theories is the following.

Suppose the original "factor" theory has three theoretical factors (T-terms) and three observable factors (O-terms). Then any one of the theoretical factors, e.g.,

t, will be (implicitly) defined in terms of, say, two of the observable terms,

(1) $\underline{t} = \text{df. } F(O_1, O_2)$

in such a way that $F(O_1, O_2)$ is a necessary but not sufficient condition for the applicability of t. Thus, t remains open-ended in the sense that the addition of new conditions for the applicability of t, in terms of new O-terms, is not incompatible with (1). This is the gist of Braithwaite's argument, although I have used a different notation from the original in order to set up the example. ([11], pp. 77-78).

There are two related senses of implicit definition here. One is that the terms which are implicitly defined are extendable whereas those explicitly defined in terms of certain observables and no others are not.[2] The extension of meaning is accomplished by a procedure which is roughly the same as adding new correspondence rules to the theory in terms of which any given T-term is further connected with the observable base. Adding new rules to a theory whose T-terms were explicitly defined would be equivalent to creating a different set of concepts rather than merely extending the range of the old ones.

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[2]Braithwaite ([11], p. 77): ". . . the theoretical terms of a scientific theory are implicitly defined by their occurrence in initial formulae in a calculus in which there are derived formulae interpreted as empirical generalizations, the theoretical terms cannot be explicitly defined by means of the interpretations of the terms in these derived formulae without the theory thereby becoming incapable of growth."

The second sense in which the theoretical terms are implicitly defined is that they derive their meaning from the O-terms which occur in observation sentences which are deduced from the theory with the help of the T-term in question. These two notions tie together in that as a theoretical term aids in the deduction of propositions containing new observables, its definition extends to include these new observables in some relation to the observables in terms of which the T-term was initially defined. As Braithwaite says, "The implicit empirical definition of the theoretical terms in a scientific deductive system consists in the fitting of the calculus to the system from the bottom upwards." ([11], p. 78) By this he means that the observable (O-) terms are given their meaning directly and then the T-terms derive their meaning indirectly through their relationship to these O-terms.

The upshot of this analysis is that theoretical terms do not have meanings independent of the theoretical structure to which they belong. The same is true of propositions which contain theoretical terms. Their meaning is determined by their place and role in the deductive structure. The O-terms and statements which contain only observable terms are assumed to have meaning independently of any theoretical system. Thus, terms like "black" and propositions like "All ravens are black" are meaningful independently of any theory, while terms like "electron" and propositions like "All electrons have charge = q" are

meaningless outside the context of some theory about electrons.

Since theoretical terms only have an indirect meaning, it would seem to follow that theoretical propositions also have only indirect meaning. For Braithwaite, genuine theoretical propositions or hypotheses are general statements of the form: All x is y. ([11], p. 14) Braithwaite notes that it may sound strange to say that the theoretical hypotheses only have indirect meaning. ([11], p. 84) He tries to make this view plausible by arguing that all general statements only have indirect meaning, hence, a fortiori, those general statements which are theoretical hypotheses only have indirect meaning. His argument seems to me to be unsatisfactory and inconclusive.

Braithwaite first points out that certain terms like "every" were traditionally recognized by logicians to have no independent meaning outside particular contexts. Thus, "every" by itself is meaningless but "every cat" is not. Such terms were called "syncategorematic" by logicians. Braithwaite then tries to argue that propositions of the form "Every x is y" are also syncategorematic. Just as terms like "every" must be referred to a designated class in order to be meaningful, general statements of the form "Every x is y" must be referred to a deductive system in order to be meaningful. In Braithwaite's view whatever meaning "Every x is y" possesses, it derives from the fact that in conjunction with a premise of the form "A is an x," the

proposition "A is a y" is deducible. Braithwaite then goes on to argue that "Every cat eats fish" has no direct meaning although propositions of the form "Felix eats fish" do have direct meaning. ([11], p. 82ff.)

Braithwaite goes on to suggest that theoretical terms (like the Schrodinger Ψ-function) gain their meaning in the same way that terms like "every" do. By implication, theoretical propositions involving Ψ-functions would be indirectly meaningful in the same sense that "Every x is y" is supposedly only indirectly meaningful. This position is summed up by Braithwaite in these words: "Once the status within a calculus of a theoretical term has been expounded, there is no further question as to the ontological status of the theoretical concept." ([11], p. 82)

Insofar as Braithwaite establishes that theoretical terms and theoretical propositions function differently in a deductive calculus from observation terms and observational propositions, we must admit that the logical status of the theoretical entities differs from the logical status of observable entities. Where we may take exception to his conclusion is the point at which he identifies the question of the logical status of theoretical entities with the question of their ontological status. In other words, specifying how theoretical terms function in a deductive nomological network does not, in and of itself, serve to specify how these terms are to be interpreted.

The way that Braithwaite seeks to avoid the ontological problem of theoretical terms is by appealing to the distinction between an abstract calculus and its interpretation. The problem is just this. Observable terms (or terms which are correlated with some observable quantity) clearly refer to object or events that we more or less directly observe. On this point, Braithwaite says, "In this book [Scientific Explanation] experience, observation, and cognate terms will be used in the widest sense to cover observed facts about material objects or events in them as well as directly known facts about the contents or objects of immediate experience. ([11], p. 8) Braithwaite explicitly rejects considering problems of the "philosophy of perception" as to what exactly we do observe. ([11], p. 8)

As such, the "ontological status" of the entities designated by observable terms is secure. We see tables, observe water boiling, all more or less directly. There is no question that if anything exists, these (kinds of) objects and events exist. This I take to be the paradigm ontological commitment for Braithwaite. The question now arises, what about all the other terms floating about in scientific treatises such as "electron," "spin," "temperature(?)," "radiation," etc.? What is the ontological status of what they refer to, if anything? We do not directly observe them in the same sense that we directly observe tables and boiling water. Do these theoretical entities exist? If so, are they like tables and such or

not? If not, what are they like?

Braithwaite rejects the suggestion that these theoretical terms are not empirical, that is, do not refer to anything real. These terms are empirical since they occur in scientific theories from which we can derive observation statements. ([11], p. 52) The solution to the problem of their exact nature that Braithwaite offers is based on his initial analysis of a scientific theory into an abstract calculus on the one hand and an interpretation of that calculus on the other.

Once we have made this distinction, we can shift the burden of the question about the ontological status of some theoretical entity T from the entity T to the (uninterpreted) term "T." In effect, we are no longer concerned with the concept or entity T but only with the term "T" which designates it. Instead of asking about the existence or reality of T, we ask about the way "T" functions in the scientific theory. That it does function in a theory in some nontrivial way is enough to confer respectability onto T (or "T"). All we can expect to find out about T (or "T") is how "T" functions in the calculus which underlies the scientific theory in which "T" occurs. There is nothing more to know about T (or "T"). ([11], p. 82)

This will appear to many people to be a dodge and outright evasion. It might be objected that we are not interested in how some term is used; what we want to know about is the thing or process which that term designates.

Braithwaite recognizes this possible objection and offers as a solution the return to Ramsey's solution, which consists in existentially quantifying over the theoretical terms in a theory so that they no longer explicitly appear. ([11], p. 80) This technique is known as forming the Ramsey sentence of a theory; it can be shown that no deductive power is lost in the process. The theory and its Ramsey sentence both have the same set of observational consequences. The Ramsey sentence of a theory has the advantage over the theory that the theoretical terms no longer appear.

However, the Ramsey sentence technique is still only a sophisticated evasion. For one thing, the Ramsey sentence approach assumes what is at issue, namely, that it is reasonable to divide the class of terms into two distinct sub-classes, the one consisting of the terms which designate observables and the other consisting of the terms which designate theoretical entities. Moreover, "since an unobservable entity by any other name is, however, just as unobservable, the Ramseyan shift from a definite to an indefinite referential mode continues to countenance individual theoretical entities despite its elimination of theoretical constants." ([73], p. 271)

I want to suggest that both Braithwaite's analysis of the meaning of theoretical terms and the Ramsey sentence elimination technique are both inadequate. In a nutshell, both moves suffer from the same deficiency, that is, they presuppose what is the basic point at issue, namely, that

there is a sharp distinction that can be drawn between the class of theoretical terms and the class of observational terms. It is clear that, if we grant at the outset some such sharp distinction, then there is good reason for treating one class (the observational) as somehow privileged, and seek to eliminate or reduce to functional servitude the members of the other. However, it is precisely the status of the members of this other class which we hope to clarify. To presume from the beginning that they have a radically different status is to unfairly prejudge the issue.

I want to offer an argument independent of Scheffler's to show the inadequacy of the Ramsey sentence technique. To do this we must turn first to the notion of "explanation." After all, when we ask for an explanation of something, what are we asking for? An explanation, for Braithwaite, is basically an answer to some "Why?" question. The explanation must in addition afford some sense of intellectual satisfaction. ([11], p. 319) In the physical sciences, this satisfaction is afforded most often by specifying the cause of the event along with the lawful connections which tie the cause to the phenomenon in question. Of what, then, do we want a scientific explanation? Let us restrict our attention to the explanation of observable states of affairs. To explain these matters of fact is to provide for them the cause and the requisite set of generalizations, on the basis of which we are led to expect the phenomenon in question to occur. Now what is

the phenomenon in question which we want explained? Suppose we are investigating the behavior of gases. In the course of this investigation we employ certain more or less sophisticated instruments and collect readings on the basis of which we can interpret the phenomenon. Suppose that we are particularly interested in the pressure of this gas. To determine the pressure of the gas we use a P-meter. On one particular occasion the P-meter reads 45. Now the question is, what are we trying to explain, the meter reading or the state of the gas? It seems clear that we are not trying to explain why the P-meter reads 45; what we are trying to explain is why the pressure of the gas is what it is. These are not at all the same question, since what would provide intellectual satisfaction in the one case would not necessarily give such satisfaction in the other. For example, one answer to the question "Why does the P-meter read 45?" would begin by describing the mechanical and electrical construction of the meter. By so doing and invoking the appropriate mechanical and electrical laws, we could deduce the statement "The P-meter reads 45." This "explanation" says nothing whatever about pressures, nor need it say anything at all about gas systems. It gives us no intellectual satisfaction with respect to the question we are really interested in, namely, "Why is the pressure of this gas sample = 45 psi?"

In order to give a satisfactory explanation of the pressure of some gas sample, we must at least talk about the

temperature and volume of the gas, if not about the molecular constitution of matter. But meter-readings are not pressures, temperatures or volumes. They can at best be interpreted as measuring pressures, temperatures or volumes. And it seems to be a rule of thumb, that the more complex our measuring devices the more interpretation that we must provide in order to make sense of the results. On the basis of this need for interpretation, I want to argue that terms like "pressure," "volume," "temperature," if not theoretical are at least theoretically impregnated or theory-laden to some extent.

Now the point is, if the terms like "pressure," "volume," and "temperature" are theory laden, then they are in danger of being quantified out of the theory by the Ramsey sentence technique. We would then be left with the theory explaining meter-readings, etc., which will not give us the intellectual satisfaction that we seek. For we are not primarily interested in explaining the state of a meter, rather we are interested in explaining the state of a gas.

What results from the Ramsey elimination is that the only "ontologically secure" entities are rulers, wires, metal containers, painted dials, and the like. We seem to be left with the view that a scientific theory explains the various configurations that these objects assume when juxtaposed in certain ways. When we are at the level of using Archimedes' buoyancy principle to explain why wood floats

on water, such a characterization may be adequate. But when we get to something as sophisticated as analyzing spectral lines or even measuring the pressure of gases, such a view seems to be a misrepresentation of scientific activity.

What we wish to explain, in the example above, is that the pressure of the gas is 45 psi. As such, this is an observation report, although, to a degree, theory laden. For most of science, I would argue, the theoretical content of "observational" reports cannot be completely done away with. The conclusion, of course, is that the Ramsey elimination technique can only be involved at the peril of eliminating what it is that we wish to explain.

In the light of this conclusion we must reject the attempt to eliminate reference to theoretical terms by using the Ramsey sentence. Braithwaite's alternative analysis, to shift the burden of the question about the ontological status of theoretical entities to a question about the role of some term in a calculus has already been seen to be inadequate. Nothing in Braithwaite's analysis leads us to conclude that it is either acceptable or feasible to consider the ontological status of theoretical entities as distinct in kind from the ontological status of observable entities. Such a conclusion would follow only if it could be shown that the question of the function of theoretical terms and the question of the interpretation of theoretical terms could be treated as one. The preceding discussion has been directed towards refuting this view.

As such, specifying the way in which theoretical terms function in a theory does not suffice to show that the question of how theoretical terms are to be interpreted can be ignored.

Israel Scheffler, in The Anatomy of Inquiry, carefully distinguishes between the two problems of function and of interpretation. ([70]). Scheffler sees the problem of function as follows. Given that a purely observational language capable of dealing with the entirety of science is impossible, it becomes necessary to allow the introduction of non-observational elements into the language of science. By a purely observational language is meant one in which the nonlogical predicates are all observable. A predicate is said to be (or to designate) an observable in the case of a natural language if its applicability in any given situation is readily decidable. In the case of an artificial language we assume that some set of predicates is arbitrarily specified as being observable. In order to maintain the distinction between science and nonscience, there must be some method of limiting the admissible non-observational predicates. Thus, whereas we should require that "is an electron" be an admissible predicate, we should like to be able to say that "is glubbified" (to use Scheffler's example) is not an admissible predicate.

One way of distinguishing admissible from non-admissible predicates is by specifying what function these non-observational or theoretical predicates are to fulfill

in a scientific theory. An example of what this function might be is that theoretical terms are to serve as links in connecting up diverse observational or experimental laws which, on the face of it, are not related to one another. Theoretical terms function in this way to unify our scientific knowledge and make it more comprehensive. The predicate "is an electron" has a function in physics in this sense, due to the fact that it occurs in theoretical laws which have a comprehensive scope. Similarly, "is glubbified" is functionless in this sense because it does not occur in any laws which have a wide comprehensive scope. Presumably, at some time in the future, it may become practical to introduce laws in which the predicate "is glubbified" occurred, and in that case the predicate would become significant for science.

It is obvious, however, that specifying the function of theoretical terms in science leaves wide open the question of how we are to interpret sentences in which they occur. For example, "The cat is black," a sentence which contains only observational predicates, is easily interpreted as a description of some state of affairs. Consider the sentence, "The electron spin = $\frac{1}{2}$." Can we treat this as a description or not? The argument in favor of not classifying it as descriptive is that, although it in conjunction with some laws implies some statement which is descriptive, i.e., "The X-meter reads 0.7," which we may more or less directly verify, we are not in a position to

directly verify what the theoretical statement itself is supposedly descriptive of, i.e., the state of the electron. It is a commonplace to argue that "The electron spin = $\frac{1}{2}$" is not descriptive of anything, i.e., that the ontological status of "electrons" is different from the ontological status of "cats."

The argument for considering "The electron spin = $\frac{1}{2}$" as descriptive of some thing (i.e., the electron) is the feeling that insofar as science is to provide us with "an effective systematic account of the world," then the concepts used must have an "intuitive clarity," i.e., must be of the same nature as those concepts which we use to describe the ordinary everyday world. ([70], p. 178) This is tied up with the view that science affords us a systematic account of the world, in not only explaining how things happen but also in describing the world as it "really is." Theoretical terms, on this view, extend our descriptive powers from the readily accessible to the less accessible. From this point of view, we may say that the theory of electrons not only functions to provide a comprehensive framework for our observational results but also provides a tentative description of the world. We see then that solving the problem of the function of theoretical terms does not solve the problem of how they are to be interpreted. In particular the question of the ontological status of these entities remains wide open.

Following Scheffler, we may initially distinguish two major variant positions on the interpretation of theoretical entities. Scheffler labels these Pragmatism and Fictionalism. Fictionalism is then subclassified according to whether it is "instrumentalistic" or "eliminativist." ([70], p. 181) Given that a purely observational language for the whole of science is impossible, Scheffler asks whether it is possible to construct an Empiricist Language which is both (1) significant throughout and (2) capable of expressing all of science.

There are two senses of significance that Scheffler distinguishes: (1A) intuitive clarity, and (1B) provision of an effective systematic account.

The Pragmatic position reduces the question of the significance of theoretical terms to the question of determining whether or not they are functional. Pragmatism also construes sentences in which theoretical terms occur as capable of being true or false. ([70], p. 182) The Pragmatist feels that it is possible to construct an Empiricist Language satisfying (1B) and (2).

The Fictionalist position reduces the question of the significance of theoretical terms to determining whether they are intuitively clear. The Fictionalist then points out that the satisfying of (1A) leads to the impossibility of satisfying (2). That is, some theoretical terms will be necessary in order to fulfill (2) which will be unintuitive and hence violate (1A). Hence, the statements in

which these theoretical terms occur will be literally non-significant. It follows that the Fictionalist does not feel that it is possible to construct an Empiricist Language satisfying both (1A) and (2). For the Fictionalist, ". . . a functional portion of science falls outside [the Empiricist Language], resisting construal as literally significant." ([70], pp. 183, 185) The upshot of this is that theoretical statements which are not literally significant are incapable of being true or false. The question remains how we are to interpret such statements. One attitude, which Scheffler calls Eliminative Fictionalism, seeks to eliminate such sentences from science through the use of logical devices, such as Craig's Theorem or Ramsey sentences. ([70], p. 183) Another attitude, which Scheffler calls Instrumentalistic Fictionalism, does not seek to eliminate the nonsignificant portion of the language of science, but rather interprets this portion as mere machinery for working with the significant portion.

Scheffler claims that both Instrumentalistic Fictionalism and Pragmatism, as here defined, are similar in spirit although differing in terminology. For one thing, both Pragmatism and Instrumentalism (except in a trivial sense) deny the thesis of empiricism (ET). ET is the claim that the language of science can be made thoroughly observational. (The Eliminative views, insofar as they succeed in eliminating those portions of science that refer to theoretical [or unobservable] entities are in accord with

ET). The Pragmatic view denies the ET by allowing sentences containing theoretical terms into the language of science on the same basis as any other sentences, i.e., they are capable of being true or false. Instrumentalism is in trivial agreement with ET insofar as it denies that theoretical statements are capable of being true or false. In this trivial sense, the Instrumentalist holds that the only literally significant portion of the language of science is observational. However, the Instrumentalist admits the use of statements which are not literally significant into the language of science. Although these are not statements in the sense of being either true or false, they do have a certain "functional utility."[3]

The fact that the Pragmatist likes to think of the functionally utile portions of science to be true or false whereas the Instrumentalist does not, represents according to Scheffler, a decision on their part to talk about the language of science differently; it does not represent a major difference in program. ([70], p. 187)

On the basis of this consideration, I think the similarity between the two views can be put more strongly. It seems to me that both Pragmatism and Instrumentalism,

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[3]Since I propose to conflate all these positions, Pragmatism, Fictionalism, and Instrumentalism, and call them all instrumental, I will not deal any further with these eliminative views. The alternative I will propose will be a realism which will be spelled out in the following chapters. Cf. E. Nagel, Structure of Science, p. 129.

as Scheffler outlines them, remain uncommitted on the question of the ontological status of theoretical entities. Clearly, from an Instrumentalistic point of view, theoretical terms have only a functional utility, and do not have any ontological reference. The Pragmatist, insofar as he "emphasizes function, utility and system in science" is in agreement with the Instrumentalist. ([70], p. 187) Therefore, I propose to conflate the two views into one, which I will call instrumentalism and which I will oppose to a view I will label realism.

Summary. I have tried to show in this chapter that the typical instrumentalistic moves to abandon the question of the ontological structure of theoretical entities are unsuccessful. I think this lack of success can be traced to a lack of vision on the part of the instrumentalists. Only insofar as one construes science solely or mainly as the attempt to achieve systematic predictability of observable phenomena does the move to de-ontologize theoretical phenomena seem valid. But, as I suggested earlier, and argue more fully later, this is to construe science very narrowly. Science also aims at the provision of explanations which serve to make the total range of phenomena, both observable and unobservable, intelligible to us.

The instrumentalistic critique in the form discussed here is too sweeping. It indiscriminately packs together all sorts of theoretical constructs. No ontological distinction is drawn between ψ-functions, electrons, microbes,

genes, fields, or lines of force. It seems to me that some sort of discrimination is possible. Thus, I want to argue that whereas some theoretical terms may be merely instrumental to the end of prediction and control of observable phenomena, there are other theoretical terms which do not merely function in this way. Theories which include these other terms are theories which purport to be descriptive, i.e., "true of" the world. Insofar as these theories do purport to be descriptive, certain of the theoretical terms which occur within them can be said to designate "real" entities, in the same sense that this is a real desk upon which I write. Exactly how this distinction is to be drawn and what, if any, import it has for our understanding of scientific theorizing, remains to be seen.

It might be argued that in breaking down the distinction between theoretical terms and observational terms, instrumentalism has been vanquished. The instrumentality of theoretical terms has been maintained on the basis of maintaining that there is a sharp difference in <u>kind</u> between those terms which designate observable, everyday, macroscopic entities and processes and those terms which designate theoretical entities and processes. The argument of this chapter has been to show that there is no difference in kind. Thus, theoretical entities and processes ought to be treated on a par with observable entities and processes. If this is the case, two options seem open. We could argue that as the concepts at the theoretical level are instrumental

so are the concepts employed at the observable, everyday, macroscopic level. Thus, as concepts like "electron" and "gene" are instrumental, so are concepts like "meter," "table," "chair," and "mountain." One option, then, is a thoroughgoing instrumentalism or phenomenalism about both the theoretical and everyday observable world. Insofar as this position slides over into phenomenalism, it seems bankrupt. ([77], pp. 60-105)

The other option is that if we admit that ordinary, everyday, observable entities and processes are real, then we may accord the same admission to the theoretical entities and processes referred to by well confirmed scientific theories. Insofar as theoretical entities and processes are conceived as analogical extensions of observable entities and processes, the gap between "what we see" and "what we infer" is bridged. Thus, the physical models of Chapter II play an essential role in our progressive scientific understanding of the world. The obliteration of the sharp distinction between theoretical terms and observational terms is, prima facie, a powerful argument for realism and against instrumentalism.

I shall argue later (in Chapters V and VII) that there is possibly a third option, which combines the spirit of realism with the possibility that well confirmed theories may contain instrumental elements. Thus, insofar as science provides us with an understanding of the physical world, it progresses by replacing instrumental theories with realistic

theories, or at least with theories with a minimum of instrumental aspects. This third option rests on establishing the plausibility of drawing a distinction between what is real and what is instrumental within a theoretical framework to which problem we now turn.

CHAPTER V

Scientific Realism

1. Introduction. Having examined some instrumentalist positions concerning the ontological status of theoretical terms, it is natural for us to seek out and examine some realist positions. Before we do so, however, it behooves us to attempt to clarify exactly what is at stake. This we do by trying to put the controversy between realistic and instrumentalistic interpretations of scientific theories into sharper focus. What is meant, for example, by saying that scientific theories describe, or attempt to describe, or are "about" the real world? What is meant by saying that some, or all, of our scientific theories are instrumental? Or that they are merely instrumental? Whatever the answers to these questions and others like them, it seems that the two terms "real" and "instrumental" as applied to scientific theories or terms cannot be treated separately. They stand or fall together, hence, an analysis of the one must be, at the same time, an analysis of the other.

It is the purpose of this chapter to provide such an analysis or at least the prototype of such an analysis.

Bertrand Russell once said that "The law of causality . . . like much that passes muster among philosophers, is a relic of a bygone age, surviving, like the monarchy, only because it is erroneously supposed to do no harm."

([68], p. 174) One might argue that the same could be said for the concept of reality. To characterize science as the quest for the real or as the attempt to discover (or uncover) the nature of reality is to pay lip service to characterizations which are hopelessly out of date. Ernest Nagel goes so far as to suggest that we not use the term "real" at all. ([60], p. 157)

I disagree completely with Nagel's assessment of the controversy between realists and instrumentalists. In particular, I do not accept the view that no viable distinction can be drawn between instrumentalism and realism. In my opinion, the notion of the "real" is not merely a "relic from a bygone age" but deserves serious philosophical attention. I think that clarification of what we mean by assertions of the form "X is real" have profound implications not only for the philosophy of science but for philosophy in general. Within the scope of the following discussion, these more general considerations can only be alluded to. Their development is beyond the scope of the present work. However, I do not think that the solution to the "question of reality" is a simple one, nor do I feel that what I have to say is in any sense complete. At best, it serves to point in a direction of inquiry that I believe will prove to be very fruitful.

In the next section, I want to try to cut the problem of the real down to manageable size. I do this by discussing the line of attack advocated by Rudolph Carnap

in his article, "Empiricism, Semantics and Ontology" (hereafter, ESO). ([16]) ESO, I feel, contains many valuable insights although I think it is muddled in places and downright incorrect (or inadequate) in several essential respects. The implications of Carnap's notion of an "alternative linguistic framework" for the problem of reality are explored and Carnap's "easy" ontological solution is discussed and then rejected. The idea of alternative linguistic frameworks set up in accordance with diverse aims and purposes leads into a discussion of the general role of aims and purposes in delimiting the real. A separate section is devoted to the question of the aims and purposes of scientific linguistic frameworks. Finally, a realist view by Grover Maxwell which derives from Carnap's work is discussed and points of agreement and disagreement between his view and mine are noted. Maxwell's criticisms of Nagel are discussed and accepted with at least one crucial emendation.

The net result is that I attempt to show how a distinction between terms which are (or designate) real entities and those terms which are merely instrumental can be validly drawn within a linguistic framework L. Thus, Carnap's "easy" solution to ontological questions is rejected and it is argued that these questions are neither trivial nor unenlightening.

2. <u>Alternative Conceptual Frameworks</u>. Carnap, in a very

influential article, for realists at least, attempts to divide the problem of reality into manageable parts.[1] In certain circumstances, the question "Are ϕ's real?" is trivially and obviously to be answered in the affirmative. In other circumstances, the question, "Are ϕ's real?" is a pseudo-question which admits of no true or false answer. To say that ϕ's are real in this second sense is merely to choose certain ways of speaking over others. Thus, no matter what standpoint you choose, the question of the ontological status of entities involves no more than pragmatic considerations. This analysis deserves our careful consideration.

The basic notion in ESO is the concept of a linguistic framework. Carnap's primary concern seems to be to provide an account that makes sense out of abstract linguistic (or semantic) entities such as the elusive "proposition." However, he does point out that his method and analysis is applicable to all sorts of linguistic frameworks, including languages for mathematics and physics.[2]

What, then, is a linguistic framework? Nowhere in ESO does Carnap explicitly answer this question. Early in the article, he indicates that a linguistic framework is to be understood as a "system of linguistic expressions."

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[1]Carnap ([12], p. 217). G. Maxwell calls it a "classic essay" in his article, "Theories, Frameworks and Ontologies" [57], p. 132.

[2]Carnap ([12], p. 206). See the discussion of Maxwell's views in section 4 below.

([16], p. 205 n.) Other places he seems to equate it with a "language." ([16], p. 206) There is an ambiguity in the concept of a language; it may refer to a set of linguistic expressions only or to a set of linguistic expressions plus terms and formation rules. The former we may call a language in the narrow sense, the latter, a language in the wider sense. It seems that we are to take "linguistic framework" as referring to a language in the wider sense. ([16], p. 213) As such the linguistic framework will contain both extra-logical and logical terms. Among the extra-logical terms will be constants which designate individuals, predicates (which may be taken as designating classes of individuals, or higher order predicates which would be classes of classes of individuals, etc.) and variables for both individual and predicate terms. In addition, rules must be specified for the manipulation of these terms, through use of which we may construct expressions. These may be called, generally, syntactic rules of expression formation and transformation. If the linguistic framework is to have an interpretation, then there must be rules for interpreting the symbols and hence the expressions which occur. These may be called, generally, semantic rules. All talk <u>about</u> the linguistic framework is to be conducted in the context of another framework which serves as a meta-language for the primary linguistic framework.[3]

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[3]For more detail on the construction of artificial languages and their structure, see ([14]).

Finally, we assume that associated with the framework are certain confirmation procedures which indicate, within the framework, which sentences are to be accepted and which are to be rejected. ([16], p. 207) These procedures may or may not be formalized, although Carnap assumes that some "rational reconstruction" of any set of intuitive confirmation procedures is possible. ([16], p. 207)

Can we learn anything from examples that Carnap cites? In section 2 of his paper, Carnap discusses several different frameworks. ([16], pp. 206-13) The two frameworks which are of most interest to us in our present investigation are the thing-framework and the spatio-temporal coordinate-framework. ([16], pp. 208, 212) The thing framework allows us to speak of "the spatio-temporally ordered system of observable things and events." ([16], p. 207) Within this framework we may ask questions about desks and tables, pieces of paper, Napoleon, unicorns, the birds and the bees, big rock candy mountains, and the like. Within this framework, we may ask whether these things or events are real or merely imaginary. ([16], p. 207) The answer we give will depend on the confirmation procedures that are associated with the framework. If we accept that the thing-framework is the framework of ordinary discourse, then these confirmation procedures may be only partially explicit or in some cases, mere rule of thumb procedures. However, "the concept of reality occurring in these internal questions is an empirical, scientific, non-metaphysical

concept." ([16], p. 207)

Consider, alternatively, the spatio-temporal coordinate framework (hereafter, the STC framework). Here the entities allowed by the framework are space-time points. ([16], p. 212) With the help of qualitative ("red," etc.) and quantitative ("mass," etc.) predicates we may specify what we call the "physical state" of a spatio-temporal point or region. ([16], p. 212) Again, within <u>this</u> framework, we may ask questions about the physical states of various regions, which are specified by quadruple intervals, e.g., $(x_1 \leq x \leq x_2,\ y_1 \leq y \leq y_2,\ z_1 \leq z \leq z_2,\ t_1 \leq t \leq t_2)$. These questions are answered within <u>this</u> framework according to confirmation procedures (different from those of the thing framework) which are associated with it.

The decision to use either the thing-framework of the STC-framework is a pragmatic rather than a theoretical decision. ([16], p. 212) I am not sure I understand exactly what Carnap means by a "theoretical decision" but I presume that it means a decision which follows from our adoption of some theory or other. An example of such a decision within a given framework might be as follows. In the thing-framework we talk about colored surfaces. Then we might decide, on color-theoretical grounds, to reject the sentence "That surface is both blue and yellow all over." An appeal here is made to some doctrine of color incompatibility or the like. Similarly, making predictions within a given framework, which follow from theories within the framework,

and which are to be tested empirically, would be matters for theoretical decisions. For example, again in the simple thing-framework, if I say that I am about to mix a blue pigment with a yellow one, I can safely predict that the resulting pigment will be some shade of green.

This cannot be all of the story, however, since Carnap does allow that "theoretical considerations" may influence our pragmatic decision to adopt one framework over another. ([16], p. 212) An example of a case like this is our decision to use real numbers for space-time coordinates rather than rationals because of the simplicity that is achieved by our ability thereby to use the mathematical calculus. ([16], p. 212) A second example of a theoretical consideration of this kind is involved in our decision, with respect to the STR-framework, to specify the point coordinates as spatially three dimensional. This follows, Carnap allows, from "common observations." ([16], p. 212) The net result is that it is not at all clear just what <u>is</u> intended as a "theoretical consideration." In particular, just why these examples taken from ESO are "theoretical considerations" rather than "pragmatic considerations" is unclear. This unclarity means that the distinction between pragmatic and theoretical considerations is fuzzy. This fuzziness in turn extends, I think, to the distinction Carnap wishes to draw between internal and external questions.

Let us return to our discussion of the two frameworks

previously considered, the thing-framework and the STC-framework. Whereas we could legitimately ask within either framework about the reality or unreality of entities occurring within them, we cannot legitimately ask that question of the frameworks themselves. Thus, to inquire, "But which of these two frameworks tells us what is really real?" is, from Carnap's point of view, a pseudo-question. If we choose one framework, that framework confers ontological respectability onto the kinds of entities discussed within that framework. If we choose the thing-framework, then things and events are real. Which particular things and events are real (Napoleon, Santa Claus, or the Fall of the House of Usher) is an internal question to be answered in accordance with the accepted procedures of the (in this case) thing-framework. If we choose the STC-framework, then space-time points and physical states are real. Again, which particular physical states are real (actual?) is an internal question to be answered in accordance with the accepted confirmation procedures of the STC-framework. But, to step outside either framework and ask, "Are things the real entities or are spatio-temporal points and physical states the real entities?," is to ask a question for which there is no legitimate answer.

Taken in this external sense, questions about reality are matters of practical, rather than theoretical, decision. To say that things are real, in an external sense, is merely to say that we choose, for various reasons, to use the

thing-framework. Other reasons may crop up which would "gently persuade" us to adopt the STC-framework. In this case, spatio-temporal points or regions and the physical states associated with them would be real (in an internal sense).

One question that arises is how extensive or comprehensive these frameworks are to be taken? Is the whole of science (whatever that may encompass) supposed to constitute, at least in principle, a single framework? Or are we to take "fields" of science as each having its own framework, i.e., one framework for physics, another framework for biology and so on? Or are we to consider that individual theories each constitute a separate framework, so that we have one framework for Newtonian mechanics, one framework for the Special Theory of Relativity, and so on?

At one point, Carnap remarks that "To be real in the scientific sense means to be an element of the system; . . ." ([16], p. 207) This suggests that science as a whole is to be considered as one framework. Indeed, it would hardly seem possible to treat each individual theory as a separate framework because that would seem to entail that the choice between two theories would be merely a pragmatic question.

There are certainly pragmatic issues involved in the choice between accepting alternative theories. These pragmatic considerations are reflected, in part, in the confirmation procedures which are used to determine the

acceptability of a theory. To say that a theory must be well-confirmed in order to be acceptable is not enlightening unless one has some idea of what "well-confirmed" means. At what point it is decided that the evidence and other considerations supporting a theory T are sufficient to warrant saying that the theory is well-confirmed is clearly not a theoretical issue, but a pragmatic one.

However, even so it seems wrong to say that the only considerations in choosing one theory over another, say for adopting the special theory of relativity over Newtonian mechanics, are pragmatic considerations. But, insofar as theoretical considerations are to be identified with internal questions, this suggests that specific theories be formulated within a common framework, with respect to which, theoretical considerations are internal considerations.

This result seems confusing. For a question of the form "Is theory T well-confirmed at time _t_?" is an external question with respect to the theory T, but an internal question with respect to the linguistic framework within which T is formulated. In addition, if alternative theories can be formulated within the same framework, then one must consider external questions as questions external to the framework as a whole. In the case here under consideration, where the framework is the framework of science, such a question would amount to a pragmatic question about the desirability of adopting a scientific point of view at all.

Finally, it seems that one must also consider questions which are internal to a specific theory T. If T = Newtonian mechanics, then one typical question of this kind would be, "Does that mass have velocity = v?" This is clearly an empirical question which is decided in accordance with the confirmation rules of the framework within which T is formulated.

The result is that whereas there are considerations which mitigate against identifying specific theories with separate frameworks, there are also problems generated if frameworks are taken to be such that alternative theories can be formulated within the same framework. This leads to some complications concerning the "external-internal" question distinction which is central to Carnap's proposals concerning ontological issues.

This brings us to the heart of the matter--that is, Carnap's distinction between internal and external questions with respect to linguistic frameworks. Since I think it very likely that this distinction will not suffice for the neat resolution of ontological questions that Carnap desired, it will pay us to examine this distinction in more detail.

The basic difference between internal and external questions with respect to linguistic frameworks is set forth by Carnap in the following passage:

> we must distinguish two kinds of questions of existence: first, questions of the existence of certain entities of the new kind <u>within the</u>

> framework; we call them internal questions; and second, questions concerning the existence or reality of the system of entities as a whole, called external questions. ([16], p. 206)

The first thing that strikes us in this passage is that internal questions are defined as "questions of the existence of certain entities of the new [my italics] kind within the framework." What about the old kinds of entities already capable of being referred to "within the framework"? Part of the answer seems to be that Carnap always talks about the "introduction" of new entities and the "construction" of linguistic frameworks for the new entities. ([16], p. 206) But, surely, new frameworks are not created out of the air, they are constructed within already existing frameworks.

Call such an antecedent framework L. Let L_1 be the new framework constructed in the context of (or from the ashes of) L. Presumably L_1 will certify certain entities that L does not, otherwise there would seem to be no point in constructing L_1. Call these new entities Φ's. How are we to understand internal questions with respect to these entities? Internal with respect to L_1 or with respect to L? The answer we give will depend, I should think, on whether or not we assume that L survives the birth of L_1.

Our primary concern, recall, is with theoretical entities that occur in scientific contexts. Consider your favorite theoretical entity, e.g., the atom (or the virus or the gene, or what have you). I think that it would be safe to say that the construction of linguistic frameworks

adequate for the expression of sentences about atoms, or viruses, or genes, did not entail the dissolution of the parent framework.[4] Since these are the kinds of cases which most interest us, we may assume, for our purposes at least, that L survives.

Given that L survives and is somehow connected with L_1 (perhaps L_1 should be called a sub-framework of L), to which framework should we look when we want to determine (internally) whether ϕ's are real or not? It seems clear that internal questions about the reality of ϕ's are to be considered with respect to L_1. Of course, any question which is internal to L_1 is <u>also</u> internal to L, if L_1 is a sub-framework of L. But, remembering that the converse is not, in general, true, i.e., that all questions internal to L are not necessarily also internal to L_1, will help to keep matters clear.

Trouble seems to brew, however, when we consider the "external" questions. Again, if L_1 is the framework within which ϕ-expressions appear and whose limits determine what questions are internal questions about ϕ's, then it seems that it also should be the framework outside of which

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[4]Not quite so safe perhaps. There are some empiricists(!), e.g., Paul Feyerabend, who argue that the constructing of new theories essentially involves the wholesale replacement of one ontology by another. This would amount to treating each theory as an individual framework. The main considerations for a change in theory appear to be pragmatic for Feyerabend. See [31]. Feyerabend's views are highly controversial. For an illuminating discussion of some of the major problems involved in his approach, see [78]. For a general discussion of the complex epistemological issues involved, see [74].

questions about Φ's are deemed to be external ones.

The situation then is as follows. Suppose that L is some language which contains an attenuated version of physics without, say, the atomic theory. Certain considerations, the exact nature of which need not detain us here, lead us to extend our framework L by constructing a new framework L_1 within which atomic theory can be formulated. Certain expressions in L_1 will contain the term "atom." We then ask, "Are atoms real?" Within the context of L_1, this is an internal question, with an obvious answer (according to Carnap), namely, yes. Within the wider context of $L-L_1$ (that part of L which does not contain L_1) the question is an external one. The answer to this external question must take the form of an affirmative or negative decision to utilize the framework L_1. The ontological status of atoms is not at issue in this case.

So far so good. However, recall that when we first considered the notion of a linguistic framework we decided that the whole of science or at least the whole of physics constituted the framework with respect to which ontological questions about the status of scientific entities were external or internal. Recall Carnap's words, "To be real in the <u>scientific</u> sense is to be an element of the system" [my italics]. ([16], p. 207) This suggests, if we assume for simplicity's sake that physics is all of science, that L and <u>not</u> L_1 is the framework which defines which questions about Φ's are internal or external ones. But now we seem

to be in real trouble because there is a sense in which the decision to extend L to L_1 (in our atomic theory example) is tinged with both "theoretical" (whatever that may mean) and pragmatic considerations. But if this is so, and if questions about the reality of Φ's framed in the context of L are internal questions, then it is hard to see how Carnap can argue that the answers to such questions are "trivial and obvious." ([16], p. 217)

This is an ambiguous point in Carnap's treatment. Ostensibly, external questions of existence with respect to a framework are questions about the existence of a system of entities. But insofar as these questions, for Carnap, are ill-formed, they have no "ontological import." Thus, if one considers the thing-framework of physical objects like chairs, tables, desks, etc., then the external question, "Are physical objects real?" is not a question concerning the reality of physical objects as such, but is, in fact, the question "Should the physical object thing-framework be adopted or not?"

Supposing that the physical object or thing-framework has been adopted, then we may, within the context of the framework, ask questions of the form, "Is this or that a physical object?" The answer to this internal question is an empirical matter and to be decided in accordance with the confirmation procedures of the framework. The answer to questions such as these are neither analytic nor trivially obvious.

Similarly, if we are considering the framework of theoretical physics, then the internal question "Is this an electron?" is, of course, a matter of empirical confirmation. In what sense, then, are the answers to internal questions about reality "trivial and obvious"?

Suppose, instead of the question "Is this an electron?" we consider the more general question, "Are there electrons?" or "Do electrons really exist?" As questions external to any framework within which an electron theory is formulated, these questions are, according to Carnap, ill-formed. They admit of no theoretical answer, because as external questions, they are not theoretical questions but pragmatic questions.

As an internal question, "Are there electrons?" can be taken in two ways, and therein lies part of the problem in Carnap's treatment. One way to consider the internal question "Are there electrons?" is as a theoretical question. By this is meant that the answer is "yes" if, in fact, there is a well-confirmed theory of electrons formulated within some more general scientific framework. Of course, such questions cannot be answered in either an analytic or trivial manner. However, there is a second sense, in which the answer to the internal question "Are there electrons?" is analytic and trivial.

Suppose that, in fact, there is a well-confirmed theory of electrons within a scientific framework. Then, if the question "Are there electrons?" is construed as the

further question, "Granted there is a well-confirmed theory postulating the existence of electrons, but are electrons real?" it is to be answered trivially and obviously, in the affirmative. In other words, all it means to say that electrons are real is that they are postulated by a well-confirmed theory. This position is developed more fully by Grover Maxwell, and is discussed and criticized below (section 4). Thus, there are, in this case at least, two senses of the internal question "Are electrons real?" one of which demands an empirical, matter of fact answer, the other of which demands a trivial, affirmative reply.

One may account for the twofold nature of the internal question, "Are there electrons?" by distinguishing those questions which are internal to a scientific framework but external to the theory of electrons (in which case the answer is an empirical contingent matter) from those questions which are internal to the theory of electrons itself (in which case, given that the theory is well-confirmed, the answer is trivially, yes).

The second sense of the internal question "Are there electrons?" has particular import for the realism-instrumentalism controversy. Carnap's position that the answer to this question, given the fact that a theory is well-confirmed, is analytic and trivial collapses realism and instrumentalism together. That this is Carnap's position seems evident from the following passage: "In fact, however, all that can accurately be said about atoms or the

field is implicitly contained in the physical laws of the theories [my italics] in question." ([16], p. 211)

There are two things to be noticed about this remark. In the first place, the fact that it is the "physical laws of the theories" which determine all there is to know about the atoms suggests that the framework, within which questions about atoms are internal, is the atomic theory itself. This leads us right back to the beginning of our discussion where we rejected the view that each individual theory constituted a separate framework. This view entails that the replacement of theories is not an empirical question at all but solely a pragmatic question. This seems to be a strange garden path on which to find an empiricist. If this were the correct interpretation, it would seem that Carnap had not managed to recover the empiricist baby which had been thrown out with the metaphysical bathwater by the early logical positivists.

In the second place, Carnap here adopts a position on theoretical entities which greatly resembles Braithwaite's instrumentalistic view. In fact, for Carnap, any entity (theoretical or not) is real if and only if it fits into a framework of expressions which connect it up with other entities which are recognized as real, according to the rules of the framework. This view we have already implicitly rejected as involving a conflation of the functionality of theoretical terms with their ontological respectability. We reject the maxim that whatever is functionally utile is

real (as opposed to merely instrumental). Thus, Quine's formula, "To be real is to be the value of a variable" is not the final word on ontology, just because it fails to make the distinction betweeen what is (is real) and what is (is merely instrumental).[5]

Despite these reservations about the exact way in which the internal-external question distinction is to be drawn, I think the basic insight is, in an important sense, a sound one. The same is true, I believe, of the notion of linguistic frameworks. Carnap does not say much about the pragmatic considerations which go into determining what frameworks we will accept, but he does point out that the purposes for which the linguistic framework is to be used will to a large extent determine what factors we will take to be relevant for making our decision. ([57], pp. 135ff.) This, I think, is a very significant insight and its implications deserve wider consideration.

Any framework is designed to serve certain purposes. As such, what kinds of entities it countenances will be determined in large measure by these purposes. What sorts of things would serve as likely purposes for particular frameworks? On this point, Carnap is unfortunately, in Maxwell's words, "tantalizingly terse." ([57], p. 135) The only example of a purpose that Carnap offers is "the purpose of communicating factual knowledge." ([16], p. 208)

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[5]One method for drawing this distinction is discussed in the next two sections, pp. 117-138.

This is unfortunately very unhelpful unless we have some idea of what the "facts" are. But, what counts as factual, like what counts as real, is surely relative to given frameworks. If this is so, then it is "trivial and obvious" that any accepted framework enables us to communicate factual knowledge. If we step outside the framework and ask, "But is what this framework enables us to communicate really factual?" I do not know what kind of answer would be appropriate. Grover Maxwell notices this problem and likens the procedure whereby we decide which frameworks to accept (or in his words, how to modify a framework) to a "bootstraps" operation. ([57], p. 138) I think that Maxwell is right on this point. I tried to suggest the same sort of difficulty in the above discussion about "external" and "internal" questions. However, I think that Maxwell, like Carnap, has seen only a partial truth.

We must admit, it is clear, that there are different ways of speaking and that which way we choose to speak depends upon our purposes. Instead of alternative linguistic frameworks, I propose that we talk of alternative conceptual frameworks. Each conceptual framework has its own set of rules and procedures. Each conceptual framework is adequate for certain purposes and inadequate for others. Since we have many purposes in speaking, we ought to be willing to live with the fact that we may need to use different conceptual frameworks. Where I would diverge from Carnap and Maxwell is in their restriction of the

discussion to alternative scientific frameworks. It seems to me that science, being only one human activity among others, may not serve all the purposes which human beings have.[6]

Insofar as this is true, we may have to recognize the legitimacy of non-scientific frameworks for certain purposes. Clearly what is countenanced by a given framework will depend on what purposes that framework is designed to serve. Insofar as we are here primarily interested in scientific frameworks it behooves us to inquire into the aims and purposes of science. If it turns out, as very likely it will, that there is no one set of aims and purposes which fit all activities we would want to call scientific, then we must live with this fact and take it into account when we evaluate the significance of expressions which occur within scientific frameworks. This is another reason why I cannot agree that internal ontological questions are trivial and obvious.

As examples of alternative conceptual frameworks which may not be designed to do the job of science (whatever that may be) we may consider religious frameworks or metaphysical frameworks. I am not sure exactly what purposes one might associate with such frameworks but we need only assume that at least some of them are different from

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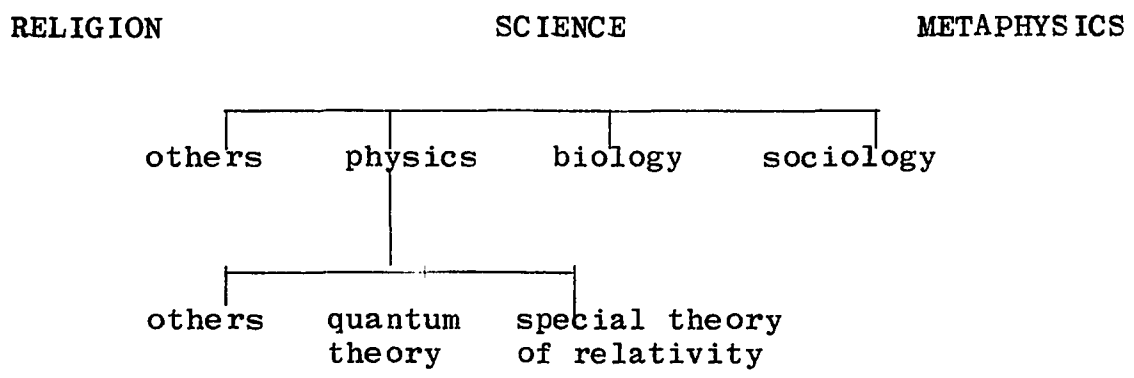
[6]I do not mean to suggest that Carnap and Maxwell do not also believe this, just that they do not discuss it in the context of these articles.

those purposes for which we do science. Of course, one might argue that such frameworks are of no concern to us, since whatever they do, they do not convey "factual knowledge," i.e., they are noncognitive frameworks. And while we would not want to deny that language has non-cognitive purposes, we are only interested in frameworks which are at least cognitive frameworks.

But of course this will not do. It will not do because we have implicitly assumed that what is scientific is identical with what is cognitive and nothing else is. This formula may have been acceptable to the positivists circa 1920, but it will not suffice today. And if we reflect upon it for a moment, it seems that Carnap ought to agree. For to say of a framework that it is cognitive is to make an external assertion about the framework. As such it is not a theoretical judgment, based on some kind of evidence or scientific theory, but merely a pragmatic one. Furthermore, if we remember that each framework <u>defines</u> what is factual within it, it is <u>trivially true</u> that any framework can be used to convey factual knowledge. To say of a framework that it is cognitive is only to say that it can be used to convey factual knowledge, but what justifies the further restriction of the appelation to scientific frameworks alone? The price of alternative frameworks seems to be inflationary, and may well be more than Carnap wants to pay. Of course, the motivation for the restriction is the positivistic view that the only "genuine" statements

are scientific statements. But once it is recognized that "genuine" is an external and not an internal consideration, all that was once supposedly gained is lost again.

Now if we accept this generalization of the notion of alternative linguistic frameworks, it is clear that the internal-external question distinction gets very complicated. The net result is that again the triviality of internal questions is open to dispute. Even if we decide that internal questions are still trivial (once we have decided just which questions are internal) the least that seems to follow is that the significance of those trivial answers is open to various interpretations. One might sketch the situation roughly as follows:



This diagram is only a rough representation of what seems to be a very complicated situation. Thus, we have placed Religion, Metaphysics and Science roughly on a level with one another as (again roughly) independent conceptual frameworks. Some might want to argue that Metaphysics ought to be a supra-framework subsuming both Science and

Religion underneath it. Of course, there may be other (roughly) independent frameworks on a par with those shown above. Another point is that these three major categories are by no means ultimate but that it would probably be possible to construct an unending series of frameworks both above and below those shown. If we then concentrate on Science, are we to accept physics and biology as alternative frameworks as well as quantum theory and the special theory of relativity?[7] If so, what is their inter-relation? Do they all serve the same purposes? Compare geography with molecular physics.[8]

How are we to decide what the limits of a framework are? Everything we have said so far points to purposes as the delimiting factors. Different purposes, different frameworks. But, then, what sorts of things are to count as purposes? The purpose of conveying factual knowledge we have seen to be common to all frameworks. We cannot distinguish individual frameworks, e.g., Science, from the others by appealing to a cognitive-noncognitive distinction without circularity.

Let me put this point a slightly different way. Empiricists, in choosing Science versus other frameworks as

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[7]Of course I do not mean to suggest that Religion and Metaphysics may not be similarly divided. But this is not our present concern.

[8]For an interesting discussion along these lines see Max Black, "The Definition of Scientific Method," in Sprague and Taylor (eds.), Knowledge and Value, pp. 241-57.

cognitively significant, convey their feeling that somehow Science is a privileged framework of some sort. But, of course, the question is, privileged with respect to what? In their hearts I believe that the empiricists feel that Science is a privileged framework for gathering and evaluating knowledge about the world. But when we listen to their words, it turns out that the framework of Science is only one possible among many, and fulfills only some of the purposes human beings may have in speaking. What makes these purposes such that this framework offers us cognitive (and implicitly, meaningful) information about the world whereas the others do not? Only because the empiricists define the scientific framework as the framework which is cognitive. This is blatantly circular.

Maxwell notices that within the "framework" of frameworks, "the epistemological enterprise must to some extent be circular." ([57], p. 137) But, he adds, there is nothing to be done about it but to live with it. In this he is surely correct, as I think he is about so many of the other issues concerning scientific realism.

The result is that we must carefully consider the aims and purposes of our frameworks before we can decide which to adopt or how to modify the ones we have. Insofar as science does not serve all these purposes, science cannot comprise all that is useful for us to know or what Maxwell calls the "totality of 'facts about the world.'" ([57], p. 131)

What are the implications of this discussion for

scientific realism? First, it is clear that what is accepted as real, insofar as it is framework dependent, and insofar as frameworks are purpose dependent, itself depends on what the purposes of our frameworks are. From this it seems to follow that even if we exclude inter-framework questions of reality (i.e., "Are atoms real or is God or both?"), then it is still possible to question whether or not the entities or processes countenanced by the well-confirmed theories internal to a framework are real or not. For suppose that we have a framework L designed to serve certain purposes. For simplicity's sake, let us suppose there are only three purposes, p_1, p_2, and p_3. Now it is clearly <u>possible</u> that some theories or some terms in the context of L do not serve all of these purposes. We may have some entities, say Φ's, which serve all three purposes. Others, say Ψ's, may not. In which case the question "Are Ψ's real?" is not necessarily trivially true. Of course, <u>in a sense</u>, Ψ's <u>exist</u>, but we may opt for saying that they exist only instrumentally and not (as Φ's do) really. Hence, the distinction is not drawn between <u>real</u> (= exists) and <u>unreal</u> (= does not exist), but rather between <u>real</u> and <u>instrumental</u>. And, of course, this latter distinction may very well be a matter of degree in a sense in which the former cannot be. ([16], p. 313; [57], p. 137)

How this distinction between <u>real</u> and <u>instrumental</u> for science is to be drawn depends, of course, on what we decide the aims and purposes of science are. This question

is taken up in some detail in the next section.

3. Aims and Purposes of Scientific Frameworks. Talk about aims and purposes is very complicated. We must distinguish between subjective and objective purposes. Jones's purposes in becoming a scientist are not the same, somehow, as the aims and purposes of science itself. Is it possible to determine what the intrinsic aims and purposes of science are? Given that it is possible, must any activity serve all those purposes in order to be classified as scientific? What we are asking for here, in effect, is the correct way to define the term "science." Is it possible to define science in terms of essential characteristics, e.g., a characteristic method, such that every activity which is to be called scientific has these essential characteristics? Or is the term "science" more like the term "game" for which a Wittgensteinian analysis in terms of "family resemblance" is more appropriate? ([7], p. 249)

On the one hand, it seems rather arbitrary to single out certain features of disciplines which are generally accepted to be scientific and define science in terms of them. On the other hand, it seems that there is no clear-cut set of features which in fact are characteristic of every activity we actually label as scientific. The problem of determining the "correct" definition for a term is always a difficult one and the more general the term to be defined the more difficult the problem becomes. There is a method

for dealing with these situations. That is, to replace the indefinite term by a rationally reconstructed term, which while gaining in precision must, as a consequence, lose something. Hopefully, what is lost is not too important or essential. But of course, this is always a pragmatic question.

If we take this route we must, of course, adhere as closely as possible to ordinary usage, realizing that complete fidelity is impossible. What we must be careful of is prejudging issues by the way we select defining characteristics. Thus, we could always specify the aims and purposes of science in such a way as to preclude the possibility of drawing the kind of distinction we want to between what is real and what is only instrumental. On the other hand, we could specify the aims and purposes of science in such a way that the distinction neatly falls out. Our only court of appeal is ordinary usage and pragmatic justification. The result is we can never say that the particular reconstruction we choose is the only possible or even the only plausible one.

A view that enjoys wide circulation among contemporary philosophers of science is that the central aim and purpose of scientific inquiry is prediction, and thereby control over what we are able to make predictions about.[9] An alternative view is that the central aim of science is to

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[9]Although there are critics of this view. See [72].

provide explanations. Predictivity and (implicitly) control are necessary conditions which must be satisfied by any account which purports to be a scientific explanation, but they are not enough. An essential ingredient of an adequate scientific explanation is also understanding or intelligibility. If we consider the framework of science then we would say, on this view, that the primary aim of science is to provide such explanations. Any devices we use towards this end, e.g., theoretical postulates or theoretical entities are, in a sense, only instrumental to these ends.

This is generally true of any linguistic framework. Suppose L to be some such framework. Then L will serve certain purposes. Then, any terms introduced into the framework in accordance with these purposes will be instrumental, in the very trivial sense that they are only means toward certain ends. If the framework is the framework of science, then the terms in that framework will be instrumental towards the provision of explanations. If we may borrow Carnap's terminology for a moment, we may say that from an <u>external</u> point of view the contents (i.e., terms and expressions and rules) of any framework are <u>only</u> instrumental. But, it seems to me that we may fruitfully distinguish this question from the <u>internal</u> question of (if Φ is the term we are interested in) the instrumentality versus the reality of Φ's.

Suppose our language framework L to be the framework of science. Then, one purpose (p_1) might be

"predictivity," another (p_2) might be "control" and still a third (p_3), might be "understanding" or "intelligibility." Then if we claim the central aim of science is to provide intelligible explanations, each of the p_i's should be considered in the light of that aim. Granted the crudity of the approach, it seems that we can at least establish the plausibility of drawing a realistic-instrumentalistic distinction internal to the framework of science L.

It is clear, I think, that not every term introduced into L will necessarily serve all three of these purposes. If we consider a term, say ϕ, then it may serve only purposes p_1 and p_2. It is these terms which I propose to label as instrumental, in an internal sense. (For the remainder of this section, it is the internal sense of "instrumental" which is intended unless otherwise specified. It is this sense of "instrumental" which is to be opposed to "real"). An instrumentalist with respect to scientific entities, on this view, is one who holds that the only functions or purposes of science (and, hence, of scientific concepts) are p_1 and p_2. The justification for this identification can only be pragmatic, but it seems to accord with traditional instrumental approaches such as those of Pierre Duhem and R. B. Braithwaite.[46] ([22], [11])

A term in L is real, or designates a real entity, insofar as it occurs in a well confirmed theory and serves not only p_1 and p_2, but p_3 as well. Elsewhere I have argued that p_3 is served through the introduction of models with

descriptive content.[10] A realist with respect to scientific entities is then one who holds that science is characterized by all three purposes p_1, p_2, and p_3. Note that the realist is not committed to the view that all terms in L are real, in this sense, but only that some of them are. The task of science, from this point of view, is partially the task of replacing accounts which only use instrumental terms by those which use real terms. In this way the world that is described by science becomes more and more intelligible to us.

The distinction here drawn is admittedly a crude one, but I feel that it deserves serious consideration. The fact that our view only takes three purposes into account is a rather arbitrary restriction. A more enlightening account would result, no doubt, from further consideration of other aims and purposes of science. This task as well as that of investigating the implications of this discussion for the development of a general theory of alternative conceptual frameworks will not be discussed here.

However, an immediate result which is relevant for our present discussion is that the internal question of reality is no longer a trivial one. The mere occurrence of a term in a well confirmed theoretical structure such that it is possible to derive an existential assertion of the form "There are Φ's" no longer guarantees that the term Φ

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[10]In Chapter II.

designates a "real entity." It is perfectly possible for ϕ to be merely instrumental, which would be the case if we cannot provide an intelligible interpretation for ϕ by means of some descriptive model. The implications of this result for our study are developed in the next section.

4. Scientific Realism. Grover Maxwell, in "Theories, Frameworks, and Ontology" works from a Carnapian position. He adopts the view that in order to say anything we must adopt a certain linguistic framework which enables us to talk about the world in various ways. Having adopted a given framework (or language) L, we may ask such questions as "Do ϕ's exist?" where ϕ designates an entity that can be talked about in L (or with L). With Carnap, Maxwell distinguishes between "internal" and "external" questions. The question "Do ϕ's exist?" is an internal question and can only be answered on the assumption that we have adopted one L or another. It is easy to construct alternate L's such that no mention is made of ϕ's. It is an illegitimate procedure to juxtapose these two linguistic frameworks L and L' and then ask "Do ϕ's exist?" or "Are ϕ's real?" For L-users, they are (let us assume) real. Certainly, there is a sense in which, for L-users, ϕ's exist. For L'-users, they are not. Now to ask whether ϕ's are "really real," or what amounts to the same question, "Which framework is the correct one?" is to confuse an internal question with an external one. ϕ's may be real with respect

to one framework and "unreal" (?) or nonexistent with respect to the other and that's all there is to it.

By relativizing ontologies to different linguistic frameworks it appears that messy metaphysical questions are done away with. If someone asks: "what is real?" the appropriate response is to inquire, "Real with respect to what framework?" Once this is specified, it should be possible to merely read off what there is or what is real with respect to that framework, and what is not.

As I argued above, I think there is much to be said for this view. Certainly, confusion reighs if one attempts without any regard for a particular framework to list what is real and what is not. However, it is not at all clear that all the problems concerning the nature of the real are solved by this move. Before discussing the shortcomings of Maxwell's formulation of the program, perhaps it would be helpful to examine again just what is to be required in order that, with respect to a framework L, we may say that ϕ's are real or that ϕ's exist.

We must assume that we are given a framework L which we work in. It need not be a formalized language. Within this framework we may distinguish between sentences which are L-true, A-true, and C-true. A sentence is L-true if it follows from what Carnap calls the L-rules of the language. That is, an L-truth is true by virtue of the syntactic transformations which define the structure of the language. If we consider L to be such as to include the propositional

calculus, then, e.g., all instances of "p or not-p" are L-truths.

A sentence is A-true if it is analytic (has no factual consequences) but is not an L-truth, i.e., does not follow from purely syntactical considerations in L. Sentences are A-true by virtue of "meaning postulates" or conventions about how the terms occurring in them are to be used. L-truths, by contrast, are true by virtue of structural properties they possess. If L is such as to include the kinetic theory of gases, then the sentence "The temperature of a gas is equal to the average kinetic energy of the molecules of the gas" is an A-truth. It is true by convention, not by virtue of any structural property of language.

A sentence is C-true if it is true neither by virtue of its structure nor by virtue of the meanings of the terms occurring in the sentence. A sentence is C-true if it is contingent, i.e., matter of factly true. Experience decides its truth, and it could easily be false, if experience so dictates. Of course, L-truths and A-truths are also capable of being false if certain changes are made either in the linguistic framework L or the associated meaning postulates. An example of a C-truth in a thing-language framework might be "All crows are black" or "The students are storming the administration building at noon." One of the factors that determines what is to be considered as C-true for a given framework is some apparatus of confirmation. The concept of "confirmation" is highly problematic so we will merely

assume that some notion of confirmation is associated with L. Then C-truths for L are those contingent sentences which are confirmed in accordance with the procedures for L. It is important to note that the notion of confirmation is relative to the framework L and that changing the notion of confirmation is tantamount to opting for an alternative framework. Ultimately, the notion of confirmability for any framework L hinges on what is accepted as "quickly decidably true." But again we cannot assume, because some sentence S is quickly decidably true for some framework L, that S is quickly decidable for any alternative framework L'.

Having made these distinctions, we can move on to the question we are most interested in, namely, the "reality" of the entities or processes which are designated by terms which occur in L contexts. Of course, we are most interested in the status of theoretical entities, such as electrons, but it will pay us to consider the status of everyday entities, such as tables and chairs, as well.

According to Maxwell, to say that ϕ's are real is just to say that ϕ's exist. To say that ϕ's exist is just to say that there are ϕ's. In symbolic notation, this last assertion can be represented as $(\exists x)\phi x$. ([56], p. 21) Now, to be able to say that ϕ's are real in the context of L, it is sufficient that there are highly confirmed (C-true) sentences in L which entail the sentence $(\exists x)\phi x$. ([57], p. 137) Maxwell treats only theoretical

terms, i.e., ϕ's which supposedly designate theoretical entities, but it is easy to see how his method works for ordinary entities like tables.

Let our framework L be the thing-language framework. Suppose that S = "Jones sees a red table" is a C-truth in L. S is then taken to be highly confirmed in L, where we assume that the phrase "highly confirmed in L" is well understood. In symbolic notation, if we assume that L contains predicates Rx (= "x is red"), Tx (="x is a table"), and Sxy (= "x sees y"), we have

$$S = (\exists x)(Rx \text{ and } Tx \text{ and } Sjx)$$

where "x" is an individual variable and "j" is an individual constant which denotes Jones.

Now it is clear that S entails the sentence:

$$(\exists x)\ Tx$$

which is the symbolic form of the expression, "There is (at least) one table," or simply, "There are tables." But, on Maxwell's view, this is just to say that tables exist and are real. So much for everyday objects.

Consider now the sentence, "Electrons exist." Suppose we have a theory T which is well confirmed in some framework L such that

$$T \text{ and } S_i \longrightarrow (\exists x)\ Ex$$

where S_i are some highly confirmed C-truths and "$(\exists x)$ Ex" is the symbolic representation of "There exists at least one electron" or "Electrons exist." Then, with respect to L, we may say that electrons are real. The same technique

may be used to show that, for certain theories, "lines of force" or the like also exist and, hence, are real.

Now this, undeniably, if correct, would be a very neat solution to what seems to be a very perplexing problem. The only question that remains is whether, in fact, it is an adequate solution.

As I said earlier, there can be no doubt that Carnap's division between "external" and "internal" questions is a very useful device for achieving some semblance of order in the confusions that surround ontological issues. But, it is, at best, the first word and not the last. Having ascertained that "lines of force" exist and are real with respect to our framework, Maxwell answers the obvious question "If they are real, are they physical objects?" in the negative. Of course they are not physical objects. ([57], p. 137) But, the question then is, what kind of objects are they? Theoretical objects? The whole issue of the ontological status of theoretical entities is left unsolved. For certainly, is not the problem of the ontological status of theoretical entities just that of deciding what kind of objects these theoretical entities are?

I think part of what Maxwell is objecting to is the tendency to identify "real object" with "ordinary everyday object," with the resulting tendency to maintain that if talk about entities or terms cannot be reduced in some way to talk about ordinary everyday objects, then the former must <u>not</u> be real. Thus, Maxwell is willing to entertain the

conviction that there are many "ontologically legitimate" kinds of entities. ([56], p. 27) This conviction, I feel, is basically sound. However, it may be subject to misinterpretation and, hence, requires some elucidation.

Clearly, the notion of "ontological legitimacy" is relative to a given linguistic framework L. If we consider two frameworks L and L', then it is quite probable, as we have seen, that some entities which are ontologically legitimate with respect to the one will not be ontologically legitimate with respect to the other. To take a rather extreme example, let L be a framework or sub-framework which embodies among other scientific theories, the atomic molecular theory. Now the atomic molecular theory entails that atoms are real, in Maxwell's sense. Hence, for L, atoms are ontologically legitimate. Let L' be some framework which is used for religious discourse. In L' we will assume that there are sentences of the form "What man proposes, God disposes" and the like. Now it is clear that with respect to L', some users would argue that there is some collection of highly confirmed sentences in L' which entail the sentence "God exists." The confirmation procedures for L' will in general not be the same as those for L. The net result is that L' confers ontological legitimacy on "God." If some L'-users refuse to draw this conclusion, the others could always claim that those who refuse to countenance the existence of God were merely using a different linguistic framework L''. Again we see

the potentially neat disposition of seemingly complex philosophical issues.

What happens when we compare L with L', or even combine them together? If we do the latter, we must insure that no contradictions ensue. ([57], p. 133) This I think could be easily done. The fact that the confirmation procedures for L and L' are different does not necessarily mean that they are incompatible. We now have a super framework, SL, which confers ontological legitimacy on both "atoms" and "God." Well, now what are we to say? One might say that atoms and God are both real with respect to SL, but somehow are not the same kind of entity. There is no problem here as we are to accept the existence of different kinds of entities. But surely this is to trivialize the distinction between them.

As a matter of fact, the two frameworks L and L' seem to serve radically different purposes. As such, perhaps it would be better to refrain from combining L and L' into SL. Then, we can refer their ontological difference to a difference in framework. With this move it is perfectly clear why they are ontologically distinct. They are ontologically distinct because their legitimacy is conferred by frameworks which are set up to serve different ends.

Notice, though, that this is <u>not</u> the main context in which Maxwell wants to confer legitimacy onto different kinds of entities. In Maxwell's case, we are given <u>one</u> L

within which different kinds of entities are legitimized. But, if we accept Maxwell's view, then within a framework L, it does not make sense to distinguish the "merely instrumental" from the "real." This conclusion is rejected below. Some devices, we would like to argue, are theoretical fictions. Therefore, within a given linguistic framework, we want to be able to distinguish what is "real" from what is "merely instrumental."

The net result, again, is that we must distinguish between external and internal questions. For Maxwell, as well as for Carnap, internal questions of ontological commitment are trivial. ([16], p. 209) As I said earlier, it seems to me that this solution is too easy and is, in fact, false. It is not the case, I would argue (and do so argue, below), that our commitment to a particular framework entails that we must accept the real existence of all the entities which are designated by terms within that framework. To say that all such entities exist "in a sense" is to trivialize the matter, for precisely what sense of "exist" is exactly what is at issue. I think it is possible and fruitful to draw a distinction between what is real and what is merely instrumental even within a given framework L. This point can be made very nicely, I think, by examining Maxwell's criticisms of Nagel's view that the distinction between instrumentalism and realism is a merely verbal issue.

In the Structure of Science, Ernest Nagel argues that the dispute between realists and instrumentalists is

merely "a conflict over preferred modes of speech." ([60], p. 152) In the article "The Ontological Status of Theoretical Entities," Maxwell criticizes Nagel's view. ([56], pp. 20-22) I agree with Maxwell that Nagel's view is incorrect, but I disagree with Maxwell's own assessment of the situation. A sketch of Nagel's argument and Maxwell's reply is followed by a critique of Maxwell's approach. This will result in clarifying the possibility of drawing a viable distinction between instrumental and real terms internal to a framework L.

The main reason Nagel offers for considering the controversy between realism and instrumentalism to be a verbal dispute is that "real" seems to have many different "meanings." ([60], p. 146) Thus, he offers several conflicting criteria for what is meant by "real," and he stresses "that the criteria . . . are intended to be explicative of what is supposedly <u>meant</u> in a number of contexts when something is said to be physically real" [my italics]. ([60], p. 150)

The various criteria that Nagel considers are as follows:

(1) A thing or event is physically real if "the thing or event [is] publicly perceived when suitable conditions for its observation are realized." ([60], p. 146)

(2) ". . . every nonlogical term of an [experimental or theoretical] . . . law . . . designates something that is physically real, provided that the law is well supported

by empirical evidence and is generally accepted by the scientific community as likely to be true." ([60], p. 147)

(3) "A term designating anything physically real must enter into more than one experimental law, with the proviso that the laws are logically independent of each other and that none of them is logically equivalent to a set of two or more laws." ([60], p. 147)

(4) "A term signifies something physically real, if the term occurs in a well established 'causal law'. . . in some indicated sense of causal." ([60], p. 149)

(5) "The real is that which is invariant under some stipulated set of transformations, changes, projections, or perspectives." ([60], p. 149)

This list, Nagel admits, is in no way exhaustive, but from it he concludes that there are many different meanings of "real" or "exists." The result is that the question, "Are ϕ 's real or only instrumental?" for Nagel is ambiguous and no clear cut answer is possible. ([60], p. 151) Whether one says that ϕ's are real or not depends on which criteria for "real" one adopts. Nagel concludes that it is difficult to escape the conclusion that the dispute between instrumentalism and realism is merely verbal, since either position, if spelled out in enough detail, "can assimilate into its formulations not only the facts concerning the primary subject matter explored by experimental inquiry but also all the relevant facts concerning the logic and procedures of science." ([60], p. 152)

Maxwell makes two criticisms of this argument, one of which seems acceptable, and the other only with reservations or reformulations. First, Maxwell argues that Nagel has confused meaning with evidence. ([56], p. 21) This develops out of an ambiguity in the notion of "criteria." Maxwell's point can be made most fully by distinguishing between two kinds of criteria, which we will here call evidential criteria and meaning criteria.

Evidential criteria for a proposition p are criteria that serve as evidence that what p asserts is the case. Meaning criteria for p are criteria for what p means, i.e., definitions of p. A simple example will bring out the difference clearly. Suppose we are detectives prowling around a suspicious area late at night. We hear what appears to be a shot ring out from a darkened alley. We proceed to investigate and discover the following tableau. A man we know as Smith is found lying on the ground in a pool of blood. His clothes and face are bloody and there are signs that a struggle has taken place. Standing over Smith is another man known to us as Jones. Jones also has a disheveled appearance, is covered with blood and gives all indications that he has been in a fight. In addition, he is holding a revolver that later investigation reveals (1) has been used in shooting Smith, and (2) belongs to Jones. The blood on his clothes also turns out to be the same type as Smith's. What are we to conclude? Clearly all the evidence points to the fact that Jones has shot

Smith, has, in fact, murdered him. All of the observable facts are evidential criteria for the assertion that Jones murdered Smith. However, no one would want to say that all this evidence was what we mean by the assertion that Jones murdered Smith. Hence, the evidence for ascribing a particular predicate to an individual ("murderer" to "Jones") is not necessarily part of the meaning of the phrase "Jones is a murderer."

Nagel does not make this distinction. The criteria that he suggests as alternate criteria for the meaning of "real" are, in fact, alternate evidential criteria for the existence of certain kinds of entities. ([56], p. 21) On this point I am in complete agreement with Maxwell. Nothing in Nagel's argument requires us to accept the conclusion that any of the criteria he discusses are "defining characteristics of existence." ([56], p. 21) They are, however, perfectly good evidential criteria for the existence of certain kinds of entities. Thus, Maxwell's position is that the basic idea of any kind of meaning criteria for "exists" is mistaken. I would only disagree when he seemingly concludes from the fact that there cannot be any meaning criteria for "exists," that it follows that there cannot be any meaning criteria for "is real." This point I discuss in conjunction with Maxwell's second criticism of Nagel's position.

Maxwell objects to Nagel's approach on the following grounds: ([56], p. 21)

(1) Existence is not a property.

(2) Nagel cites various possible criteria for "being real," or what is the same for both Nagel and Maxwell, "exists."

(3) These criteria (1-5 above) are all, in order for Nagel's argument to have any force, <u>meaning</u> criteria.

(4) But all of the criteria that Nagel lists are "properties of sorts."

(5) For any of Nagel's criteria, say, C, we have

ϕ's exist = (definition) ϕ's are C. But, this is to make existence into a property, which "nine hundred years of analysis" should have convinced us, it is not.

(6) Maxwell's own explication involves, for him, the truism that

ϕ's exist = (definition) there are ϕ's

and There are ϕ's = (definition) ϕ's exist.

If our language is formalized, then this last can be written

There are ϕ's = (definition) $(\exists x)\phi x$.

(7) Contemporary logic assures us that "$(\exists x)\phi x$" does not attribute any property to ϕ's that confers "existence" (and, hence, for Maxwell, "reality") onto it.

(8) Furthermore, Maxwell maintains that this is the last word on the question of the meaning of "exists" and "real," since the meaning of "There are ϕ's" is "clear enough so that no further explication is seriously needed" ([56], p. 21)

(9) But then Maxwell goes on to say that ". . . if we have a well-confirmed set of statements . . . which entail the statement "There are ϕ's" then it is well confirmed that ϕ's are real--full stop!"

There are two points that I wish to make about this analysis. In the first place, it may seem that Maxwell has rejected Nagel's criteria as property-bearing only to substitute one of his own. That is, it may look like Maxwell has said that

ϕ's exist = (definition) "We have a well confirmed set of statements . . ., etc." But the point is that Maxwell's criteria are not <u>meaning</u> criteria but <u>evidential</u> criteria. That is, satisfaction of Maxwell's criteria highly confirms the existence of ϕ's, but does not analyze the meaning of "ϕ's exist."

In the second place, Maxwell considers the statement "ϕ's are real" to have the same meaning as "ϕ's exist." However, whereas existence may not be a property, "being real," it seems, may indeed be a property. This does not necessarily do violence to ordinary usage (to which Maxwell appeals in his own analysis) since there is at least some sense of "real" where "real" is a property, although existence is not. Just to cite one example, compare the symbolic forms of

(1) "this nickel is genuine (real)"

and

(2) "this nickel is counterfeit"

If we let Nx = "x is a nickel" and Cx = "x is counterfeit" then we get

(1) $(\exists x)$(Nx and not-Cx)

and

(2) $(\exists x)$(Nx and Cx)

True, in this instance, "real" turns out to be a negative property, but this does not do violence to ordinary usage at least on some analyses. ([5], pp. 70-71) The point is that there are some senses of "real" in which to be real is a property, although existence may not be.

In our sense, within a scientific framework L, if p_1, p_2, and p_3 are purposes which must be served by a term in order that we say that ϕ designates a real entity, we have

(3) ϕ's are real = (definition) Real ϕ's exist.

and

(4) Real ϕ's exist = (definition) There are real ϕ's.

In a symbolic language, "There are real ϕ's" would be symbolized as "$(\exists x)(\phi x$ and Rx)," where Rx = x is real is a predicate which collects the properties which an entity must have to be real. In our example, Rx = p_1x and p_2x and p_3x, where p_ix = "x serves purpose p_i."

This shows conclusively, I think, that it is a mistake to identify the question of existence with the question of reality. In particular, even once we are internal to a particular linguistic framework ontological

questions are not trivial. Even within a framework (or theory) it is possible to distinguish what is real from what is merely instrumental.

CHAPTER VI

Models and Theoretical Terms: Some Realist Views

Having discussed the problem of scientific realism in general, it is time to investigate the implications of this position for the particular question of the ontological status of theoretical entities. In this chapter I propose to investigate three attempts to formulate a realistic position with respect to the status of theoretical entities. I shall argue that none of the three positions we shall look at adequately distinguishes itself from instrumentalism.

1. One recent forceful advocate of realism in physics has been Paul Feyerabend. In a series of papers, he has defended (with special reservations for current quantum theory) the realist as opposed to the instrumentalist interpretation of physical theories. (esp. [32], [33]). He offers several arguments designed to show that instrumentalism is incompatible with a progressive scientific spirit. An examination of these arguments will, I feel, show them to be less than completely convincing.

One argument is based on the conservative policy supposedly generated by instrumentalistic interpretations. What the argument boils down to is that realism is claimed to be more "instrumentalistic" in producing better theories than is instrumentalism. In "Realism and Instrumentalism," Feyerabend considers this point with respect to the Copernican

Revolution. ([33], pp. 286ff.)

On the basis of the available evidence at the time of Copernicus and Galileo, the instrumentalistic view of Copernicanism ought to have prevailed over the realist view. According to Feyerabend, it was through the persistence of the realists that we came ultimately to accept the truth, i.e., that the earth really moves around the sun and not vice versa, contrary to the appearances. According to Feyerabend, if physicists had accepted the instrumentalistic interpretation and thought of the Copernican system merely as a handy tool for calculating planetary motions, they would not have had the requisite confidence in the theory to try and develop it any further. The realist view, that the Copernican system is truly descriptive of the solar system, presumably increases the physicist's confidence in it to the point where he spends time trying to improve it. But what does this increased confidence consist in? At the time of the innovation of the Copernican hypothesis, the empirical evidence was insufficient to distinguish between it and its Ptolemaic rival. Yet the Copernican hypothesis made predictions (e.g., about parallax) that the Ptolemaic hypotheses did not. Is not our increased confidence in the reality of the Copernican universe just our increased confidence in the Copernican hypothesis due to the verification of these extra effects? If so, is not our acceptance of Copernicanism in the long run due to the fact that it turns out to be a better predicting tool than the Ptolemaic

theory, in which case, what does our acceptance of Copernicanism have to do with any commitment on our part to the alleged reality of the universe as so described?

Feyerabend writes, ". . . the persistence of the Copernicans was finally rewarded and the belief in the basic correctness of their point of view justified. The realistic position triumphed . . . ." ([33], p. 301) But, how so? Continuing, "Does it not show that the realistic position encourages research and stimulates progress, whereas instrumentalism is more conservative and therefore liable to lead to dogmatic petrification?" ([33], p. 302) The answer, of course, should be no. One could always argue, it seems, that it was not the realistic interpretation of Copernicanism which led the way but rather the fact that in the long run, the Copernican doctrine led to results which conflicted with the Ptolemaic. But, the realist might argue, the Copernican system had all but been adopted <u>before</u> the parallax confirmations were made, so how can the fact that Copernicanism proved to be a better instrument of prediction have influenced its adoption? The answer to this, surely, is that in between the time that the theory was first announced and the time that the existence of parallax effects was confirmed, Kepler and Newton made their significant contributions to our understanding of planetary phenomena. Their ability to simplify the description of the planetary motions drastically by doing away entirely with the epicycles still remaining in the Copernican

view swept away the last remaining plausibility of the Ptolemaic system. But all we are entitled to conclude from this is that the sun-centered system became the simplest method for determining the motions of the planets. The earth centered astronomy was no longer the most useful tool for this purpose, although it remained the most useful tool for other purposes, i.e., for navigation. ([52], p. 37)

It must be pointed out that Feyerabend himself does not accept the above argument as the strongest one for realism. Feyerabend suggests that there are "grave difficulties" in the way of accepting the realist position. Some of these difficulties become apparent in the controversy over the interpretation of contemporary quantum theory. ([33], p. 290ff.) Nevertheless, Feyerabend concludes that there are strong methodological reasons for always preferring the realist interpretation. ([33], p. 308)

These methodological considerations are presented in the following way. ([33], p. 282, n. 4) All physical theories are problematic, according to Feyerabend, in the sense that there are always observations which are <u>prima facie</u> incompatible with the proposed theory. The only thing to do with these <u>prima facie</u> falsifiers is to put them aside for the moment and try to develop the theory in such a way that it will be able to accommodate them. Feyerabend proposes a three-fold division of "empirical content" for a theory T.

Class I (C): This is the class of observation

statements which support the theory. The class of supportive evidence.

Class II (D): This is the class of observation statements which tend to disconfirm the theory. The disconfirmative evidence, or the potential (or actual) falsifiers of the theory.

Class III (P): This is the class of prima facie falsifiers.

Given this classification we can establish four points concerning the relative merits of theories.

(1) A theory T is refuted if we find that (D) is non-empty.

(2) A theory T is well supported (at time t), if, so far, (C) has many members and (D) has none. Of course, if the "theory" is so designed that (D) cannot have any members, then the "theory" T is not disconfirmable or unfalsifiable, and fails to be scientific.

(3) A theory T is metaphysical to the extent that (P) is non-empty; the more members (P) has, the more metaphysical is T.

(4) In discussing the relative merits of two theories, T and T', the degree to which they are metaphysical should be considered and the one that is less metaphysical should be accepted (assuming that it is the better confirmed of the two).

We may note that whether an observation sentence belongs to (D) or to (P) depends on the availability of

alternative theories. For Newtonian mechanics, e.g., is the perihelion shift to be taken as disconfirmative evidence or as a prima facie falsifier? With the non-availability of any equally comprehensive alternative, it seems that we must count it as a prima facie falsifier. In fact, this seems to be quite general. In the face of a lack of viable alternatives, the (D) for any theory T must remain empty. Only in the light of some new theory T', for which some of the prima facie falsifiers of T are confirmatory evidence, would we consider these same prima facie falsifiers of T to be disconfirming evidence for T. Thus, I should think that this classification scheme is only useful insofar as it is possible to compare two theories T and T'.[1]

Feyerabend's methodological point is that, in the light of the above considerations, it is the duty of the scientist to search out theories that are alternatives to those at hand, not only in the case any difficulties may arise within the theory (and from what he claims for the non-emptiness of (P) such difficulties always exist), but as a matter of course. ([33], p. 306)

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[1]Feyerabend [33]. This point is made by Feyerabend in section XV. In the light of this, the distinction between genuine scientific theories and crank theories can be put more forcefully than Feyerabend does in section XII, p. 305. The crank theorist is one who ultimately refuses to shift any elements of class P into class D, or even refuses to look for evidence which would support such a shift. Now if what I claim above is correct, it is clear that such evidence cannot be merely empirical in nature, but must utilize the search for viable alternative theories. Thus, the crank theorist is one who refuses to consider theories alternative to his own.

Now Feyerabend claims that this consideration leads us to postulate realistic theories over instrumentalistic theories since the realistic theories are invariably stronger, i.e., claim more, than their instrumentalistic counterparts. ([33], p. 306) But despite the truth of this, the crucial difference between the realistic and instrumentalistic version of a theory (that the theory is truly descriptive of the world) is not a matter for empirical test. Feyerabend's program seems perfectly compatible with the instrumentalistic program. In the light of the principle of testability, that we ought to search out the most powerful theories with the greatest number of potential falsifiers, the instrumentalist view merely demands that we seek to develop sharper and more powerful tools. In other words, even if Feyerabend is right about the way we test theories and replace them by better ones, this program does not imply, as he claims, that the theories be "descriptions of reality rather than . . . mere instruments of successful prediction." ([33], p. 306)

One can only conjecture why Feyerabend thinks that this must be the case. One reason may be his identification of instrumentalism with the conservative attitude of the Copenhagen school.[2] Although Feyerabend disdains philosophical arguments which support the instrumentalistic view of microphysics ([33], p. 300), the argument that he

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[2]This appears to be Smart's interpretation also. See ([76], p. 40).

offers to support realism is itself philosophical and does not resolve the basic differences between the instrumentalist and the realist. Similarly, the physical arguments in support of an instrumentalistic view of microphysics over a realist view, can just as easily be seen as arguments in favor of one instrumental theory over another instrumental theory.

In a review of David Bohm's book on microphysics, Feyerabend suggests that, along the lines of argument of Bohm's, the instrumentalistic view of quantum mechanics according to the Copenhagen view follows from the assumption that the "manifold aspects of our experience" are describable only in classical terms. ([33], pp. 326-27) It follows that "a microscopic theory cannot be anything but a device for the prediction of a particular kind of fact, viz., of a classical state of affairs." ([35], p. 326)

Feyerabend takes this to be asserting that <u>any</u> instrumentalism is wedded to the "facts of experience" which are supposed to be unalterable. ([34], p. 327) He contrasts the instrumentalistic view of quantum theory with a realist tradition wherein "the facts of experience . . . are not regarded as unalterable building stones of knowledge." "The [realist] tradition proceeds [by assuming] that our [theoretical (?)] ideas <u>as well as</u> our experiences may be erroneous." ([35], p. 327)

This seems to suggest that the conservatism which Feyerabend associates with instrumentalism has to do with

his belief that instrumentalism is intrinsically tied up with the view that the facts of our ordinary experience are inviolable. This, it would seem, is both incredible and false. I see no reason for assuming that instrumentalism commits us to accepting any given observational base as inviolable and unchangeable. It is certainly clear that instruments in the ordinary sense do not function in this way. It is perfectly reasonable to assume that with a refining or modifying of measuring techniques that different values for, e.g., lengths, will be observed. Clearly, from the facts that our theories do change, and our measurements do change, it behooves the theorist to devise more and more effective means of measuring. This he should do whether he is an instrumentalist or a realist with respect to fundamental particles. If theories are just tools, it is perfectly reasonable within the instrumentalistic framework to work towards the sharpening and refining of these tools. The instrumentalist, as such, is in no way committed to the absolute stability of some prior methods of describing data. If he can effect a slicer that can cut thinner pieces (and, as a result, leads to an explanation of hitherto unexplained or disconnected phenomena), it is within the spirit of instrumentalism for him to do so. In effect, Feyerabend's argument that realism is preferable to instrumentalism because the former is progressive and the latter is conservative is unconvincing. Either view can be progressive. The notion of progressiveness does not touch the central

issue between the two, i.e., the ontological status of the theoretical entities referred to by the theory.

2. Another contemporary advocate of scientific realism is J. J. C. Smart. In Philosophy and Scientific Realism, Smart defends the view that our scientific theories (especially those of physics) are true and that the micro-entities and micro-processes of physical theories are "ontologically respectable." ([81], p. 47) As such, they are "on a par" with, and as real as, concrete entities of the macroscopic world such as stars, trees, microbes, and large fat molecules. ([81], p. 36) On the other hand, not all the terms of physical theories designate real entities. Some of these terms represent logical or theoretical fictions. ([81], p. 34) Whether Smart succeeds in drawing a tenable distinction between real theoretical entities and fictional ones remains to be seen.

In addition, Smart offers some general arguments against instrumentalism which are worthy of consideration. Smart points out the similarity between instrumentalism and philosophical phenomenalism. As such, some of the arguments against phenomenalism in general are also possibly arguments against instrumentalism. One of these arguments is of particular importance for instrumentalism, as Smart considers it to provide a "compelling" reason for being a realist. The argument hinges on the fact that:

> the rather incoherent stream of our sense impressions is readily understood if they are

> thought of as due to our interaction with an objectively existing physical world, but on the phenomenalist theory it is a huge accident that they hang together in the way that they do. ([81], p. 25)

With respect to instrumentalism, this may be called, with Smart, the cosmic coincidence argument. ([81], p. 39) According to this argument, if we consider theories to be merely instruments for prediction and theoretical terms to be merely useful fictions, then we have no reason to expect our theories to be successful. But as a matter of fact they are successful. The realist argues that the instrumentalist must find this success remarkably fortuitous. Suppose we consider the brown desk upon which I am now writing. From a phenomenalist point of view, the material desk does not <u>really</u> exist. What exists are the phenomenal appearances which cluster together and which I label "desk." Now, as a matter of fact, this phenomenal cluster exhibits a remarkable coherence and stability. Thus, if I leave the room for a moment and then return, I find the same cluster of phenomenal appearances. Similarly, if I perform experiments on the desk, by cutting it up or scratching it, the results are regularly and dependably predictable. From the phenomenalist point of view, the fact that phenomena regularly cluster into "objects" is mysterious.

An analogous situation exists with respect to theoretical entities. The characteristics of the electron, say, are regularly reproducible. It is this feature, in

part, which leads us to accept the real existence of electrons. There is as much reason to doubt the reality of electrons as there is to doubt the reality of desks. From an instrumental point of view, this is all a "cosmic coincidence." There just does not seem to be any acceptable reason why the world should behave in such a way that what is real can be connected through theories which are only instrumental. On the other hand, if we adopt a realist attitude towards theories, none of this is surprising, since our theories are taken to be descriptive of an underlying reality. ([81], p. 39)

Smart considers this to be a very telling argument for realism. In abolishing the need for assuming a cosmic coincidence, it provides a "great and compelling reason" for believing in the reality of theoretical entities. ([81], p. 47) And yet, careful reflection suggests it is fundamentally mistaken.

For one thing, it is misleading to assert, as Smart does, that the cosmic coincidence argument offers theoretical grounds for rejecting phenomenalism or instrumentalism. ([81], p. 39) I would rather say that this is, at best, a philosophical or even a metaphysical ground for realism. I think that this is preferable so as not to confuse this kind of ground with theoretical grounds in the sense that present day quantum theory offers theoretical grounds for the principle of uncertainty or that Dirac's theory offers theoretical grounds for assuming the existence of the

positron, because these theoretical grounds relate to observable and testable consequences and as such cut across the realist-instrumentalist controversy.

Even as a philosophical ground for realism, the cosmic coincidence argument provides a poor foundation. It seems to me that in light of our previous discussion of alternative linguistic frameworks, the cosmic coincidence argument either involves a confusion between internal and external questions or, viewed as an internal issue, reduces to a tautology.

Briefly, the realist demands of his theories that they provide an intelligible description of the world. Smart's implicit assumption seems to be that science and its conceptual apparatus offer us an insight into the "way things really are"; and it is on this basis that the realist can make the success of our theoretical speculations intelligible. From our prior discussion of alternative linguistic frameworks we see that this will not do. There are many possible frameworks we may use to "describe" or conceptualize the world. To take one of these frameworks as defining "the way things really are" begs the question at issue.

Frameworks are designed to serve certain purposes and ends. There can be no privileged frame in terms of which we can argue that the others are to be judged. No "theoretical" considerations can lead us to adopt any one framework over another, since "theoretical" considerations

are internal to particular frameworks and as such implicitly assume their ends and purposes.

As such, I do not see how realism can offer any hope with respect to the question "Why are our theoretical speculations successful?" This suggests that perhaps it is not a proper question. As I just said, I think there are basically two ways to view the argument, either of which reduces its force to nought.

First, Smart may be considering the success of science from an external point of view. That is, from a point of view external to the framework of science. In this case, the argument might be posed as follows. The framework of science is eminently successful. How are we to account for this success? Any answer to this question requires an extra-scientific point of view. To appeal to the fact that scientific theories provide us with truths about an underlying reality in an attempt to explain this success is patently invalid. For whatever it means for something to be "real" in a scientific sense, this same sense cannot be legitimately applied to the framework as a whole. The question of scientific reality is a question internal to the scientific framework. The invalidity of the appeal to an "underlying reality" rests on the fact that "real" is not being used in the same way in both cases. In fact, the phrase "underlying reality" is an incomplete phrase. It is incomplete in the sense that it does not specify any linguistic framework with respect to which it

is being used. If the phrase is completed by specifying some framework, e.g., by saying "underlying reality as revealed by the framework of science" then the argument becomes patently circular. One is saying that our scientific theorizing is successful because our theories reveal the underlying reality as revealed by the framework of science. The attempt to interpret the "cosmic coincidence" argument as providing an external ground for making the success of science plausible must be abandoned. Either it reduces to a circular argument or makes an appeal to some metaphysical sense of "real" which in no sense can be identified with the sense in which science provides evidence for the reality of its findings.

The attempt to interpret the "cosmic coincidence" argument as providing an internal ground for making the success of science plausible fares no better. For a scientific realist, i.e., one who holds that theoretical entities (at least some of them) are real entities, must hold that they are real in a scientific sense. This sense must be specified internal to the framework of science. But, if this sense of "real" is adhered to throughout, then the cosmic coincidence argument holds equally well against both the realist and the instrumentalist. For internal to the framework of science, the realist can no more account for the success of theories than can the instrumentalist.

From an internal standpoint, we can only ask why

one theory T_1 is successful where another theory T_2 fails. And, here, the appropriate answer is that T_1 is more successful than T_2 because that is the way the world is, as determined by our scientific framework. But with this last necessary proviso, we have established nothing.

The compelling nature of the cosmic coincidence argument, despite its inadequacies, results, I think, from the implicit assumption that science is the privileged frame of access to "truth" about the world. Once this assumption is challenged and abandoned, the persuasiveness of the argument from coincidence disappears. Any hesitancy to accept the resulting state of affairs can be traced to an urge to re-establish science as the unique bridge to some transcendent (and hence, metaphysical and pseudo-) reality.

Despite these reservations concerning the general arguments against phenomenalism and instrumentalism, I think that Smart is correct in distinguishing between real and fictional theoretical entities, although, in the final analysis, I do not think that he has provided a proper foundation for the distinction. Nevertheless what Smart has to say about the distinction between real and fictional theoretical entities is both interesting and illuminating and deserves our careful attention.

In order to make the distinction between real and fictional theoretical entities, Smart begins from a common-sense standpoint to consider various senses of the word

"real." Two ordinary senses, in which "real" is contrasted with "illusory" on the one hand and "fake" on the other are rejected. The point of bringing up these contrasts is to indicate the sense in which we assert that something is real by denying that it is illusory or fake. So we may establish the sense in which we want to say that theoretical entities are real by determining what we are denying when we say that they are not real. Or, conversely, when the instrumentalist denies that theoretical entities are real, what is he claiming about them? It is clear that he does not mean that they are illusions. To consider just one sense of "illusory," we may say that the thirsty man in the desert is suffering from an illusion when he thinks he sees an oasis that is not there. The scientist who talks about electrons, e.g., is not suffering from an illusion in this sense, nor does the instrumentalist wish to suggest that an electron is a fiction in the sense that it is an illusion. Similarly, the instrumentalist does not mean to say of electrons that they are "fakes" or "counterfeits" as one might say of a spurious dollar that it is a fake. ([81], pp. 33ff.) Smart suggests the line between real and fictional theoretical entities be drawn elsewhere.

The instrumentalist, in asserting that the existence of submicroscopic phenomena is to be understood in terms of macroscopic phenomena (although denying the translatability thesis: that statements about microscopic phenomena are translatable into statements about macroscopic phenomena)

is asserting that the concepts associated with submicroscopic phenomena are defective in some way. ([81], pp. 35ff.) Now Smart wants to say that while electrons are somehow real concrete entities, lines of force are just logical fictions. ([81], p. 34) But then, what is the sense that "lines of force" is a fictional concept? Following the above, we must think that the concept "lines of force" is defective in the sense that the existence of lines of force can only be understood in terms of macroscopic concepts.

The only rationale Smart gives for thinking lines of force to be fictional is that, according to theory, there are 2π lines of force emerging from every unit charge. But there cannot (logically) be 2π of anything, hence, "lines of force" is a defective concept. This is a logical defect (although not like "square-circle" is logically defective). ([81], p. 34) The criterion of defectiveness, however, pertains to the need for the concept to be understood in terms of macroscopic concepts. If we are to assert that electrons are real, we are denying that the concept "electron" is defective. Are we asserting, then, that the existence of electrons can be understood on its own terms, or purely in terms of other submicroscopic concepts? What does this mean, however, over and above the fact that "electrons" occurs in a well validated nomological network with other submicroscopic concepts? Yet, "lines of force" occurs in the same well validated nomological network as "electrons" and hence, at least in this respect, is on a par with

"electrons."

Smart talks as if the real submicroscopic concepts refer to "concrete entities" and the defective ones do not. How about "equipotential surfaces"? ([81], p. 30) Are they "real" or "fictional" in Smart's sense? It seems clear that "equipotential surface" does not designate a concrete entity, yet it is not logically defective in the sense that Smart claims "lines of force" is. That is, there does not seem to be any logical reason for rejecting the existence of "equipotential surfaces." Smart fails in his attempt to characterize the difference between real and fictional theoretical entities on the basis of some sense of defectiveness in the concepts involved.

A second characterization is more promising. ([81], p. 36) The distinction is now drawn along lines of ontological continuity. Smart draws up a table of entities ranging from the very large (stars) to the very small (microbes) and then asserts that theoretical entities which "fit" on this list are "real" in the sense that they share an ontological continuity with macroscopically real entities. Clearly, "lines of force" and "equipotential surface" do not fit on this list, and in this sense they are fictions. They are perhaps ontologically continuous with macroscopic fictions such as "equator," "meridian line," "international date line," and, depending on your predelictions, "nations." In the context of this distinction, attributing concreteness to real theoretical entities makes

sense. Electrons are concrete entities just in the same sense as are tables, chairs and Smart; and in the same sense, equipotential surfaces and nations are not.

This second characterization is not without its own difficulties. There is a difference between electrons (and protons) and microbes, Smart admits. ([81], p. 38) There are theoretical reasons for asserting that the former theoretical entities are unobservable. It is, as Smart says, ". . . built into the meaning of the words 'electrons,' etc., that fundamental particles cannot be seen." ([81], p. 38) Smart denies, however, that this is a good reason for putting them in different ontological categories from macroscopically observable entities.

Of course, one could also argue that the mere fact that the reality of the fundamental particles can be asserted in contradistinction to the fictionality of lines of force or equipotential surfaces is no reason to include them in the same ontological category as microbes and other macroscopic concrete entities. What other reason could there be for including electrons and the like (there's the rub!) in the same category as microbes? Clearly not their observability, which all the other macroscopic members share. In other words, what criteria for concreteness are we going to invoke for theoretical entities? This problem is perhaps <u>the</u> most difficult facing the realist and I do not think that Smart anywhere offers a solution to it. Before considering this crucial issue, there is one further

difficulty with Smart's view that is worth considering.

Although it is true that there are theoretical reasons for asserting the unobservability of electrons and photons, these reasons are not of the same kind for both cases. In the case of electrons, they are just too small to be seen. In the case of photons, however, it is difficult to understand what "seeing a photon" means, since, as Smart points out, if we saw photons we wouldn't see anything else. ([81], p. 38) The implications of this difference are not clear for Smart's position. What is the status of the photon? Is it to be considered a concrete entity or not? Smart's view to the contrary, I think it is a mistake to say that because the theory opts against it, therefore it is meaningless to speak about observing an electron. I would prefer to say that it is a physical impossibility, given the theory, but that given a different theory, the impossibility might disappear. In some sense, it seems to me that the realist is committed to the existence of entities independently of any theories we have as to how they behave. Smart, in holding that statements about electrons which go counter to the accepted theory are meaningless, is swimming dangerously near to the instrumental reef. The concreteness of theoretical entities, if it means anything, must entail at least the independence of these entities from any particular theories about them.

The case of the photons in this view is somewhat

puzzling. There is a sense in which it is more than just physically or theoretically impossible to see photons. It is more of a conceptual impossibility in a wider sense, such as directly seeing one's own eye is impossible. We can see reflections of our own eyes and could presumably be operated on to enable our left eye to see our right one, or to twist our left eye in such a way that it could see itself from behind; but for the left eye to see directly into itself seems not only physically impossible but in some way conceptually impossible also. This is not perhaps the happiest of examples, but it is clear, I think, that the impossibility of seeing a photon and the impossibility of seeing an electron are impossibilities of different kinds.

On the other hand, the photon theory of light is, after all, only a theory. It is entirely possible that some day in the future we will come to drastically revise our theories about how we see. In that case, the photon may be dropped out of science altogether, just as the concept of the caloric fluid was abandoned. Or we may retain the notion of the photon and just relegate the machinery of seeing to some other apparatus. In the latter case, then, the impossibility of seeing a photon, if indeed it turned out to be theoretically impossible to see one, would revert to the same kind of impossibility that prevents us from seeing an electron now. However, the distinction between the two types of impossibility would still remain, I feel,

since the new apparatus of seeing, whatever it may turn out to be, would be unobservable in the strong sense that the photon, on a photonic theory of light, is now unobservable.

Let us return to the question of the criteria for concreteness. As I mentioned before, this is an extremely perplexing problem for anyone who wishes to make a distinction between what is real and what is fictional. Nowhere does Smart offer any criteria whereby we can arrange those objects which are to fall into his concrete ontological series from those which are not. To say that ϕ is a theoretical fiction is to say that the concept "ϕ" is somehow defective, is not to say enough. What kind of defectiveness is to be attributed to "ϕ"? Smart's view seems to be the following. It is best to quote Smart at length here, since the import of what he says is not transparently obvious.

> When someone says that "electrons are real" he is denying that "electrons are not real." By the latter it is not meant that electrons are defective, imitation, or counterfeit: if electrons are not real, then there are not any to be defective, imitation, or counterfeit. But nor is he denying the reality of electrons in the way which a man who wishes simply to deny the usefulness of the electron theory might say that electrons are not real. The man I am thinking of is not denying that electrons in a sense exist, but he is saying that their existence is to be understood solely in terms of macroscopic concepts. This would be to assert a sort of defectiveness in the concept of the electron (or perhaps of the word "electron"). It would be to say that electrons are theoretical fictions like lines of force, not even <u>non</u>-existent objects like unicorns. On his view, electrons neither do nor do not exist in the sense in which mountains or unicorns do or would exist. ([81], p. 35)

The general conclusions to be drawn from this are that

(1) If "ϕ" designates a theoretical entity, then when someone says "ϕ's are real," he is denying that "ϕ's are not real." This, in itself, is not too helpful. However, it is clear from the discussion that "ϕ's are not real" means that "ϕ's are theoretical fictions."

(2) "The man . . . [who says, "electrons are not real"] . . . is not denying that electrons in a sense exist, ..." From this remark it seems to follow that the assertion "electrons are not real" is <u>not</u> equivalent to the assertion "electrons do not exist (in some sense)." Since "ϕ's are not real" is taken to be equivalent to "ϕ's are theoretical fictions," it follows that "ϕ's are theoretical fictions" is <u>not</u> equivalent to "ϕ's do not exist."

(3) A concept "ϕ" which designates a theoretical fiction is defective in the sense that the existence of ϕ's "is to be understood solely in terms of macroscopic concepts." Thus, Smart understands these theoretical fictions in the same way that an instrumentalist might view the concept of the atom, that is, simply as a convenient concept for summarizing a set of macroscopic data about meter readings and the like. Unfortunately, Smart never gets around to explaining why "lines of force" is a theoretical fiction in this sense (of being a convenient "picture" for summarizing data about "the distribution and directions of the forces that would be exerted on unit

electrical charge at all places within a certain region of space") and electrons are not. He states that lines of force are fictional, recall, because they are characterized by the number 2π, and there just are not 2π of anything. ([81], p. 38) However, he does not argue this point.

One interesting point which does emerge from this discussion is that Smart seems to make a distinction between the question of "reality" and the question of "existence," although he does not develop this line of thought to any extent. He does suggest that to say of a concept that it is fictional is to say something about the way it is "used," but these remarks are very brief. ([81], p. 35) However, it is a small step from this view to the view that the difference between real and instrumental terms and theories is reflected in the different functions or purposes that they serve. Unfortunately Smart does not develop this theme and after all we are left with no clear idea on how to distinguish between real theoretical entities and processes and fictional theoretical entities and processes.

Despite these reservations, I think that Smart's distinction between real and fictional theoretical entities is sound in principle. Whereas the instrumentalist seems committed to the view that all theoretical entities are fictions, the realist, for Smart, need not be a realist about all theories and all theoretical entities, but only about some of them.

3. In his article, "Existential Hypotheses," Herbert Feigl makes a distinction between "realists" and "phenomenalists" ([24]). As he states it, the issue between the two hinges on the question of the "independent existence" of objects which are referred to in scientific theories. The phenomenalists, of which there are a wide variety, are clustered under the characteristic of adhering to what Feigl calls the "translatability thesis." This is the thesis that all meaningful statements can be translated into a base (observation) language. Whether such a language is taken to be a sense-data language or a physicalist thing language is of no particular import. A realist is then defined by Feigl to be someone who opposes the translatability thesis, one who holds, in other words, that not all the meaningful statements of science can be translated into a base language. Just as there are several varieties of phenomenalism, so there are several varieties of realism. The realism that Feigl finally advocates is termed by him "Semantic Realism." ([24], p. 50)

According to Feigl, the theoretical terms (which are the problematic ones) have a "surplus meaning" above and beyond the meaning they derive from being partially translatable into observable statements, which are the experimental consequences of the laws in which they occur. This surplus meaning Feigl identifies with the "factual reference" of these terms. "Factual reference" is, however, ambiguous and needs to be further explicated. Part of the notion of

factual reference is tied up with the notion of semantic reference in the sense of Tarski and Carnap. ([24], p. 50) On this view, a term t in a language L has semantic reference if there exists a metalanguage ML and a translation of t into ML. Thus, the term "electron" in our scientific language L has semantic reference insofar as there is a translation of "electron" into a suitable metalanguage, say English. This condition is too weak for the notion of factual reference, as is noted by Feigl as well as some of his critics. ([86], pp. 169-70; [24], p. 50) For from a purely semantical point of view, any term t (e.g., "Absolute") has a semantical reference if there is a translation of t into an appropriate metalanguage. Whereas we wish to confer respectability on the theoretical terms in science, we want to withhold our imprimatur from such unrespectable terms as "Absolute." In order to do so we need to specify a further condition for "factual reference." This further condition is developed by Feigl from what Wilfred Sellars calls "pure pragmatics." ([24], p. 50) In addition to the requirement of semantical reference, we require that every legitimate term t occur in nomological relationships which link the various theoretical terms together and connect them with the (observational) evidential base. ([24], p. 50) In other words, we require that a theoretical term occur in some well established framework of laws which have observational consequences, in such a way that the term under consideration is necessary for the derivation of some observational

consequences which would not be derivable if the term were missing from our theoretical vocabulary.

The main objection to Feigl's analysis, as I see it, is that the realistic view he has outlined is indistinguishable from certain varieties of instrumentalism.[3] The problem is that Feigl distinguishes realism from phenomenalism (which we have already identified as a form of instrumentalism) solely on the basis of accepting or rejecting the translatability thesis. Now it should be perfectly clear from what has been said before that it is completely within the spirit of instrumentalism to deny the translatability thesis and also deny that theoretical entities have any reality or any factual reference. An instrumentalist that held that talk about electrons, e.g., was not completely translatable into an observational base, could still maintain that the concept of "electron" is only a useful fiction. Thus, Feigl's formulation of semantic realism is still too weak. The second condition, of nomological commitment (if you like), while it excludes undesirable concepts, is still compatible with an undesirable (from the realist point of view) interpretation along instrumentalistic lines.

The question remains whether there is a suitable middle ground between pure semantic realism on the one hand and Feigl's semantic realism-instrumentalism on the other. Realist positions of any shade are immediately suspect as

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[3]A similar point is made by Hempel in [80], p. 171.

metaphysical and Feigl is quick to assure his readers that there is nothing metaphysical about his view. He considers this a blessing, whereas it is in fact a damnation; for in his haste to separate himself from anything metaphysical, he seems to commit himself to a form of instrumentalism. Immediately upon opting for semantic realism, he feels called upon to ask "Does semantic realism involve a metaphysical transcendence?" ([24], p. 50) In the course of defending semantic realism from the taint of metaphysical "vacuities," he issues a caveat against taking "picture and model thinking in science" seriously. ([24], p. 52) However, the judicious use of models in physical theories can be used to support the right kind of realism and also make it intelligible. For those whose stomachs are not queasy we might consider this view a quasi-metaphysical realism. But first it will pay us to consider Feigl's reasons for preferring to think of his view as realistic as opposed to instrumentalistic. We may also, along the way, assess the evaluations of some of his critics.

Feigl's main argument to support semantic realism centers around the treatment of "existential hypotheses." Feigl defines existential hypotheses as "specific descriptive statements which have (at least up to the given moment) been verified only incompletely and/or indirectly." ([24], p. 42) These may be directly testable as, e.g., (1) "There is red ink in my ballpoint pen" or indirectly testable, as (2) "There is a magnetic field strength of such

and such magnitude at such and such place, at such and such a time." The justification that Feigl presents is based on his claim that only within a realistic frame does "it make sense to assign probabilities to existential hypotheses." ([24], p. 54)

The argument runs as follows. Feigl manages to reduce the various forms of phenomenalism which he considers into one position which he labels "Syntactical Positivism." ([24], p. 46) "Syntactical Positivism" is closely related to the deductive nomological frameworks of scientific theories as proposed by Braithwaite and Hempel. It has the characteristic, in Feigl's words, that it "contributes . . . plausibility to the view that the entities which figure in the laws of theoretical science are <u>nothing but</u> useful formal constructs." ([24], p. 46) With these opposing views in mind, we can take up the problem of specifying the relationship between existential hypotheses and the evidence upon which they are based. It will be helpful if we consider a particular case, say (2) above. (2) is an example of an indirectly testable existential hypothesis, since it contains a theoretical construct which we can take to be "electro-magnetic field of strength s, at x,t" (=EM(s,x,t)).

How do we come to assert (2)? From a syntactical positivist point of view, we assert that (2) obtains on the basis of a series of (observable) conditionals of the form, $TC \supset E$, where "TC" stands for certain test conditions, and "E" for the observed effects of those test conditions.

Label the set of such conditionals necessary to assert (2), "$\{TC \supset E\}$." What, now, is the relationship between (2) and $\{TC \supset E\}$? On the basis of well known phenomenalistic analyses, it can be shown that (2) is not logically equivalent to any finite set $\{TC \supset E\}$. ([24], p. 55) Feigl concludes that the relationship between (2) and $\{TC \supset E\}$ must be physical, i.e., evidential. ([24], p. 55) But again, as is well known, the evidence for a physical statement can never guarantee the truth of the statement. Hence, (2) cannot be physically equivalent to $\{TC \supset E\}$. The only alternative, as Feigl sees it, is that $\{TC \supset E\}$ supports (2) with a certain degree of inductive probability. It is Feigl's contention that the use of inductive probabilities can only be made intelligible in the light of a realistic framework. ([24], p. 57) His reasons for thinking this are not completely spelled out as he admits in replies to some of his critics that he could not then "furnish an accurate reconstruction of the meaning of inductive probability for existential hypotheses." ([86], p. 193) However, it is possible to reconstruct his basic line of thought. He feels that on either the "syntactical positivist" or the "semantical realist" view that "we can define inductive probability only if we have first of all clearly settled the vocabulary and the rules of the language in which both the hypotheses and their supporting evidence are formulated." ([86], p. 193) We can do this only if we "presuppose a definite set of particulars, predicates, and relations."

([86], p. 193) Now as Feigl sees it, the states of bodies and the spatio-temporal framework (field values) cannot be identified with any of the operations which are used to measure them. These states which are designated by theoretical terms must be treated on a par with the states, etc., which are designated by observational terms and which make up the evidential support for theoretical statements. ([24], pp. 48, 57; [86], p. 193) But in the framework set up by the syntactical positivist view, the theoretical terms are <u>not</u> on a par with the observational terms. The theoretical terms in such a view represent "nothing but" convenient formal fictions and are certainly not on a par with the observational terms. The syntactical positivist view is, therefore, incapable of making the notion of the inductive probability of existential hypotheses intelligible.

But this does not appear to be the strongest argument that Feigl could offer. In these times AC (After Carnap) we must be especially careful in discussions on probability to distinguish two quite different concepts, which Carnap has labelled probability_1 and probability_2. Probability_1, or inductive probability, is the probability associated with degrees of confirmation and strengths of evidence. Probability_2 is the relative frequency interpretation of probability that we associate with such statements as "The probability of 50 heads in 100 tosses of a fair coin = $\frac{1}{2}$." The probability with which we are concerned is the inductive probability concept. Now it is perfectly

correct, as Feigl asserts, that the notion of inductive probability can only be defined within a well specified language L. ([17], p. 283) However, when Carnap discusses the signs that occur in these L's, he writes, "we do not lay down an interpretation for the [predicate terms] or the [individual terms] because the choice of a particular interpretation is irrelevant for both deductive and inductive logic." ([17], p. 58) It follows, at least from this point of view, that the results of inductive logic are irrelevant to the issue of realism and instrumentalism which hinges precisely on the interpretation of these terms. It seems quite within the spirit of Carnap's position that the predicate terms need not all be "on a par."[4]

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[4]Carnap [16], p. 214. On the introduction of new terms into the language, Carnap says:

> "Many philosophers regard a question [concerning the existence or reality of the total system of the new entities] as an ontological question which must be raised and answered before the introduction of the new language forms. The latter introduction, they believe, is legitimate only if it can be justified by an ontological insight supplying an affirmative answer to the question of reality. In contrast to this view, we take the position that the introduction of the new ways of speaking does not need any theoretical justification because <u>it does not imply any assertion of reality</u>. [my italics] We may still speak . . . of 'the acceptance of the new entities' since this form of speech is customary; but one must keep in mind that the phrase does not mean for us anything more than acceptance of the new framework, i.e., of the new linguistic forms. Above all, it must not be interpreted as referring to an assumption, belief, or assertion of 'the reality of the entities'. . . . An alleged statement of the reality of the system of entities is a pseudo-statement without cognitive content . . . the question of whether or not to accept the new linguistic forms [is a practical

At times, Feigl seems to be arguing another point, namely, that we cannot introduce new predicates into the language on the basis of inductive probabilities. For instance, he says, "the decision to supplement phenomenal description at all with 'transcendent' hypotheses is not in itself based upon inductive arguments." ([86], p. 48) In other words, we could not introduce the term EM(s,x,t) into a language in which it did not already occur on the basis of inductive probabilities calculated within the language. But there is no reason why a given language could not be extended by introducing new predicates. This would, in general, change the values of the degree of confirmation function, but it is hard to see how the semantic realist is here any better off than the syntactical positivisit. ([17], p. 283) The introduction of new predicates is a common result of scientific investigation, both theoretical and experimental. Hence, the semantic realist would have to revise his language accordingly to accommodate the introduction of any new predicates in much the same way as the

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question].
In a footnote to this statement, Carnap alleges that the position here outlined is substantially the same as Feigl's in "Existential Hypotheses." But Carnap is here talking as an outright instrumentalist. The best that can be made for this position is that it is uncommitted about the ontology of theoretical entities; but insofar as Carnap represents "external" ontological questions as pseudo-questions, and "internal" ontological questions as "trivial," he must be classified in the instrumentalist camp. So therefore must Feigl, if this truly represents his position.

syntactical positivist. Whether we are concerned with probability statements supporting existential hypotheses involving old predicates or whether we are concerned with introducing new predicates into the language, the point of view of Carnap's theory of inductive probability seems neutral with respect to the possible interpretation of these predicates.

It appears that Feigl has not succeeded in establishing a difference between his realism and the instrumentalistic alternatives. This is the consensus of his critics in a symposium devoted to the topic of "Existential Hypotheses." [86]

At one point in the original article and later in the rebuttal to his critics, Feigl appeals to what must be taken as a version of the cosmic coincidence argument. ([24], pp. 41, 54; [86], p. 194) In discussing generally the adequacy conditions for any philosophical analysis, he remarks that ". . . a measure of correspondence to common-sense, together with logical consistency and some all-round completeness and circumspection, are the standards by which we may most justifiably judge the success of philosophical analyses." ([24], p. 54) With reference to the problem at hand, I take this as an indirect way of saying that it is not in accord with common sense to think that the theoretical entities are not "on a par" with observable entities, i.e., it doesn't make sense to think that theoretical entities do not exist. ([24], p. 56) This sentiment is

echoed in an earlier passage to the effect that "there is quite generally a certain correspondence between the basic (logical) frame principles of knowledge and some of the broad features of the cosmos as represented in the results of knowledge." ([24], p. 41) That is, without the assumption of this "certain correspondence" the success of our knowledge efforts in general, and our scientific investigation in particular, remains an unfathomable mystery.

This seems to be a version of the cosmic coincidence argument. The circularity latent in the argument is manifested in Feigl's invoking of a "certain correspondence" between the "basic" principles of <u>scientific</u> knowledge and the "broad features of the cosmos as represented in the results of [<u>scientific</u>] knowledge." (My italics and addition.)

The major shortcoming of Feigl's analysis is that, like Smart and Maxwell, he treats the ontological problem of theoretical terms as the problem of the existence versus the non-existence of theoretical entities rather than as the problem of <u>real</u> existence versus systematic existence. This distinction, recall, rests on the fact that merely because a well-confirmed theory entails an assertion of the form $(\exists x)\ \phi x$, this does not commit one to asserting that "ϕ's are real." In the light of this distinction, it is possible for "ϕ," in the given circumstance, to be an instrumental concept. For Feigl, however, the necessity of interpreting the results of certain experiments in terms of

hypotheses which postulate the existence of micro-entities is seen as the death blow for instrumentalism.

What Feigl has in mind is the following. Suppose we have a theory T, say, the kinetic theory of gases. Then, from an instrumentalist point of view, the terms "m" and "v," designating the mass and velocity, respectively, of individual molecules are merely parameters designed to account for and systematize empirical generalizations about macroscopic variables such as P, T, V, etc. ([24], p. 195)

As long as the theory is interpreted instrumentally, then, according to Feigl, there is no way in which the theory can be "inductively fruitful." By this he means that it would be impossible to interpret the results of experiments designed to measure these parameters. The key point is this. In setting up an experiment to measure the velocity of an individual molecule, we assert, somewhere along the way, an existential hypothesis to the effect that "There are molecules." This existential hypothesis, embedded as it is in a well confirmed theory, for Feigl, represents an admission that molecules are real. From his point of view, the very interpreting of a measurement of a micro-variable presupposes our admission of the reality of whatever it is that the micro-variable ranges over. It seems then that the price a theory must pay in order to be inductively fruitful with respect to its micro-entities is that it be interpreted realistically.

I have argued above, and at some length, that the distinction between instrumentalism and realism should not be drawn, as Feigl does here, between existence and non-existence. Since I think that the question of the reality of theoretical entities is distinct from the question of the existence of theoretical entities, it seems to me to be perfectly compatible with an instrumentalistic view of micro-entities for a theory to entail existential hypotheses about those micro-entities. I see nothing contradictory in assuming that a given term is merely an instrumental parameter within a theory and yet still be measured as the result of some experiment.

Now Feigl does hold that it is inconsistent to assume that a given term is merely an instrumental parameter within a theory and yet still be measured as the result of some experiment. He does so because he assumes, with Maxwell, that to exist is to be real--full stop. If my arguments, to the effect that this is not all there is to the question of reality, have any credibility, it follows that Feigl's analysis must be rejected. The fact that existential hypotheses can be derived from certain theories does not commit one to the reality of those entities that the theory asserts to exist.

Conclusion. This concludes our discussion of various attempts to develop a realistic interpretation of scientific theory. If this analysis is correct, all of these attempts

fail insofar as the arguments they produce against instrumentalism are either inadequate or apply equally well to realism. The key difficulty in these attempts is that they all assume that to exist is to be real, and that, therefore, one need only establish that a theory entails an existence assertion about some entity ϕ in order to establish ϕ's are real. As I have argued earlier, in Chapter V, I believe this view to be mistaken. In essence, if no distinction is made between "real" and "exists," then it seems to me that instrumentalism has the upper hand, and that no arguments for a realist interpretation of scientific theory have any validity.

Of course, I do not think that instrumentalism, in general, is true. That is, I do not think that instrumentalism provides an adequate point of view for characterizing scientific activity. This is not incompatible with believing that <u>some</u> theories (or some aspects of all theories) in science are <u>only</u> instrumental. What I maintain is that the general aim of science is to provide theories which are interpreted realistically and not instrumentally. But the key to the difference between instrumentalism and realism lies in determining the difference between real existence and instrumental existence.

As I have suggested earlier, I see this difference as exemplified in the different conception of the aims and purposes of scientific activity. The instrumentalist, in effect, demands nothing but predictive success while the

realist demands something more than mere predictive success. This something more is an intelligible interpretation of scientific theories. This demand is fulfilled by the provision of physical models.

CHAPTER VII

Models and Scientific Realism

The aim of this chapter is to tie together the threads of the previous discussion and argue from this that a model-supported realism can be maintained. The use of physical models will turn out to be a crucial factor in explicating different aspects of the controversy between realism and instrumentalism in science.

In Chapter II an analysis of the notion of a scientific explanation was offered. The main point of this analysis was to argue that scientific theories explain only insofar as they provide intelligible interpretations of the mechanisms and processes associated with the theoretical structure of theories. It was suggested that the notion of intelligibility has largely been ignored in recent discussions in the philosophy of science because of a lack of any clear explication of that troublesome term. I proposed that the concept of a physical model be employed to alleviate this lack. Two features of models were distinguished. A model has both descriptive and formal aspects. The key to intelligible theories was shown to be the employment of models with descriptive aspects. These descriptive aspects and properties were conceived of as being attributed to theoretical entities by analogy with antecedently understood, familiar characteristics of (ultimately) observable phenomena. The two aspects of physical models, to a first approximation at least, serve two distinct functions of scientific

explanation. The formal or structural characteristics of the model are associated with the predictive function of explanatory theories. The material or descriptive characteristics of the model are associated with the intelligibility function of explanatory theories.

In Chapter III, it was argued that theories are capable of being predictive although they may not be intelligible in the sense that the theoretical models may have a minimum of descriptive content. Thus, some writers, notably Mary Hesse, have argued that theories without descriptive models cannot be strongly predictive. This point of view was discussed and rejected. It was recognized, however, that the theoretical apparatus of any acceptable theory must have a minimal amount of descriptive characteristics, otherwise no observable consequences could be deduced from the theory. This minimal amount of descriptive content is conferred on the theoretical terms of a theory via correspondence rules which connect the theoretical terms with terms which designate observables and, hence, have descriptive content.

In Chapter IV, several instrumentalistic views on the status of theoretical entities were discussed. These views, in general, hold that theoretical entities, or theoretical terms, are instrumental devices for organizing and making predictions about observable phenomena. Instrumentalism entails that questions about the ontological structure of theoretical entities are misguided. It was

argued that this view is mistaken since it neglects the function of theories to provide intelligible descriptions of both observable and theoretical phenomena. This neglect is due to the fact that the instrumentalist emphasizes the theoretical function of predictivity and de-emphasizes or ignores the theoretical function of providing intelligible descriptions. In general, instrumentalism can be characterized as the view that the central aim of scientific theorizing is to produce theories which are highly predictive, either partially or totally neglecting the equally important function of making the phenomena intelligible.

In Chapter V, the controversy between realism and instrumentalism was recast in the light of an examination of the notion of alternative linguistic frameworks. The problem of delimiting the "real" was seen to be relative to the choice of a particular framework. Since frameworks are constituted in terms of certain aims and purposes, it was argued that what was counted as real would be relative to these aims and purposes. In talking about the real from a scientific point of view, it is necessary to consider the aims and purposes of science. Two aims, predictivity and intelligibility, were singled out as being central to the realist-instrumentalist controversy. The instrumentalist was characterized as one who held predictivity to be the central aim of scientific theorizing; the realist, as one who held that both predictivity and intelligibility were central aims of scientific theorizing.

It was argued that the crucial move for maintaining a distinction between realism and instrumentalism was the distinction drawn between the concept of existence and the concept of reality. Both scientific instrumentalism and scientific realism can be interpreted as maintaining that theoretical entities exist in some sense. But any analysis which does not allow one to make differentiations between senses of existence cannot maintain the distinction between realist and instrumentalist interpretations of theory. If the realist-instrumentalist controversy is not to degenerate into a merely verbal issue, as Nagel claims that it does, an analysis of different senses of "exist" must be provided. If no such analysis is forthcoming, then the realist-instrumentalist controversy must be judged a pseudo-issue, a difference over preferred "modes of speech." Such an analysis was offered in Chapter V, where it was argued that to say of an entity that it exists does not not commit one to the view that it has real existence. But to say of scientific entities that they really exist, is to be able to provide a physical model which confers intelligibility onto those theoretical entities which our well-confirmed theories assert exist.

In Chapter VI some "realist" views of theoretical terms are examined and found wanting, chiefly because they fail to make a clear-cut distinction between those theoretical elements which are real versus those theoretical elements which are only instrumental. I argue that the key

to such a distinction is the availability and deployment of physical models as outlined in Chapter II. Those theories or theoretical elements for which such models can be provided will thereby achieve a degree of intelligibility for the theory.

Many attempts to formulate realistic interpretations of scientific theories either face grave, perhaps insoluble difficulties (such as Smart's), or seem to be not significantly different from instrumental interpretations (such as Feigl's). Therefore, I suggest that the only viable method for distinguishing between realist and instrumentalist views of scientific theories is along the lines suggested in Chapter V. There the crucial difference between instrumentalism and realism was seen to be in terms of a difference in considering what are the aims and purposes of scientific activity. What it means to be a scientific realist is that the realist holds that theories provide not only predictive power but also provide an intelligible "picture" of the physical world, or the world as seen by physics (or as seen by any other scientific discipline). The availability of descriptive models for the theory constitutes evidence that what the theory describes is real.

There is no reason to consider these models as sacred, i.e., no reason to fear that once introduced they can never be modified or replaced. We are just as much, if not more, capable of being mistaken about the theoretical (and unobservable) aspects of the world as we are with respect

to our ordinary observable experiences. Just as our initial impressions in everyday experience are likely to be tentative and subject to later correction, so too our models and theories are tentative and subject to later correction. If the models lead us astray with respect to the expectations we derive from them, they must be suitably modified or abandoned. The same is true of our descriptions of the observable world around us. Incomplete descriptions are replaced by more complete descriptions.

The notion of the incompleteness of description may be further elucidated by means of an analogy with photography. When we take a picture, the lens size and focus determines to a large extent the amount of detail that will be present in the resulting photograph. A long range view of a forest will show the general outline, some individual trees but very little other detail. As we focus in on the forest, the resolution of our photograph gets better and more detail becomes evident. Theories, I suggest, are analogous (but <u>only</u> analogous) to pictures in this sense. What appears in successive photographs may not be present in the original. Certainly, also, what appears at first as a rectangular shape, gradually assumes a more definite structure, with branches, leaves, bird's nests and insects. This sequence is analogous to the sequence of investigation in science. Consider again the familiar example of the study of gas phenomena. At one point in the development of a theory of gases, the very fact that there are different

kinds of gases is established and certain gross physical relations between their pressure, temperature and volume are determined. Further investigation indicates that gases are composed of even smaller (at first invisible) particles which are called molecules. Further developments reveal the structure of these molecules and the existence of even smaller sub-molecular particles and so on.

Of course, this is only a rough analogy and is at best suggestive. In particular, whereas it makes sense to speak of a photograph as a "copy" of some original, I am not at all sure that such locutions make sense with respect to talk about scientific theories. For whereas we can always compare the photograph with the original, we are not always in a position to compare a theory with anything. The only sense in which we can compare a theory with its "original" is via its predictions and testable consequences or by comparing the predictions of one theory with the predictions of an alternative theory and comparing them with respect to how well they organize our experience.

If this is so, then one might ask, what more is there to the question of the "reality" of theoretical entities except the predictions and testable consequences of the theory? If the above analogy with photography is taken too seriously, this does present a formidable problem.

For example, Smart suggests that a serious unsolved problem for any realist view is the question ". . . how would we compare a theory which was broadly correct in its

ontology but whose predictions were rather inaccurate as against a theory which was incorrect in its ontology but which made fairly accurate predictions." ([75], p. 135) If this is a genuine problem, then it will prove to be extremely difficult for the realist to solve. However, it may be possible to sidestep it. The way in which the problem is formulated seems to presume two points, both of which need to be reconsidered.

The first point is that the problem as formulated seems to suggest that the difference between instrumentalism and realism is an empirical question. The force of the argument of this thesis is that from a point of view of practical policy, i.e., the predictive success of theories, there is no appreciable difference between instrumentalism and realism. Based solely on the empirical success of a given theory, it is always possible to accept an instrumental version of it. Of course, if a theory is not predictively successful, then it does not even make sense to suggest that the theory is "broadly correct in its ontology." That is, unless being "broadly correct in its ontology" means that the theory postulates the right kind of entities but somehow misdescribes them. Then someone might suggest that Dalton's atomism was, from the point of view of modern atomic theory, broadly correct in its ontology, but that the details were somehow wrong.

In cases like this where there is a model which could serve as the basis for an intelligible understanding

of the phenomena, then the only clue to its "correctness" is its predictive success. Unfortunately, predictive success does not guarantee correct ontology as the cases of, e.g., the Ptolemaic theory or the caloric theory of heat well attest. Consider the caloric theory of heat. Until about 1850, its predictive success was much greater than the success of the rival kinetic theory of heat. Yet the caloric theory has long been discarded, and the fluid model for heat is only used for didactic purposes. Was there any reason before 1850, say, to recognize the basic correctness of the ontology of the kinetic hypothesis as opposed to that of the caloric hypothesis despite the predictive success of the latter and the predictive failure of the former? The "correctness" of the ontology of the kinetic theory is only apparent after the theory has proved to be predictively successful and has been interpreted in terms of an intelligible model.

The dilemma for the realist is that it seems that there ought to be a way to distinguish the two properties of "correct ontology" and "predictive success." The point where Smart's statement of the problem is misleading is in suggesting that the phrase "correct ontology" makes sense. Ontologies, it has been argued, are relative to particular frameworks. The phrase "correct ontology," without reference to a particular framework, is a pseudo-concept. Implicit behind its use without reference to a particular framework, is the presumption that, in some extra-linguistic

or extra-conceptual sense, a "real world" exists. It was argued earlier, in the discussion of the "cosmic coincidence" argument, that this view is philosophically indefensible and that the "representational realism" which it implies must also be rejected.

The view here advocated, that realism relative to a framework is to be defined in terms of the aims and purposes of the framework, avoids these difficulties. What is scientifically real is determined by those theories which are well-confirmed and have intelligible models.

The use of the term "model" to designate the theoretical entities and processes that theories describe is especially designed to reflect the provisional nature of theoretical entities. The models represent the best "picture" we have at any given time of the structure of the world as presented by science. No apologies or cautions need be voiced about this re-introduction of picture thinking into physics, or science in general. The only danger is that we take the pictures too seriously as reflecting an objective fixed view of reality. Thus, the caveat issued by Feigl and others should be taken as a warning not against picture thinking as such, but against taking any picture as the final one.

A final objection must be considered. Even if it is granted that theories are realistic only to the extent that they are associated with intelligible models, still no grounds have been provided for distinguishing between

real and instrumental elements within a theory. How is it possible, with Smart, to distinguish between concepts, like "lines of force," which may be instrumental, and concepts like "electron" which designate real entities, if both concepts occur within the same well confirmed theory? It would seem that, after all, there are only two options. Either theories are treated as mere instruments for the prediction and control of observable phenomena, in which case both "electron" and "lines of force" are instrumental fictions, or theories are treated realistically as descriptive aspects of the world which are "on a par" with the macroscopic everyday world with which we are most familiar, in which case both "electron" and "lines of force" designate real entities. This dual alternative seems forced on us by the arguments to the effect that, with the inability to draw a sharp distinction between theoretical and observational terms, there is no reason to treat the theoretical realm as instrumental and the observable everyday realm as real.

However, as was indicated earlier (in Chapter VI), this presents some problems. It does not seem as if all the concepts used to describe even the macroscopic world designate entities or processes which we would want to consider "real." Consider, again, the case of the mountain and the meridian line. Whereas the mountain is clearly a real, concrete entity, meridian lines, although useful in keeping our times and our positions straight, are only

convenient fictions. There is a sense in which mountains exist and are real, and a sense in which meridian lines exist but are not real. It seems fitting that this duality carry over to the theoretical level, if, in fact, we are to assert that there is a parity between the theoretical and the everyday level. Yet the dissolution of the sharp distinction between theoretical and observational terms suggests a thoroughgoing realism, in which it must be argued that well-confirmed theories be realistically interpreted. This seems to present an insoluble dilemma. On the one hand, we want theories that are realistic, on the other, we want to leave open the possibility that they contain instrumental elements.

This dilemma is more apparent than real. For there are actually two distinct issues under consideration here, which, when clearly distinguished, can possibly both be handled by considering various aspects or functions of physical models.

The first issue may well be termed the problem of model identification. At its inception, a theory may consist of a set of hypotheses couched in terms of unknown X-factors, which when juggled together, allow for the prediction of certain observable consequences. Mendel's formulation of the gene theory is a case in point. As researchers develop and refine this instrumental concept, to make a long story short, the gene becomes identified with a certain molecule. Actually, this is a gross

oversimplification, since the mechanism of heredity is now generally attributed to several distinct molecular processes. But the pattern of associating the instrumental concepts of the original theoretical formulation with concepts which occur in some well-confirmed theory with an intelligible model, i.e., a molecular model, is evident. In fact, this process of confirmation works both ways. Although the molecular theory was generally established before the identification of the genetic mechanisms with molecular activity, the fact that it is possible to identify the genetic mechanism with molecular activity only serves to strengthen our confidence in the molecular model and other theories and models which draw upon it for support. Alternatively, the identification of the genetic mechanism with the well established molecular theory serves to strengthen our confidence in the genetic theory. The process of confirmation works both ways. It is in this sense that science progresses by replacing instrumental theories with realistic theories, that is, with theories which are well-confirmed and have an intelligible model.

The second issue may well be termed that of concept-identification. Given that T is a well-confirmed theory with an intelligible model, then which of the concepts occurring in T designate real entities or processes and which designate only instrumental entities or processes. Now, insofar as it is possible to draw this distinction at the level of ordinary experience, it should be possible to

draw a similar distinction within theoretical frameworks. Just as mountains at the macroscopic level are real, and meridian lines are not, some such distinction should apply to the theoretical level as well. The question that arises is: what theoretical concepts are analogous to the instrumental concepts at the level of everyday experience?

An examination of such concepts as "meridian line" or "international date line" or "equator" reveals that their striking characteristic is their conventionality. Each is, or derives from, the "0-point" for a scale of measurement. Once this characteristic is singled out, other examples come quickly to mind, e.g., scales for measuring temperature, pressure, distance and the like. This suggests that what corresponds to these instrumental concepts at the theoretical level are concepts associated with scales of measurement. Surely the scales employed in the measurement of energies (ergs), electric field strengths (gauss) or astronomical distances (parsecs) are conventional, and to that extent instrumental. These scales and their 0-points are part of the structural or formal characteristics of physical models. This, however, does not seem to be a very satisfactory solution. For one thing, it is not clear that scales of measurement are the kinds of things about which existence claims can be made.

The delimitation of what is instrumental within theories which are, on the whole, realistic, is a very perplexing problem. Yet it seems that if the argument of this

dissertation is correct, and it is possible to distinguish questions of existence from questions of reality, there ought to be some way in which instrumental elements can be distinguished from real elements within a theory.

Insofar as the aim of science is to provide us with an intelligible picture of the world, and hence, to replace instrumental theories by realistic ones, it might be suggested that it is theories which are at a "half-way house" stage, which contain both instrumental and realistic elements. Thus, part of a theory may be interpreted in terms of some physical model, but other parts as yet may not be so interpreted. This would suggest that theories which countenance both real and instrumental entities are those theories in the process of transition from purely instrumental theories to purely realistic theories.

This proposal is not entirely satisfactory either, for it suggests that instrumental concepts are, by nature, transitory, designed to be replaced or re-interpreted in the light of some model. There can be no doubt that many instrumental concepts are designed in this way, e.g., the instrumental concept of the gene in Mendel's theory. But, if the concept of a meridian line is taken as paradigmatic in any sense, it seems that some concepts are irreducibly instrumental. Meridian lines were not conceived in order to be replaced by, or to be interpreted in the light of, some intelligible model. To think this would be to seriously misconstrue the function of meridians. But,

then, what irreducibly instrumental concepts can we find at the micro level? Better still, if there are such irreducibly instrumental concepts, by what criteria can we determine which are irreducibly instrumental and which are not? There does not seem to be any clear answer to this question, which suggests that perhaps another direction will prove more fruitful.

If we revert back to fundamental considerations for a moment, the distinction between instrumental and realistic theories was drawn along lines of differing aims that these theories have. Perhaps the attempt to draw the distinction between what is real and what is instrumental at the level of individual concepts within theories puts too much strain on the distinction. Staying at the level of a theory as a whole, we might say that a theory is realistic to the extent to which it is well-confirmed and has an intelligible model. Then, if for some ϕ's within the theory, it is possible to derive an assertion of the form $(\exists x)\phi x$, then ϕ's exist and are real. This solution would entail the acceptance of Maxwell's basic proposal with a slight emendation to the effect that ϕ's are real if existential assertions about ϕ's can be derived from well-confirmed theories which are interpreted in terms of an intelligible model.[1] Of the three alternative directions here proposed, the third seems to be the most

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[1]See above, Chapter V, pp. 134-138.

promising, although none really seems satisfactory.

As such, the realist-instrumentalist controversy divides into three distinct issues. The first issue is resolved to the extent that it is pointed out that all conceptual frameworks and all scientific theories within the framework of science are only means instrumental to certain ends. This does not preclude us from making distinctions within these frameworks between real and instrumental theories. The second issue is resolved by arguing that the progress of science is best characterized, in part, as a move to replace instrumental theories by realistic theories which are interpreted through intelligible models. The third issue, that of concept identification, remains somewhat problematic. To the extent that the dual aspect of physical models, their descriptive and structural properties, can be exploited, it may be possible to distinguish between realistic and instrumental elements of an, overall, realistic theory. This line does not, however, appear to be too promising, although a sophisticated analysis of the descriptive-structural distinction might prove fruitful. To the extent that this distinction cannot be drawn sharply or clearly, so too a realist-instrumentalist distinction within realistic theories will prove difficult to establish.

The most promising line, as was noted above, seems to be a modification of Maxwell's basic approach. The gain in modifying Maxwell's position is that we are no longer committed to the view that the concepts contained in well-

confirmed theories necessarily designate real entities or processes. They are real only to the extent that the well-confirmed theory has an intelligible model.

There remain, of course, many open questions, but the plausibility of grounding a consistent realism on the notion of physical models has, I feel, been established.

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