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THE COGNITIVE STATUS OF SCIENTIFIC THEORIES

DISSERTATION

Presented in Partial Fulfillment of the Requirements for
the Degree Doctor of Philosophy in the Graduate
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By

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*****

The Ohio State University
1970

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CHAPTER I
DESCRIPTIVISM, INSTRUMENTALISM, AND REALISM

Section I
Introductory Remarks

The purpose of this dissertation is to examine and clarify the role of theoretical concepts in science. This examination will consist of analyzing and criticizing various current views of theory formation in science. In general the methodological role of theoretical concepts is the function such concepts serve in scientific theories, since theoretical concepts are constituents of theories. Their function is interrelated with the functions of scientific theories. There seems to be fairly general agreement among philosophers of science that the function of theories is to explain and predict the occurrence of phenomena (i.e., whatever is capable of being experienced) or to explain scientific laws. Disagreement arises as to how theories explain laws or phenomena. For instance, the traditional view holds that scientific explanation and prediction have a deductive structure. Yet this deductive view has been rigorously criticized from different points of view, some of which I shall also be considering.

Thus the methodological role of theoretical concepts is how these concepts function in enabling theories to explain and predict. The so-
called ontological role of theoretical concepts concerns whether they refer to events, entities, or properties. If they do not refer to such extra-linguistic things, then it seems that they have little, if any, ontological or denotative function. Views of course vary as to whether theoretical concepts have any such ontological function and, if they do, to what sort of things they refer. A common view, realism, holds that theoretical concepts refer to unobservable events, entities, or properties. Another way of interpreting this problem is asking for what Ernest Nagel calls the cognitive status of theories, i.e., "whether theories can be rightly regarded as asserting anything whatsoever, and if so what, and whether it is appropriate to characterize theories as either true or false statements."

Someone might ask why it is so important to wonder whether there actually are unobservable entities such as electrons with unobservable properties. As I see it, the problem basically is one of significance and of finding a criterion for scientifically acceptable significance. If theoretical concepts refer to unobservables, then the problem of significance is the problem of attempting to justify one's belief in the existence of such unobservables. It would seem that whenever we are asked to justify our claim that something exists, e.g., that there is a dog in the yard outside, or that it exists in a certain way, e.g., that the dog in the yard outside is black, we usually appeal to what we can observe. But to what do we appeal to justify our claim that something unobservable exists? If scientific theories contain such concepts,

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then this is the problem facing the scientist as well as the philosopher of science.

Most philosophers of science hold that scientific theories and laws must have empirical content or import—that the theory or law in question "must be capable, at least in principle, of test by experiment or observation." This criterion of empirical import is usually what is meant, at least partially, by the thesis of empiricism, especially in science. If one is going to hold that theories or theoretical statements (those statements containing theoretical concepts) refer to unobservable things and also that theories have empirical import, then one will have to hold that empirical import is not limited to statements which refer only to observable things.

Now the importance of the criterion of empirical import is to rule out explanations that have no empirical import and are, consequently, considered to be unscientific. As Richard Bevan Braithwaite puts it:

The function of a science ... is to establish general laws covering the behavior of the empirical events or objects with which the science in question is concerned, and thereby to enable us to connect together our knowledge of the separately known events, and to make reliable predictions of events as yet unknown.

Granted that all scientific theories are to have empirical import and that the entire language of science, call it 'E,' is empirical, just what is meant by 'empirical' is left unanswered. There are some

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views such as reductionism which hold that all non-logical terms of science are reducible to observable terms, i.e., all non-logical terms describe or refer to observable entities and their properties. If such a reduction is successful, then empirical import might be identified with what can be observed. This would definitely solve the problem of significance for theoretical concepts if one can adequately explicate what is meant by 'observable.'

Section II

Feigl's Presentation

Let us thus examine briefly some of the views that attempt to explain how theoretical terms acquire empirical import—or hence to explain what their methodological and ontological roles are in E. Herbert Feigl presents nine different views: (1) naive physical realism, (2) fictionalistic agnosticism, (3) probabilistic realism, (4) naive conventionalistic positivism or phenomenalism, (5) critical phenomenalism, operationalism, or positivism, (6) formalistic phenomenalism or syntactical positivism, (7) contextualistic phenomenalism, (8) hypothetical-deductive realism or explanatory realism, and (9) semantic realism. In examining these views let us consider as observed the deflection of a needle of an instrument.

(1) According to naive physical realism every observed event has a cause whether the cause itself is observable or not. The needle

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deflection and the principle of causality, according to this view, confirm the existence of a magnetic field as the cause of the needle's deflection. A problem immediately arising with this view is the explanation of 'confirmation.' Other problems are justifying the principle of causation and showing why one should suppose that the cause of the needle's deflection is a magnetic field.

(2) According to fictionalistic agnosticism, the concept 'magnetic field' is a useful fiction and the needle acts as if there were existing nearby a magnetic field. Since we will never know that such things exist because they are unknowable, fictionalistic agnosticism apparently grounds all knowledge claims upon the observable. Any reality which is unobservable is "forever unknowable." Surely this view requires both an explication of what is meant by a useful fiction and a justification that all empirical knowledge is grounded upon the observable. This latter requirement may seem almost obviously true: it is obvious, it is analytic even, that empirical knowledge is grounded upon the observable. Yet if 'grounded upon' means 'deduced from' or 'identical to,' it is not at all obvious.

(3a) In the third view, probabilistic realism, one also does not know with certainty that a field exists and causes the needle to deflect. "But it can be inferred with probability from the behavior of the needle and other items of evidence." Feigl claims that this view holds that "all inference that proceeds from observables to (directly) unobservables

5Ibid., p. 44.
6Ibid.
7Ibid., p. 45.
must be based on inductive probabilities.\footnote{Ibid.} It is not clear what this last statement means. Apparently it means that if we have statements about observables, then from these statements we can infer only inductively a statement about unobservables. But, we may ask, Is there any such inductive procedure? The issue in question looms so large that I here digress with an initial analysis. Consider that traditional inductive procedure called \textit{inductive generalization}. From observing several cases of crows being black and from neither observing nor knowing of any cases of crows being non-black, we infer by inductive generalization the universal statement 'All crows are black.' Let us assume that our premises or data in this inductive procedure are all statements about observables, e.g., 'this is a crow and it is black.' The question is whether or not the statement 'All crows are black' is about observables. How would one go about arguing that it is not about observables? If we consider the statements comprising the data and the conclusions, we discover that they all have in common the concepts 'black' and 'crow.' These concepts are usually considered to be observational concepts. If that consideration is correct or justified, then the inductive procedure in question is not, from this point of view, an inference from observable concepts to unobservable concepts.

One might, however, argue as Braithwaite does\footnote{Braithwaite, \textit{op. cit.}, pp. 82-85.} that a sentence like 'All crows are black' is given meaning in a different way from (say) 'This crow is black.' In asserting the latter we are attributing the property of being black to a specific crow; but in asserting the...
Braithwaite maintains, we do not attribute being black to a specific thing called 'All crows'. The sentence 'All crows are black' is given meaning by its logical relationships to other statements in a specific language (in this case ordinary language). Its logical relationship is such that when it asserts a statement in conjunction with another statement asserting that a specific thing is a crow, it logically follows that that specific thing is black. (There are, of course, other logical relationships also.) Thus a universal statement containing observable concepts and the concept 'all' is given meaning by virtue of the logical relationships of its language.

Braithwaite's discussion brings up the point as to whether 'All crows are black' is about, refers to, or describes anything. The statement 'This crow is black' is about a specific observable crow. Similarly one could say that 'All crows are black' is about each and every crow that has, is, and will exist. Now the specific crow is observed and, therefore, is observable. The question arises as to whether all crows are observed and are, therefore, observable.

I think most people would hold that at least some parts of the future are observable, viz., those parts which they in fact will observe. But the future is not observed as future, only as present. Consequently, it seems that from a temporal point of view and from the view of a finite observer who does not know how many crows there are, (and consequently, whether they all are observed) the statement 'All crows are black' could be about unobservables. Surely, 'all crows' leaves open the possibility of a crow existing unobserved. There could be

10Ibid.
unobservables, but *are* there? Does inductive generalization enable us to conclude that there *are* unobservables? I do not think so. Even with inductive generalization, the most we can legitimately infer is that all crows, whether observed or unobserved, are black. We cannot infer that there are unobserved crows. Hence to justify probabilistic realism another inductive procedure besides inductive generalization will have to be used (if there are any). We shall return to this problem later.

(3b) A second component of probabilistic realism is the claim that existential hypotheses (i.e., any statement about an unobservable usually asserting that it exists or how it exists, e.g., "there is a magnetic field nearby") "possess a surplus meaning over against their evidential basis; they are not equivalent with or reducible to (by translation) any set of actual or possible confirming statements."11 By the 'evidential basis' of an existential hypothesis is meant the premise(s) or data which support the inductive inference. This second part of probabilistic realism is an essential part of the first. In an inductive inference the conclusion always contains more than what is contained in its evidential basis or premises. Consequently the conclusion (in this case the existential hypothesis) would possess surplus meaning. An important point to note here is that the concept 'contains more' or 'surplus meaning' needs to be clarified.

(4) The fourth view Feigl calls 'naive conventionalistic positivism' or 'phenomenalism.' This view is an explicit denial of probabilistic realism. According to phenomenalism, existential hypotheses

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11Feigl, op. cit., p. 45.
have no surplus meaning over their evidential basis but are in fact defined by, equivalent to, reducible to, or translatable into the data. Such a procedure would be more nearly deductive than inductive.

Phenomenalism denies the distinction between "unobservable terms" and "observable terms." Hypotheses and their peculiar terms summarize in a simple fashion "what could in principle be formulated as regards the actually observed facts." The crucial point of this theory is whether or not an adequate definitional or translational procedure is possible.

(5) The fifth view, critical phenomenalism, operationalism, or positivism, is a modification of the preceding one. According to this view statements about theoretical entities cannot be translated completely into statements about what is actually observed. We have to consider also that magnetic fields, for instance, not only deflect the observed needle in question, but all sorts of needles which we are not presently observing. Furthermore, the magnetic field affects electric currents, paths of moving charged particles, and needles on instruments. Hence we have to consider the possible behavior of all the other needles, electric currents, etc. The assertion of possible behavior is made by using hypothetical or conditional statements, i.e., a statement that tells "us what happens, or what would happen, under specified circumstances, i.e., experimentally introducible conditions." Every time one of these conditional statements is verified, the existence of the entity in question is confirmed. Yet critical phenomenalism holds

\[12\text{Ibid.}\]
\[13\text{Ibid.}\]
that the existential hypothesis is logically equivalent to the set of all conditional observational statements and categorical observational statements which confirm it. (An observational statement is any statement whose non-logical terms all refer to observable things.) As with phenomenalism proper, so too with critical phenomenalism, there is ultimately no unobservable-observable dichotomy. Theoretical concepts are extremely condensed and economical formulations of sets of statements about actual and possible experiments. The crucial point with this view is whether or not it is possible to adequately translate existential hypotheses into statements of actual and possible experimental procedures without remainder or surplus meaning.

(6) The sixth view, formalistic phenomenalism or syntactical positivism, is a modification of critical phenomenalism. According to this view all scientific laws containing theoretical concepts, like Newton's laws of motion, are postulates of a calculus or axiomatic system. From these postulates together with observational statements and coordinating definitions, empirical laws (universal observational statements) and observational statements about specific observable individuals are deduced. In this view there are three distinct kinds of statements: the postulates, the coordinating definitions, and the observational statements. Postulates contain only logical and theoretical concepts. Observational statements contain only logical and non-theoretical or observational concepts. Coordinating definitions (usually called correspondence rules today) are statements that connect the postulates to observational statements. In order to make the connection, the coordinating definition will have to contain logical terms, theoretical terms,
and observational terms. These coordinating definitions explicate part of the meaning of the postulates in terms of observational statements. This partial explication or interpretation leaves open the possibility of surplus meaning in the postulates which is advocated by realism. In order to avoid this move, the formalistic phenomenalist holds that the postulates are merely "useful formal constructs . . . , abbreviatory schemes for the description of the complex relationships between observables." A crucial issue for this view is the status of the coordinating definitions. If theoretical statements are merely abbreviatory for observational statements, one wonders whether there ultimately is, as Feigl maintains for this view, a distinction between theoretical and observational terms.

(7) A somewhat similar view is contextualistic phenomenalism. According to this view, the observational terms of science are so interlinked in a network of relationships (both logical and causal) that:

it depends upon the context of the experimental investigations. . . which of these relationships may be regarded as genuine laws. . . and which others are then taken to be definitions.

Because of their interrelationships, hypotheses or theories cannot be tested in isolation, but in the context of the entire network.

In testing one hypothesis we invariably fall back on others which in this context are construed as definitions and provide the. . . background and presupposition without which the very notion of a test of this kind is impossible.

\[^{14}\text{Ibid.}, \ pp. \ 46-47.\]
\[^{15}\text{Ibid.}, \ p. \ 46.\]
\[^{16}\text{Ibid.}, \ p. \ 47.\]
\[^{17}\text{Ibid.}\]
According to this view, then, what is confirmed by experimentation is not merely a hypothesis or theory but the system as a whole. If the system has surplus meaning that is not completely reducible to what is testable, then we seem to have a realistic view. Consequently for contextualistic phenomenalism, all surplus meaning must be completely reducible to the evidential basis, and as I pointed out with respect to the preceding phenomenalistic views, an adequate reduction must be possible for this view to be successful.

(8) The last two views presented by Feigl are types of realism. According to hypothetico-deductive realism or explanatory realism, a scientific theory is a hypothesis which explains and predicts phenomena by means of deduction. The hypothesis, because it contains unobservable concepts, is not capable of being directly confirmed. It is, however, indirectly confirmed by true observation statements usually in the form of successfully verified predictions which have been deduced from it. Unlike probabilistic realism, hypothetico-deductive realism is not, Feigl maintains, committed to justifying its hypothesis by inductive procedures. I shall consider how accurate Feigl's assessment is when I take up realism more fully.

(9) The final view Feigl mentions (and which he holds) is semantic realism. In presenting semantic realism Feigl attempts to clarify 'surplus meaning' since he believes that this explication is primarily what is missing in hypothetico-deductive realism. 'Surplus meaning' designates both the factual reference of the theoretical terms in a theoretical statement or law and the factual reference of existential hypotheses.¹⁸

¹⁸Ibid., p. 48
By 'factual reference of a concept' Feigl means merely that to which the concept refers. The factual reference of a concept or statement must be considered along with the evidence for it. The reference should not be identified with its evidence. If they are identified, then of course we have some form of phenomenalism because the theoretical term or statement would then be about or identical to its evidential basis. In other words, semantic realism holds that theoretical concepts and statements are about or describe unobservable existing entities, events, and properties. In examining this view more carefully, I shall be concerned with the justification of the distinction between the factual reference of a term and its evidential basis. This distinction is, so Feigl holds, the main issue between a realistic view and a phenomenalistic view.

Another point should be noted also. Even if I discover that theoretical terms have a surplus meaning—that they cannot be reduced completely to observational terms, it does not follow that such surplus meaning is factual reference, i.e., that the terms refer to or name independently existing entities such as fields, atoms, and genes. The surplus meaning could be, for instance, a logical property that enables one to make predictions of certain phenomena after observing another set of phenomena. A final point should also be briefly mentioned. Since semantic realism holds that theoretical terms do have reference, the question arises as to whether this reference is empirical or non-empirical, i.e., metaphysical. Feigl maintains that semantic realism does not involve metaphysics. I shall also examine this claim.

In looking back at these nine views one can see that five are
phenomenalistic and four are realistic. Furthermore, the last view presented in each group attempts to include the sounder parts of its preceding kind, as well as attempting to meet and solve difficulties presented by its predecessors. Feigl himself realizes these relationships and notes that naive physical realism reappears in more sophisticated forms in probabilistic realism, hypothetico-deductive realism, and semantic (which he sometimes calls 'empirical realism') realism. The fictional part of fictionalistic agnosticism (i.e., that part which holds theoretical concepts to be merely useful fictions) and naive conventionalistic phenomenalism become more adequately formulated in critical phenomenalism and formalistic phenomenalism. Feigl also mentions that much of contextualistic phenomenalism is easily formulated as part of both hypothetico-deductive realism and semantic realism. The latter two, however, do not share the former's emphasis on the reducibility of theoretical concepts.

Section III

Nagel's Views

Like Feigl, Ernest Nagel also presents and analyzes some views concerning the function and ontological status of theoretical concepts and theories in science. Nagel presents three main views and subdivides them without naming the subdivisions. Nagel's three views are: (1) descriptivism, (2) instrumentalism, and (3) realism. Let us examine the main tenets of these three views and compare them with the views

19Ibid., p. 52.
discussed by Feigl.

(1) Nagel maintains that descriptivism is associated with the common notion that scientific theories never explain phenomena, but merely describe their behavior in the simplest manner. According to Nagel, the basic tenets of this view are as follows: (1) a theory is simply a "compendious but elliptical formulation of relations of dependence between observable events and properties," and (2) "theoretical terms like 'atom' are simply shorthand notation for a complex of observable events and traits, and do not signify some observationally inaccessible physical reality." Theories, as assertions, are not by themselves either true or false, but they are to be "translatable into statements about matters of observation" which are true or false.

(1a) Nagel distinguishes two different kinds of descriptivism. The first and most radical view is that all knowledge claims must be founded on what the observer immediately senses. The common name for these objects of immediate sensation is 'sense data.' These sense data are the content of both sensory and introspective experiences. They are the ground for all knowledge because it is thought that statements merely describing them are either indubitable or incorrigible. As Bertrand Russell put it: "Every proposition which we can understand must be composed wholly of constituents with which we are acquainted."

\(^{20}\)Nagel, op. cit., p. 118.
\(^{21}\)Ibid.
\(^{22}\)Ibid.
\(^{23}\)Ibid.
\(^{24}\)Ibid., p. 120.
and by 'constituents with which we are acquainted,' Russell means statements or terms only about sense data. Consequently all our knowledge about the physical world must be completely defined, translated, or reduced (i.e., without loss of any meaning) to statements about sense data. All scientific theories and theoretical concepts must, moreover, be so related to statements about sense data.

The second and less radical form of descriptivism holds that the ground for all knowledge (at least scientific knowledge) is that which is publicly observable, i.e., the world of chairs, tables, human beings moving and acting, and the like. Consequently, on this view, all scientific theories and theoretical concepts must be completely defined, translated, or reduced to statements about what is or can be publicly observed. This second view is usually called 'physicalism' (when applied in psychology, 'behaviorism'; in physics, 'operationalism') and the first view is usually called 'phenomenalism.'

Nagel's radical descriptivism (my own label) is closely related to Feigl's 'phenomenalism,' 'critical phenomenalism,' 'formalistic phenomenalism,' and 'contextualistic phenomenalism.' What all these views have in common is that theoretical concepts and/or theoretical statements, if scientifically significant (can be placed in or be part of E), must be completely defined, reduced, or translated into observable concepts and/or observational statements, respectively. The main differences between Feigl's "descriptivisms" (the four views mentioned just above) seem to be in their translation technique. For example, phenomenalism (naive conventionalistic positivism) translates theoretical

\[^{26} \text{Ibid.}, \ p. \ 46.\]
terms and statements into what is actually observed; critical phenomen­nalism, into what is actually observed and what can be observed. But Nagel's subdivision of descriptivism is based primarily on what kind of things are actually observed: sense data or physical objects and pro­cesses. So it seems that Nagel's presentation goes one step further in the explication of the concept 'observable.'

Nagel holds that the major difficulty facing any form of descriptivism is showing how such a translation of theoretical statements into observational statements either about sense data or the publicly observable is possible.27 Besides the problem of translation (with which Nagel is alone concerned), other major difficulties to descriptivism have been presented. Boruch A. Brody and Nicholas Capaldi note that besides the fact that no such translation has been successfully carried out, theoretical statements do not seem to be coordinated with only one observational statement.28 According to Brody and Capaldi, "there are an indefinite (possibly infinite) number and variety of observational terms and statements related to any one theoretical statement."29 If this statement is true, then descriptivism may be in some difficulty. Surely there would be some difficulty in actually carrying out any pro­gram requiring even an indefinite number of translations. Another point which has been presented by Frank Ramsey and Braithwaite has been cited

27Nagel, op. cit., pp. 121-125.


29Ibid., pp. 7-8.
by Mary Hesse. She notes that theories are held:

to be general and predictive and therefore capable of assimilating an indefinite number of new observations without themselves radically changing in meaning. Explicit definitions could not leave room for this and could not exhaust the potentially infinite and largely unknown range of observables to which the theory might be relevant.

If Hesse's criticism is sound, however, it will only affect those descriptivistic procedures that attempt to define explicitly theoretical terms into observational terms. This reply, of course, involves the notion that reduction and translation are not the same as explicit definition. (Brody, Capaldi, and Hesse call the view descriptivism, 'reductionism.' I shall retain Nagel's label only because some philosophers have attempted to make a distinction between 'reducing a term' and 'translating a term'.)

The third general criticism of Brody and Capaldi has already been mentioned in Hesse's quote above: descriptivism cannot give an adequate account as to how theories both explain and predict phenomena.

Nagel cites as descriptivists of some fashion or other such philosophers and scientists as Ernst Mach, Percy W. Bridgman, Bertrand Russell (at one time) and William John Macquorn Runhine. The origins of descriptivism go back at least as far as Berkeley, Hume, and Mill. Bridgman, the advocate of operationalism, is probably the most outstanding contemporary descriptivist.

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31 Ibid.
32 Brody and Capaldi, op. cit., p. 8.
33 Nagel, op. cit., pp. 120-127.
The second view that Nagel presents is instrumentalism. Like
descriptivism, instrumentalism holds that theories are neither true nor
false. They are simply "logical instruments for organizing our experi-
ence and for ordering experimental laws."\(^{34}\). (An experimental law is a
universal statement containing only logical and observational terms.)
By theories being 'logical instruments' is meant that they are "rules
or principles in accordance with which empirical materials are analyzed
or inferences drawn. . . ."\(^{35}\) Theories or theoretical statements are
not to be considered as premises from which conclusions are drawn, but
the actual rules of inference which enable one to draw a conclusion
(an observational statement) from a set of premises, all of which are
observational statements and, sometimes, definitions.

Instrumentalism is thus primarily concerned with the manner in
which scientific theories function. Descriptivism, on the other hand,
seems to be more concerned with the meaning of theories or theoretical
statements and concepts. From the recent literature on the cognitive
status of theoretical entities in science, one finds that the main dis-
cussion is centered on the debate between realism and instrumentalism.
Descriptivism or operationalism (as it is usually called) does not seem
to have many advocates today. This decline is probably due to what
Wilfrid Sellars calls the "stampede" of philosophers from phenome-
nalism.\(^{36}\)

\(^{34}\) Ibid., p. 118.

\(^{35}\) Ibid.

\(^{36}\) Wilfrid Sellars, "Phenomenalism," Science, Perception and
Instrumentalism holds, then, that theories organize the observed data and permit inferences to be drawn from them as to future experimentation (predictions). Theories cannot be reduced or translated into a set of observation statements, i.e., theories are not summary descriptions of phenomena. Furthermore, if theories are to organize observations, they apparently must be more than summaries or even generalizations of them.37

Because theoretical concepts under instrumentalism are considered as intellectual devices in organizing experimental data, one may well wonder if they have any factual reference. Nagel believes that 'factual reference' from instrumentalism's point of view is simply the subject matter (the experimental data) for which the theories have been constructed to organize and predict. One might wonder if there is any possibility of a theory having surplus meaning, since Feigl held that the surplus meaning of a theory or theoretical construct was its factual reference. Nagel replies that a theory, for the instrumentalist, could have surplus meaning:

either in the sense that it is interpreted in terms of some familiar model; or... as in the case of other instruments, its further uses... may be more inclusive than those actually assigned to it at any given time.38

These two senses of 'surplus meaning' need to be clarified. By a 'familiar model' is meant an explanatory picture of the theory in terms which are readily known or understood. The second point requires some elaboration. Instrumentalism holds that theories are tools that enable

37Nagel, op. cit., p. 127.

38Ibid., p. 131.
the scientist to organize and predict phenomena. A hammer is also a tool. It can be used to build tables and buildings. But no one can specify exactly all its possible uses since "the products of its use may increase both in number and in kind." A hammer, moreover, is not in any familiar sense 'equivalent' to what it can produce; and a hammer does not seem to represent in any clear fashion anything that it can produce (unless it is another hammer or model of one). Theories, for instrumentalism, are analogous to tools in these respects: they are not equivalent to nor represented by what they deal with. The important question is whether either notion of surplus meaning, upon careful examination, involves the factual reference of theoretical entities and properties.

Another way of looking at theoretical concepts from the instrumentalist's point of view is by considering that subclass of the ideal concepts of science--concepts such as 'straight line,' 'perfect elasticity,' 'perfect levers,' 'isolated particles,' and the like. Nagel believes that these limiting and ideal concepts do not refer to any physical objects or their properties, yet they are suggested by physical objects. Yet one might want to argue that even though such concepts do not refer exactly, they refer approximately to physical objects. And this suggests the possibility of there being degrees of factual reference. Nagel notes, however, one curious limiting concept: 'instantaneous velocity.' Since

instantaneous velocity is defined as the limit of the ratios of the distance and time as the time interval diminishes toward zero . . . , it is difficult to see how the numerical value of this limit could possibly be the measure of any actual velocity.\(^{40}\)

\(^{39}\)Ibid., p. 130.
\(^{40}\)Ibid., p. 131.
What Nagel says here about 'instantaneous velocity' seems to hold for limiting concepts in general: it is not possible for them to refer even approximately to anything physical. A. Cornelius Benjamin, in writing about such ideal and limiting concepts, states that they are very useful explanatory concepts even though they cannot exemplify anything in the physical world.\(^4\) If both Nagel and Benjamin are correct on this point, then the problem arises as to how such concepts which do not and probably could not refer to anything physical enable us to explain, understand, and predict things which are physical. This problem must be solved by the instrumentalist. But the very use of such concepts in science tends to strengthen the instrumentalist's position: such concepts neither have nor can have factual reference.

We noted earlier that the instrumentalist construes theories or theoretical statements as rules of inference by means of which statements about one set of phenomena are inferred from other statements about phenomena. This basic thesis raises the problem as to what kind of rules of inference theories could be. Take the following argument:

\begin{itemize}
  \item All men are mortal.
  \item Socrates is a man.
  \item Therefore, Socrates is mortal.
\end{itemize}

It makes use of the rule of inference which Nagel calls the principle of the syllogism, i.e., "a statement of the form 'x is P' is derivable from the two statements of the form 'all S is P' and 'x is S.'"\(^5\) The conclusion of the above argument is drawn in accordance with the principle


\(^{5}\) Nagel, *op. cit.*, p. 138.
of the syllogism by means of the premises. The rule of inference (also called 'leading principle' or 'inference ticket') is not a premise of the argument. Since, furthermore, the rule of inference does not refer to the subject-matter of the premises and conclusion, but only to their form, it is a formal rule of inference. On the other hand, the statement 'Socrates is mortal' seems to be inferable also from 'Socrates is a man' as a premise in accordance with the rule of inference "A statement of the form 'x is mortal' is inferable from a statement of the form 'x is a man.'" This rule of inference is a material rule rather than a formal one because it refers to specific subject-matter terms that occur in the premise and the conclusion and thus sanctions only those arguments having those terms in that relationship.

Nagel believes that it is simply a matter of convenience whether we construct an argument with a formal rule of inference or with a material rule of inference. He also claims that in one context a statement may function as a premise and in another as a rule of inference. But is Nagel, in making this latter claim, identifying the statement 'All men are mortal' with the rule of inference "A statement of the form 'x is mortal' is inferable from a statement of the form 'x is a man'"? Nagel claims that there is no good logical reason why theories cannot serve as premises in scientific explanations and predictions. But premises are statements and statements have truth value. Do rules of inference have truth value? Undoubtedly if some statements are identical to rules of inference and all statements have truth value. Nagel does not explicitly say that the above statement and rule of inference are identical, but he says that "a given statement may function
as a premise in one context but may in effect be used as a leading principle in another context. What Nagel seems to be saying here is that a statement may, at times, be used as a rule of inference. Does this mean that it is a rule of inference? The question seems to be whether linguistic forms or structures other than rules can function as rules.

In attempting to answer this question let us reconsider the hammer analogy. The hammer's main function is to drive nails into wood. But it is also possible to use another tool, say a screwdriver, to drive nails into wood. So it might be concluded that in certain contexts, a screwdriver can be used as a hammer. By analogy, could not, in certain contexts, a statement be used as a rule of inference. Is this a good analogical argument (as analogical arguments go)? There seems to be a problem in conceiving of the context in which a statement functions as a rule of inference without undergoing some sort of transformation (unlike the screwdriver). For instance, 'All men are mortal' seems to be merely about 'all men.' However, the rule of inference, which has been associated with that universal assertion, seems to tell us that we are permitted to infer certain statements from other statements. If they are not about the same thing, as it seems to be the case, then how is it possible for the former ever to function as the latter? I simply do not see how this is possible. If, however, we construe (probably contrary to common usage) all universal assertions as rules of inference, then of course there would be no problem, but universal assertions could not be true or false.

\[^{43}\text{ibid.}, p. 138.\]
This latter point is reminiscent of some earlier comments I made about Braithwaite on how universal assertions acquire meaning. Yet Braithwaite is reluctant to hold material rules of inference because they are all logically contingent. What he has in mind is the popular view that statements like "All men are mortal" can be false; they are not necessary truths. But they can be false only if they assert some fact or other, i.e., are statements rather than rules. Braithwaite holds that universal statements acquire meaning by the way they are used in deductive inference. This latter point seems to indicate that formal rules of inference partially determine the meaning of universal empirical statements. The remaining content is determined by their non-logical terms. Braithwaite thus seems to be denying Nagel's claim.

The central point is that linguistic structures (rather than statements) which have been used as universal empirical statements can also be used as material rules of inference. Linguistic structures in themselves have no truth value. They can be used in such a way that they have truth value; they can be used in such a way that they are rules. Hence the linguistic structure 'All men are mortal' can be construed so that 'everything belonging to the class man also belongs to the class of mortal beings' is true, or so construed that one is entitled to infer 'x is mortal' from 'x is a man.' The problem of whether theories can be rules of inference (and, therefore, material ones) will be discussed in more detail when instrumentalism is examined. One of the problems is justifying the use of material rules of inference: it seems that one could set up for any trivial reason any kind of material rule.

\[44\] Braithwaite, op. cit., pp. 86-87.
As noted previously, Nagel believes that it is merely a matter of convenience whether we use a theory as a premise in a scientific explanation or prediction, or as a rule of inference. There is no strong logical reason for refusing either use. But Nagel seems to imply and Patrick Suppes explicitly states that instrumentalism is true only if theories are rules of inference. In appealing to the scientist's attitude toward theories, Nagel finds that most scientists believe that theories are "statements about the constitution and structure of a given subject matter." Moreover, when scientists set about to test a theory, they seem to believe that what they are testing has some factual reference.

Although just how instrumentalism answers this criticism will be examined later, one possibility will briefly be mentioned, namely, linguistic elimination of theoretical concepts in theories. If there were some linguistic technique by which all the theoretical terms of a theory could be replaced by non-theoretical terms (i.e., logical and observational terms) without relinquishing the theory's empirical context, then one might show that theoretical concepts are not logically necessary for scientific explanation and prediction. Such a technique is clearly related to the descriptivistic attempt to translate theories into statements about observations. And so the question arises: Are instrumentalism and descriptivism ultimately the same thesis?

45 Ibid., p. 139.


47 Nagel, op. cit., p. 139.
According to instrumentalism, all the concepts and statements of science would be either logical or observational concepts and either observational statements or material rules of inference, respectively. If instrumentalism and descriptivism are not ultimately the same view, a synthesis between them might be attempted. One could hold that all the content of a theory is complete translatable into observation statements and that the remaining part is simply structural—that it relates the observation statements together either in the form of explanations or predictions.

One might now ask: Which of Feigl's views is closest to instrumentalism? The closest view is formalistic phenomenalism because it holds that theories are formal (logical) constructs linking observational statements. Feigl emphasizes the notion of translation or reduction in all of his non-realistic views, whereas Nagel emphasizes the instrumentalist's view of theories as logical tools for organizing and predicting phenomena. Perhaps ultimately both views, instrumentalism and formalistic phenomenalism, are the same view.

Besides Nagel's criticisms of instrumentalism some other important criticisms have been made. Brody and Capaldi claim that instrumentalism's greatest difficulty is accounting "for the explanatory power of theories." Of course the soundness of this criticism rests upon just how one construes scientific explanation.

Another criticism of instrumentalism has been made by two of its most outspoken critics: Karl R. Popper and Paul K. Feyerabend, who are both realists. Popper believes that if theories are instruments, then

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48 Brody and Capaldi, op. cit., p. 10.
they cannot be refuted, since they have no truth value. But if they
cannot be refuted, then they cannot be tested. Consequently, there
would be no good reason to discard any particular theory.\(^{49}\) Agreeing
with Popper, Feyerabend adds that instrumentalism is conservative, dog­
matic, and if followed by science, can only lead to the petrifaction of
scientific knowledge and progress.\(^{50}\) According to Popper and
Feyerabend, the acceptance of instrumentalism would be rather unwise
for scientific practice. Realism, on the other hand, "encourages re­
search and stimulates progress . . . It is a very positive philosophy
and a very optimistic philosophy."\(^{51}\) On that note I think I had
better present the view called 'realism.'

According to Nagel, realism is the oldest view on the cognitive
status of scientific theories, and instrumentalism is the youngest.\(^{52}\)
While Nagel may be correct, there does not seem to be universal agree­
ment on this point. Feyerabend cites\(^ {53}\) Proclus (411-485 A.D.) and
Feyerabend\(^ {54}\) and Popper both cite\(^ {55}\) Oriander and Bellarmine (contempo­
raries of Galileo) as holders of instrumentalism with regard to some

\(^{49}\) Karl R. Popper, "Three Views Concerning Human Knowledge,"
Conjectures and Refutations: The Growth of Scientific Knowledge (New

\(^{50}\) Paul K. Feyerabend, "Realism and Instrumentalism: Comments on
the Logic of Factual Support," The Critical Approach to Science and

\(^{51}\) Ibid.

\(^{52}\) Nagel, op. cit., pp. 117-118.

\(^{53}\) Feyerabend, op. cit., p. 281.

\(^{54}\) Ibid., p. 290.

\(^{55}\) Popper, op. cit., pp. 97-100.
scientific theories (especially the heliocentric theory of the dynamic structure of the solar system). Thus if instrumentalism is the youngest, historically descriptivism would have had to fall between realism and instrumentalism. Due to descriptivism's emphasis on the linguistic techniques of translatability and reduction (although part of its origin seems to be from the British empiricists: Berkeley, Hume, and Mill), its total formulation is probably an early twentieth century one. In any case there seems to be agreement that realism is the oldest view.

Briefly stated, realism is the view that scientific theories are, besides being instruments of explanation and prediction, assertions about physical reality. As Popper puts it: "The scientist aims at finding a true theory or description of the world ... which shall also be an explanation of the observable facts." Popper holds that theories, if they are testable, do assert something about reality since the theories imply that certain things can happen whereas others cannot. It does not follow from that point alone, of course, that theories assert something about unobservable entities, events, or properties—which Feigl claims. The main difficulty with realism is in justifying the claim that theories do assert something about such unobservables. Most philosophers of science today agree that we cannot know for certain that, say, atoms or genes exist. But they claim that the evidence is such that their existence is well confirmed. In order to evaluate

56Ibid., p. 103.
57Ibid., p. 117.
whether the theories are well confirmed, I will later examine in detail the realist's logical technique of theory confirmation.

Before presenting some of the major objections against realism, I want to make a few comments about Nagel's view on the controversy between the different views. Nagel holds that ultimately there are only two views: instrumentalism and realism. Discriptivism becomes assimilated into instrumentalism because no translatability technique has worked satisfactorily. Since only observational statements and statements reducible or translatable into them are true or false or have empirical content, it would seem that the instrumentalistic view best favors the descriptivist's attitude. Secondly, Nagel holds that it is merely a matter of use whether a linguistic structure functions as a rule of inference or a premise-statement. This point tends to indicate that the controversy between instrumentalism and realism is simply "a conflict over preferred modes of speech." Yet Arthur Pap, in attempting to answer the question of whether theories are descriptions of reality or instruments of prediction, says that any evidence for the one view is evidence for the other because it is a pseudoquestion: they are logically equivalent. Third, Nagel adds that both theories can meet satisfactorily any criticism brought against them. Hence it would seem that which view is accepted depends more on the psychological

59Ibid., pp. 128-129.
60Ibid., p. 152; cf. also p. 141.
62Nagel, op. cit., p. 152.
background of the individual, i.e., on what mode of speech he prefers. It is obvious from Popper's and Feyerabend's quotes that neither believes the controversy to be merely linguistic. Thus we shall have to examine not only the controversy between instrumentalism and realism but also the controversy between those who hold that controversy to be merely verbal (ultimately a difference in how one wants to talk about theories) and those who consider it much more significant (ultimately a question of whether we can have knowledge of unobservables).

Let us now examine different realistic views. First, there is the view that what is real is that to which theoretical terms refer, and that what observational terms refer to are the appearances of the real. For example, tables and chairs are the appearances of atomic and subatomic particles. This is the view advocated by Arthur S. Eddington and in a modified sense by Wilfrid Sellars. Most people would probably find this theory shocking since it goes against the common-sense view that if anything is real, tables and chairs, and rocks and trees are.

A moderate form of realism holds that only some theoretical concepts have factual reference. This is an attempt on the part of some realists to explain the difference between theoretical concepts like 'instantaneous velocity' and 'electron' without giving up factual reference all together. One way of viewing the matter is the way of


William Kneale. According to Kneale, in a theory we suppose the existence of certain things possessing certain relationships which he calls their structure. It is by means of their structure that we can infer the existence of such entities because there can be no structure without content and conversely. What their content is we can never know: it is beyond the possibility of experience. However, the structure is what relates these entities to what are possible objects of experience: causal and logical relationships. Hence it is by means of their structure that theories have empirical import. This structure Kneale is concerned with will have to be carefully examined later.

Realism, like the two other views, is not without its problems. One of the problems is explaining the important connection between theoretical statements and observational statements. Is it deductive or inductive? Of course one way of solving this problem is to deny the distinction between theoretical concepts and observational concepts. If that distinction cannot be made or justified, then realism has no problem of explaining any such connection. Yet a new problem arises in attempting to distinguish descriptivism from realism, because ultimately what descriptivism holds is that there is no distinction between theories and observational statements provided the theories have empirical import. Perhaps Nagel and Pap are correct, then, in holding that there is no sound logical distinction between realism, descriptivism, and instrumentalism.

Another objection is one presented by Brody and Capaldi, viz.:

in the history of science we often find two incompatible theories to explain the same thing. . . . If theories are a description of an underlying reality, how is this possible?  

The strength of this criticism rests on the claim that one does find incompatible theories in science explaining the same thing. This point seems to depend heavily on interpretation, and the realist could reply that the theories in question only seem to be incompatible but actually are not. In showing the compatibility of the theories, the realists might prove that one theory is deducible from the other or that both are deducible from an even more general theory.

Another criticism of realism mentioned by Brody and Capaldi is that most philosophers of science agree that no scientific theory is final. Some scientific theories have been modified so much that they bear little semblance to their initial formulation. And it seems to be the case that, in time, any theory formulated today will either be rejected completely or modified tomorrow. Perhaps this shows that theories are but summarizations of presently known observational facts that can be modified or rejected in accordance with later observations.

Section IV
Criteria of Reality
Part I
Nagel's Criteria

In order to successfully defend realism, one must present a criterion of physical or factual reality. (By 'physical' I do not

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66 Brody and Capaldi, op. cit., p. 9.
67 Brody and Capaldi, op. cit., p. 9.
necessarily exclude 'psychological.' This criterion is important in evaluating whether we are using 'reality' univocally when we say that a table is real and an electron is real. If we are not, then we may have some basis for distinguishing theoretical from observational concepts.

Nagel presents five possible criteria of factual reality: (1) the entity must be publicly observable under certain conditions (the criterion for physicalism); (2) the theory must be confirmed by empirical evidence (usually held to a necessary condition for any theory to be scientific); (3) the term presumably designating a physical reality must occur in more than one universal observation statement or experimental law—and these laws are "logically independent of each other and ... none of them is logically equivalent to a set of two or more laws"; 68 (4) the term designating a physical reality must occur in a well established "causal" or process law (raises the question as to what counts as a causal law); and (5) the entity or what is real must be "invariant under some stipulated set of transformations, changes, projections, or perspectives." 69 As to the last criterion, the invariant properties of the entity are usually called its real or primary properties. But invariance is a function of the manner of transformation, i.e., given one set of transformation rules a property does not undergo change but, given another set, it does.

Some comments of clarification are needed for criterion (3). This criterion demands that what is factually real be identified by procedures besides those which are used to define it. If the term is

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68 Nagel, op. cit., p. 147.

69 Ibid., p. 149.
theoretical and (perhaps) is thought to refer to an unobservable physical reality, then that term must be related to observable or experimental concepts and "these experimental concepts must enter into at least two logically independent experimental laws which can be derived from the theory."70 The theoretical terms are related to the experimental terms by means of correspondence rules (Feigl's coordinating definitions). Nagel lists theoretical terms such as 'mass of a molecule' and 'mean kinetic energy of molecules' as having physical reality in this sense. Yet other terms, such as 'electric field,' seem to appear only in one experimental law. According to this criterion, then, they would not have factual reference. It may be possible for new and independent experimental laws to be formulated containing 'electric field' which would then establish its physical reality. If this is possible, then the best way of considering theoretical terms that are found only in one law is to construe them as having factual reference despite lack of any warranting evidence. This criterion is also used to insure both the "predictibility of a theory" and its being able to explain phenomena other than those which it was initially formulated to explain. It thus attempts to insure a theory against being completely ad hoc.

Nagel notes that criterion (3) is compatible with instrumentalism.71 What the criterion ultimately amounts to saying is that a successful theory can be tentatively accepted as true. In order to defend realism at this point, the realist must show that a successful theory

70Ibid., p. 148.
71Ibid., p. 151.
is indicative of genuine factual reference; in other words, that one is not simply defining physical reality in terms of experimental success—which is compatible with instrumentalism. Even if, moreover, one holds that all five criteria have to be met for a theoretical term to have factual reference, it is not obvious that the instrumentalist is incorrect since these criteria themselves could be construed as merely rules governing the use of 'physical reality' or 'factual reference.'

Part II

Bergmann's Criteria

Another set of criteria for reality are presented by Gustav Bergmann. Although Bergmann is discussing the uses of 'exist,' I shall apply his criteria to the realism-instrumentalism controversy over physical reality.

The first criterion of Bergmann's I wish to consider is: What exists is what is "in" space and time. Physical objects are in physical space and time and sensa (the immediately perceivable) are in phenomenal space and time. This criterion does thus not commit us to either physicalism or phenomenalism. This criterion is to be compared with Nagel's first criterion of publicly observable. The publicly observable only permits as real physical objects. So it seems that Bergmann's criterion encompasses more possibilities of what could be real than Nagel's first criterion.

73 Ibid., p. 110.
Another thing to note is whether the properties (including relational properties) of either physical objects or sense data are in space and time. Bergmann holds that their properties and relations "are not literally in space and time."74 But if a physical object is simply identical with all its properties (including its relational properties), then would Bergmann be correct? One might hold that a physical object is a set of properties and relations; the set exists in space and time, but its members (the properties and relations as individuals) do not. The relations, especially the spatial and temporal ones, probably determine the reality of a thing. In any event, this criterion of reality is helpful to the descriptivist, realist, and instrumentalist. There would be differences between them as to which is more fundamental—physical objects or sensa, but this is a different issue from the issues separating the three.

Bergmann's next criterion (acquaintance) reads: "We cannot know anything to exist unless we are 'acquainted' either with it or with a part of it or, wholly or in part, with a thing of its sort."75 A crucial term in this criterion which needs explication is 'being acquainted with.' According to Bergmann one explication is that we are acquainted with what we perceive76; and what we perceive are perceptual things such as perceptual objects and their properties and relations such as stones and tables, colors and tastes. This explication would be the physicalist's. According to another explication, the phenomenalistic, we are acquainted only with sense data and their properties and

74ibid.
75ibid.
76ibid.
relations as well as mental activities such as perceiving, imagining, and remembering. Although this criterion favors descriptivism, it could be used for a moderate type of realism which was mentioned earlier, namely, that at least some theoretical terms do not have factual reference. In any case reality is limited, at most, to what we are capable of being acquainted with.

The next criterion (simplicity) states: "Some entities are 'simples'; all others 'consist' of simples; the former exist; the latter don't."  A careful examination of this criterion will show that, according to it,: (1) there are two kinds of entities: simples and those that consist of simples and (2) only simples exist. One wonders just what is meant by 'an entity'? What do both have in common that enables them to be univocally so-called? In order to answer this question we will have to know what a simple is. Continuing in the physicalistic-phenomenalistic manner, one could say that the simples are perceptual (physical things) or phenomenal (sensa).

Another important term in this criterion is: 'consists of.' Bergmann explains that term by the relationships of definition, i.e.,

An entity "consists" of others if and only if the expression . . . referring to it can be defined in terms of expressions referring to those others. The definiendum refers to the defined entity and the definiens refers to the defining entities. Since the criterion states that all other entities consist of simples, then, apparently, a defined entity would consist of simples which would be the defining entities. Bergmann's

77Ibid., p. lll.
76Ibid.
example is the following: let 'x is bay' be defined as 'x is a horse and x is tawny.' This example is about the definition of a character rather than an object. One might also wonder if 'x is a horse' is a simple. The criterion seems to deny the possibility of a complex (a non-simple entity) being composed of simpler complexes.

If the defining entities are exclusively physical objects (and their properties and relations) or sensa then again we have a criterion which favors descriptivism. The definiendum and the definiens have by virtue of notational convention the same referent. The definiendum is merely an abbreviation for the definiens. The descriptivist's technique is to define all theoretical terms by terms referring only to entities we are capable of being acquainted with. Yet according to this criterion the referent of the defined term would include conjunction. Since only simples exist, their conjunction does not. For instance, in examining Bergmann's example, 'x is a bay' is defined as 'x is a horse and x is tawny,' we see that the definiens is a conjunction of two simples: 'x is a horse' and 'x is tawny.' And although there is at least one thing that is a horse and at least one thing that is tawny, it is not the case that there exists any tawny horse simply because that entity is a complex and only simples exist.

If one uses this criterion for reality, then one might be able to hold and show that the referents of theoretical terms do not exist because they consist of relations of simples; and only simples exist. But one could turn this argument around and present, in a manner similar to Eddington's, the view that the referents of theoretical terms

79 Ibid., p. 112.
are the simples (like atoms) and the complexes are the physical objects we perceive and the sensa we are directly aware of (e.g., tables, chairs, after-images, tastes). Thus, atoms would exist but physical objects or sensa would not.

This last observation seems to clarify further the descriptivism-realism controversy. Extreme realism and extreme descriptivism appear to be the converse of each other. According to extreme realism, the real is the referents of theoretical terms only; according to extreme descriptivism the real is the referents of observational terms only. Extreme realism holds that the referents of observational terms consist of the referents of theoretical terms and descriptivism holds that the referents of theoretical terms consist of the referents of observational terms. It is possible, then, for both the realist and descriptivist to accept generically the same criterion for reality but, of course, in explicating its key terms they would have to differ. The fact that both views can accept the same criterion though they either interpret it differently raises a problem: How do we evaluate their view on what is the simple? Is the disagreement ultimately verbal, i.e., is it merely a matter of what you want to count as simple? Or is it more objective, i.e., is there a factual or empirical basis for establishing the simple? Certainly I will have to examine these possibilities carefully.

The next criterion (significance) Bergmann examines is the criterion that:

A defined entity exists if and only if it is significant; an entity being significant if and only if the expression referring to it occurs in statements of lawfulness which
we have reason to believe are true.\textsuperscript{80}

Notice that this criterion, unlike simplicity, permits the existence of defined entities provided they meet certain conditions of significance. This criterion and the simplicity pattern could be merged as a distinct criterion that permits the existence of both simples and significantly defined entities.

Since the significance of a term is determined by whether it occurs in statements of lawfulness believed to be true, this criterion is similar to Nagel's third criterion mentioned above in Section IV. According to Nagel's criterion, the term has to occur in more than one logically independent law. (Nagel's criterion, however, says nothing about the laws being held as true though he probably supposes they are according to that criterion.) Because Bergmann's significance criterion and Nagel's third criterion express reality in terms of statements of laws, the important task in both explicating and defending them is to determine a criterion for lawfulness. The criterion for lawfulness could definitely have a bearing on the descriptivist-instrumentalist-realist issue in that one or more positions may not be able to hold that criterion for lawfulness. If, for example, the descriptivist or instrumentalist construes all universal conditionals as material implications, then he may not have any way of distinguishing accidental universal conditionals from nomological (lawlike) universal conditionals. Such lack of distinction might hinder the development of an adequate account of the role, function, and significance of theories and theoretical terms in science. Hence this problem will

\textsuperscript{80}\textit{Tbid.}, p. 114.
likewise have to be examined.

The next criterion Bergmann present, the realism pattern, states: "Perceptual things exist." In presenting this criterion, Bergmann makes two distinctions: (1) perceptual things versus physical things, and (2) perceptual things versus phenomenal things. A perceptual thing, explains Bergmann, is anything that I perceive. The causes of what I perceive are physical things, and science tells me what these causes are. For instance, what I perceive when I see a chair is a perceptual chair and my seeing the perceptual chair is caused (at least partially) by a physical chair. Phenomenal things, on the other hand, are either sensa or awarenesses. Bergmann classifies sensa and their properties and relations as primary phenomenal things; and awarenesses, as mental phenomenal things. The distinction between a physical thing and a perceptual thing is somewhat clear, since it is ultimately the distinction between a cause and its effect. However, what is the distinction between a perceptual thing and a phenomenal thing?

According to Bergmann, the phenomenalist explains what a phenomenal thing is on the basis of the acquaintance criterion; i.e., phenomenal things are the only things we are directly aware of. Thus for the phenomenalist, perceptual things do not exist. But it seems to me that if the phenomenalist denies anything as existing it would be physical objects as unperceived things causing what is perceived. In making a distinction between phenomenal things and perceptual things, Bergmann

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81 Ibid., p. 118.
82 Ibid., p. 117.
83 Ibid., pp. 118-119.
must make a distinction between being directly aware of something and perceiving it, or between what we are directly aware of and what we perceive, or both.

In attempting to draw this distinction, Bergmann uses the example of seeing. When I am seeing a chair, Bergmann believes that:

I am directly aware of an act, i.e., of an awareness and the two simple characters it exemplifies. One of the two is perceiving... the second character the awareness exemplifies is 'the proposition (that) this is a chair.'

The second character thus is a propositional character. Hence when I see a chair, I am not directly aware of a chair, but I am directly aware of my visually perceiving the chair (first character), and I am directly aware that what I am perceiving (usually) is a chair (second character). Bergmann's analysis at this point begins to clarify the distinction between mental phenomenal things and perceptual things.

But how are we to distinguish primary phenomenal things from perceptual things? Assuming that we are sensing a green after-image, what would be Bergmann's analysis of that act? Probably, that I am directly aware of an act; my sensing visually (first character) a green after-image and that what I am sensing visually is a green after-image (second character).

Notice the main differences in the two analysis: Perceiving in the first and sensing in the second. Sensa can only be sensed and perceptual things can only be perceived. In another essay Bergmann explicitly states that we never perceive (see, hear, taste, smell, and touch) sense data; they can only be sensed.

\[84\] Ibid., p. 119.
\[85\] Ibid.
type of acquaintance. (See the acquaintance criterion, above.) Sense data are identical to phenomenal things and, for Bergmann (in a later essay), a thing is "phenomenal if and only if it is in a mind, i.e., . . . if, it is 'in' a conscious state." To be 'in' anything is to be internal or intrinsic to that thing. Sensing is that kind of acquaintance (direct awareness) which intends (purports to refer to) a sense datum. However, a sense datum does not present the idea of external existence as, apparently, perceptual objects do. Therefore, "sense data are nonintentional facts 'in' conscious states." Now if sense data are to sensing as perceptual things are to perceiving, then, if sense data are nonintentional partly because they do not present the idea of external existence whereas perceptual things do, are perceptual things intentional? If they are intentional, do perceptual acts sometimes intend physical objects? If they do not intend anything but are, themselves, intended by something else, what is that something else since it cannot be a sense datum?

Perhaps it is not clear what I mean by 'sense data are to sensing as perceptual things are to perceiving.' Bergmann, as stated earlier, holds that perceptual things are what we are acquainted with when we are perceiving. On the other hand, phenomenal things (sense data) are

88 Ibid., p. 305.
89 Ibid., p. 326.
90 Ibid., p. 326.
91 Ibid., p. 326.
what we are acquainted with when we are sensing (are directly aware of). So there seems to be a similarity between them with respect to the acquaintance criterion. Bergmann enumerates three different uses of 'perceiving': 'perceiving\textsubscript{1}', 'perceiving\textsubscript{2}', and 'perceiving\textsubscript{3}'.\textsuperscript{92} Perceiving\textsubscript{2} involves that act of awareness which intends a perceptual fact. According to Bergmann all mental acts are propositional and intend that which the proposition purports to describe. Thus, when I perceive a chair the text of the propositional part of that awareness is 'This is a chair'—which intends the perceptual fact that this is a chair. Yet a perceptual fact does not seem to be a perceptual thing, since Bergmann explicitly states that chairs are perceptual things.\textsuperscript{93} It might seem that perceptual things are constituents of perceptual facts, since 'chair' is a constituent of 'This is a chair.' However, analyzing the situation that way may be confusing grammatical constituency with ontological constituency. Just because 'chair' is a part of 'This is a chair' it does not follow that the perceptual chair is a part of the perceptual fact that this is a chair. Since, moreover, 'this' might be construed as identical to 'the chair,' perhaps the perceptual chair is identical to the perceptual fact. Whether the perceptual thing is a part of or identical to the perceptual fact is irrelevant in asking the initial question: do perceptual things intend or are they intended? Since acts of perceiving intend perceptual facts, then perceptual things either are intended directly (if they are identical) and indirectly (if latter is a part of the former). (Since

\textsuperscript{92}Ibid., p. 313ff.

\textsuperscript{93}Bergmann, "Physics and Ontology," p. 117 and "Realistic Postscript," pp. 312, 338.
Bergmann, as quoted earlier, stated that sense data are non-intentional facts, then taking our cue from that and their similarity due to acquaintance, it would seem that perceptual things are identical to perceptual facts.

The preceding discussion was an attempt to clarify what Bergmann calls the realism pattern (his next criterion for existence), viz.: "perceptual things exist." To call this criterion 'realism' may seem unjustified until one knows that Bergmann holds that perceptual things have a mind-independent existence; i.e., they exist even when no mind is intending them. But Bergmann holds that "an entity is real if and only if it is actual, i.e., not merely a possibility, and, in case it is not mental, mind-independent." According to this criterion of real, all actual mental entities are real and likewise all actual non-mental mind-independent entities, such as genuine perceptual things and physical objects. In determining the reality of a perceptual object, Bergmann appeals to the coherence criterion which involves testing what we expect to perceive given other perceivings in accordance with laws about the property and relational behavior of the perceptual things in question. It seems that the coherence criterion is in some way connected with the significance criterion since the only way we satisfy ourselves as to the truth of laws is by testing them.

The last point needing clarification in this criterion is the distinction between perceptual object and physical object. Bergmann

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95Bergmann, "Realistic Postscript," p. 316.
96Bergmann, "Realistic Postscript," p. 312.
maintains that the phenomenalist constructs perceptual things out of phenomenal things and a positivist (physicalist) constructs physical things out of perceptual things.\(^{97}\) He advocates the view that physical objects are reconstructions of perceptual things.\(^{98}\) What is the difference between a construction and a reconstruction? In a construction what is being formed has no existence apart from that out of which it is composed. Thus, for the phenomenalist a perceptual thing is identical to a set of sense data. On the other hand, a reconstruction, as Bergmann understands it, is a kind of replacement of one thing with another. Thus, with scientific progress, the scientist slowly begins to replace the perceptual object with the physical object because of the greater and more precise explanatory success of the latter in theory. Both exist, Bergmann maintains, but not in the same way.

First of all, physical objects can neither be sensed nor perceived. Physical objects (as was mentioned earlier) are causally connected with perceptual things. For instance, we perceive flickers on scintillation screens and Geiger counter clicks but not the particles causing the flickers and clicks. Thus Bergmann is perhaps holding to an observational term-theoretical term distinction based on his distinction between perceptual and physical things. But since physical things are reconstructed from perceptual things, perhaps the difference between them is one of degree.

The criteria for the existence of a physical thing is developed

\(^{97}\)Ibid., p. 338; also "Physics and Ontology," p. 120.

\(^{98}\)Ibid.
in Bergmann's last two criteria: the process pattern and the model pattern. The process pattern is affirmative; it holds that physical objects (particles, theoretical entities) exist. According to this criterion, from a complete description of a state at any one time in conjunction with a process law, descriptions of its earlier and future conditions may be computed. Depending on the law, the prediction or postdiction will be absolute or statistical. In any case this criterion states that those entities which correspond to the undefined non-logical terms of the state description or law exist. If the law is absolute (a law in classical mechanics, for instance), then the particles would exist in time and space (see concreteness criterion above). Bergmann justified the last assertion on the basis that the classical particle is primarily like the macro-object (the physical object) except for its size. For instance, both have mass and velocity, and thus are in space and time.

There is one point about this criterion that should be noted, viz., that those entities exist which correspond to the undefined non-logical terms of the laws. This seems to be rather weak or rather obvious. If something does correspond to a term it seems obvious that that term has factual reference. Yet the question is how we know that anything corresponds to the non-logical terms of a law. It would seem that the task of a criterion is to show us how to determine whether or not anything corresponds to the non-logical terms. One could, however, argue just as soundly that the purpose of a criterion is to set down certain conditions independently of our being able to verify them. The

99Bergmann, "Physics and Ontology," p. 121.
concreteness criterion, for example, merely states that what exists or is real is that which is in space and time. The criterion does not tell us how to establish a thing's being in space and time. It would thus seem that something else is needed, perhaps another criterion, before we can apply the concreteness or process criterion in finding out what exists.

The final criterion, the model pattern, is negative in that it states that "the entities of the model do not exist." According to this pattern, the micro-language (statements about particles or micro-objects) are connected to the macrolanguage (statements about macro-physical objects). The connectors are other statements called coordinating definitions, though they are not the usual sort of definition. If one makes a distinction between macro-physical objects and perceptual objects as Bergmann does, then the macrolanguage is connected to the perceptual language by other coordinating definitions. Either the language of macro-objects or the language of perceptual objects is taken to be basic, and the microlanguage is understood in terms of it. (If one is a phenomenalist the basic language will be a sense-datum language.) The model is that linguistic system which is coordinated or understood in terms of the basic language. Since there is only one basic language, all other linguistic systems are to be interpreted in terms of it and, as a consequence, micro-object terms are to be understood only in relation to macro or sense datum terms. The notion of a micro-object as micro became misleading on this view; there are no such entities. A micro-object is a certain way of construing macro-objects:

100Ibid., p. 121.
either perceptual objects or sense data. This view is definitely descriptivism; it could also be instrumentalism because of the instrumentalist's tendency to view theories as rules of inference concerned with relations between statements rather than physical entities.

The model pattern of Bergmann should be compared with Nagel's second criterion for reality, viz., that an acceptable theory must be confirmed by empirical evidence. The usual justification for a basic language seems to be that the basic language has empirical import. Consequently, if any other language is to possess empirical import (usually held to be a necessary condition for a scientific theory), then it must be interpreted in terms of such a basic language. These, then, are the criteria Bergmann discusses which have a bearing on the descriptivism-instrumentalism-realism controversy.

Part III

Ramsey's and Braithwaite's Criteria

Braithwaite presents two criteria for the reality of theoretical concepts. The first one goes back to an essay by Frank P. Ramsey in a collection of essays, The Foundation of Mathematics, which Braithwaite edited. According to this criterion the ontological status of a theoretical concept (such as 'is an electron') is determined by a statement specifying what its status is in a deductive system. That statement is the following:

There is a property $E$ (called "being an electron") which is such that certain higher-level propositions about this property $E$ are true, and from these higher-level
propositions there follow certain lowest-level propositions which are empirically testable.\textsuperscript{101}

The higher-level statements are usually called laws and the lower-level statements are observational statements, i.e., statements about what we have observed. Braithwaite notes that the proposition (called a Ramsey-statement) does not tell us what is meant by being an electron. It only states that there are instances satisfying being an electron.

This criterion seems to be saying what Nagel's second criterion says, viz., that a theory must be empirically confirmable or testable. But most of all it sounds like Bergmann's criterion of significance, i.e., that an expression is significant if and only if it occurs in laws held to be true. The Ramsey criterion simply adds that the laws be part of a deductive system. Another way of putting the Ramsey criterion: a theoretical term expresses physical reality if it is a term in a (true) law that is deductively related to (true) observational statements.

The other criterion which Braithwaite discusses is a weaker criterion than Ramsey's or than Bergmann's significance criterion. This criterion does not answer directly the question whether electrons exist. It deals with the way in which the theoretical term functions in the calculus or deductive system. The meaning of the term is determined merely by its function in the deductive system. The theoretical term in this case would have meaning only in the context of

part IV
Hesse's Criteria

Hesse criticizes Ramsey's and Braithwaite's criteria for being too weak for realism. First of all she claims that the criteria do not permit one to distinguish "between different interpretations of the same formal calculus." Secondly, the criteria do not permit one to distinguish between concepts that are presented merely as useful fictions (such as 'fluid heat') and other concepts which are presented as referring to real entities, (such as 'molecule'). Hesse's criticisms emphasize the importance of content of the terms rather than their formal function in a calculus. This difference of function, however, is just the point at issue. A realist such as Hesse would hold that one needs a content criterion as well as a formal criterion in determining the reality of theoretical terms. On the other hand, those who are more instrumentally or formally inclined will hold that a formal criterion such as Braithwaite's is sufficient. The problem at this point would be to determine how one could resolve the conflict between the realist and the instrumentalist over which criteria are to be accepted. Hesse, by the way, holds that Ramsey's and Braithwaite's criteria are necessary conditions that a theoretical concept must satisfy in becoming a candidate for reality reference,

102Ibid., p. 80

103Hesse, "Laws and Theories," p. 408.
but she denies that they are sufficient.\textsuperscript{104} The simple point is that a realist would have to make Hesse's claim.

Hesse presents three nonformal criteria for reality or existence. The first may be called the \textit{fulfillment of expectations criterion}. According to this criterion, a theory's yielding successful expectations or predictions in new experimental situations indicates or confirms "the real existence of the entities to which it refers."\textsuperscript{105} The second criterion is that of \textit{observability and causal efficacy}. According to this criterion, in order for a theoretical entity to be scientifically real, it should under appropriate conditions have observable effects. Thus successfully predicted effects would confirm the existence of the theoretical entities (combining both the first and second criteria). Hesse notes that the second criterion enables us to:

\begin{quote}
exclude from the domain of existing entities any models which are deliberately introduced as fictions or instruments, since these would not be said to cause observable effects.\textsuperscript{106}
\end{quote}

Yet from the instrumentalist's point of view, there would never be any reason to hold that theoretical entities possess causal efficacy as well as formal relationships, simply because they do not refer to things or events that have causal efficacy. Even the descriptivist could agree with the instrumentalist at this point because theoretical entities could be complexes of observable things or events related.

\textsuperscript{104}Ibid.
\textsuperscript{105}Ibid.
\textsuperscript{106}Ibid.
causally and logically. There is for both the instrumentalist and descriptivist no need in science for unobservable causes.

Hesse's third criterion could be called the **substance criterion**. According to this criterion only substances or individuals exist whereas properties, relations, or classes do not. Hesse believes that this criterion, unlike her second one and others that have been noted, does not depend upon what is observable. The distinction is more of a logical one, i.e., between substantives and predicates. The problem that immediately arises at this point is how to distinguish substantives (words referring to substances or individuals, i.e., having factual reference) and predicates (words referring to properties, relations, or classes). If the distinction between substantives and predicates is logical, one might conclude that one can decide what are individuals and what are properties by considering a formalization of the theory under investigation. Yet this way of making the distinction appears to be somewhat arbitrary. For depending upon the manner of formalizing, in one formalization of a theory a term may refer to a substance whereas in a different formalization the same term refers to a relation or property of a substance. Hesse believes that different formalizations are due to different preformal interpretations as to what are substances and what are predicates. But her view need not be the only one. It seems quite possible that different formalizations are set up with the view of solving a problem, and that the formalization which best solves the problem (according to a criterion of solving problems) would be accepted over and against other formalizations. The criterion itself could simply be a formal criterion, such
as ease of predictability or calculation.

Hesse gives the example of two different interpretations in the field of electrostatics. According to one interpretation, mass-charges are the substances whereas "space-time positions and force-energy relations are the predicates." But according to the second interpretation field-energy or the space-time points could be the substances and mass and charges are properties of certain space-time points. Furthermore, she notes that the usual trend has been to construe as real whatever is conserved, i.e., exists throughout a period of time.

Although she does not clarify any further what she means by being conserved, she no doubt has in mind that which is believed to continue to exist at least through most if not all of the duration of the experiment.

The instrumentalist would probably argue that it is somewhat arbitrary which interpretation is accepted. It all depends on what we want to do with the theory; for example, one theory or formalization will be accepted on the basis of convenience rather than on the belief that one has factual reference. On the other hand the realist might reply that what is a matter of convenience is determined by the actual factual reference of a theory. In other words, a theory more in line with reality will be more convenient than one which is less in line with reality.

In conclusion, these are the problems, the attempted solutions of the problems, and the various concepts of 'real,' 'reality,' and

107 Ibid., p. 409
108 Ibid.
'exist' that I shall be analyzing and evaluating in detail. Since the topic is about the function and status of certain terms in theories, I shall begin the analysis by considering the different views on the general structure and function of theories in science.
CHAPTER II
THE STRUCTURE AND FUNCTION OF SCIENTIFIC THEORIES

Section I

The Traditional View of Scientific Theories

Before examining in detail the views on the cognitive status of theories (descriptivism, instrumentalism, and realism), I shall consider what the structure and function of a scientific theory has been thought to be apart from its cognitive or referential status. The main view I shall be considering is the view of Nagel and Carnap, and I shall call this view the traditional view. Furthermore, anyone who agrees (or seems to agree) with this view, I shall assume also to hold the traditional view.

What is a scientific theory? (Since I shall always be speaking of scientific theories, hereafter I shall usually drop the modifier 'scientific') According to Braithwaite:

A scientific theory is a deductive system in which observable consequences logically follow from the conjunction of observed facts with the set of the fundamental hypotheses of the system. A study of the nature of a scientific theory is thus a study of the nature of the deductive system used in the theory.¹

May Brodbeck also holds that a theory is a deductive system. Furthermore, since the deductive connections are established among

¹Braithwaite, op. cit., p. 22.
hypothetical generalizations, "a theory . . . is often referred to as a hypothetico-deductive system. . . . "

According to Milton Friedman, the goal of science is to formulate theories or hypotheses which yield "valid and meaningful (i.e., not truistic) predictions about phenomena not yet observed." Popper holds that the aim of all theories is to explain phenomena deductively. However, Popper, unlike the previous writers, holds that "scientific theories are universal statements." The other writers have said that scientific theories are deductive sets of statements. (I shall subscribe to calling a theory a set of statements. Whether the statements should have a deductive structure is a question that needs further examination.) To sum up the above quotes, it is found that a theory is considered to be a deductive system that has (1) observable or testable consequences, (2) some universal or general premises, (3) predictive import, and (4) explanatory power. These four aspects are part of the traditional view of the function of theories.

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5Ibid., p. 59.
Section II

The Structure of a Theory

According to the traditional view, a theory structurally has three components:

(1) An abstract calculus that is the logical skeleton of the explanatory system, and that "implicity defines" the basic notions of the system; (2) A set of rules that in effect assign an empirical content to the abstract calculus by relating it to the concrete materials of observation and experiment; and (3) An interpretation or model for the abstract calculus...

According to Nagel an abstract calculus is a deductive system in which only the logical relations of the terms to one another can be analyzed. One's attention is not concerned with that to which the nonlogical terms refer, only with their logical interconnections. If a theory has been so abstracted, its fundamental assumptions or premises become a set of abstract or uninterpreted postulates. To say that the assumptions are uninterpreted postulates is to say that their "nonlogical terms have no meanings other than those accruing to them by virtue of their place in the postulates." Having this latter meaning is what Nagel means by the expression 'being implicitly defined by their postulates.' In other words, the only significance the nonlogical terms have in such a calculus is the synthetic relationships expressed in the uninterpreted premises or axioms. Further relationships between the terms can be derived by deducing from the postulates other linguistic abstractions. It should be noted that neither the postulates of an

6 Nagel, op. cit., p. 90.

7 Ibid., p. 91.
abstract calculus nor anything deducible from them have any truth-value since they are statement-forms rather than statements. For instance, if the theory in question is the kinetic theory of heat, then some of the postulates of the theory refer to molecules. But if the theory is abstracted into its logical form, then the implicit meaning of 'molecule' would be merely all the relations connecting 'molecule' with other terms in the abstracted postulates and statement-forms deducible from them. (For convenience I shall call any statement-form deducible from postulates a 'theorem.') Nagel claims that the only way of determining what is meant by 'molecule' is by examining the postulates of the molecular theory. Anything which satisfies the conditions of the postulates concerned with molecules would be a molecule.

A question that arises in considering implicit definition is whether implicit definition has any bearing on a term's factual reference, i.e., can we ascertain merely by the implicit definition of a concept whether the concept refers to any extralinguistic entity. In deciding this question, one will have to examine the postulates. Now if all the postulates are hypothetical, e.g., 'If x is an A, then x is a B,' then one would not, of course, be able to determine whether the concept has factual reference. But what if one of the postulates is existential, e.g., 'There is at least one x that is an A'? An example of an interpreted existential postulate might be: 'There are molecules and they are elastic.' Such a postulate does purport to express factual reference but, of course, one cannot determine its truth (if it is interpreted) merely by the implicit definitions alone. Thus it would seem that postulates, either interpreted or uninterpreted, can
be hypothetical or existential. If they could only be hypothetical, then so would be their interpretations and, consequently, theories would not have any factual content since (1) hypothetical statements have no factual content (in the sense of being categorical existential assertions) and (2) from a set of hypothetical statements, only other hypothetical statements are deducible. Thus, in order for a theory to have factual import at least one of its postulates must be existential.

Section III

Correspondence Rules

Since abstract postulates themselves are neither true nor false, empirical content must in some way be assigned to the postulates in order for the system to be scientifically (empirically) useful. (Such a system may be logically or mathematically useful without empirical interpretation.) Consequently, if a "theory is to be used as an instrument of explanation and prediction, it must be somehow linked with observable materials."¹⁸ Since the uninterpreted postulates are statement-forms which are linguistic entities and their interpreted statements are also linguistic entities, it would seem that what links them are also linguistic entities. Such linguistic links have been called "coordinating definitions, operational definitions, semantical rules, correspondence rules, epistemic correlations, and rules of interpretation."¹⁹

Nagel believes that there is no single schema that represents the

relationship between the uninterpreted postulates and their interpreted statements. Nevertheless, Nagel attempts to illustrate how postulates are given experimentally testable interpretation by appealing to the Bohr theory of the atom. In brief, according to that theory, observable line spectra of various chemical elements is associated with the jump of an electron from one orbit to another orbit in an atom of the tested element. "In consequence, the theoretical notion of an electron jump is linked to the experimental notion of a spectral line." Nagel at this point is considering the relationships between theoretical notions and observational procedures (i.e., any procedure capable of being carried out experimentally). But the initial discussion was concerned with the relationships between the uninterpreted postulates and their interpreted statements; for instance, such as the relationship between 'F=ma' and 'the force of a moving body is the product of its mass and acceleration.' In this example the relationship is between uninterpreted schema and interpreted schema whereas in Nagel's example it is between 'theoretical notion' and 'observational' or 'testing procedures.' These two relationships are not necessarily the same. Only if we identify 'uninterpreted' with 'theoretical' and 'interpreted' with 'observational' or 'experimental' will they be the same. It is true, of course, that theoretical notions by themselves are observationally uninterpreted (assuming that the distinction between the theoretical and the observable is justifiable). Yet it

10 Ibid., p. 93.
11 Ibid., pp. 94-95.
12 Ibid., p. 95.
might be possible to give a term or schema an interpretation other than an observational one. For instance, the mathematical interpretation of the schema '7-5=12' is apparently not an observational interpretation. Nevertheless one could hold that those terms which are given a non-experimental interpretation are not either theoretical notions or observational notions. They are logical or mathematical notions and lack justifiable factual reference. According to this view (descriptivism, of course) only those terms having an observational interpretation possess any justifiable factual reference. Thus it seems that we can make a distinction between 'uninterpreted' and 'theoretical' and between 'interpreted' and 'observational' except where we are dealing with factual reference.

Furthermore, rules of interpretation are still needed even if one does not hold to the theoretical-observational distinction, because of the even more basic distinction between uninterpreted schema and interpreted notions or statements. If one does not want the methodology of science to deal with such rules of interpretation, then one will have to disagree with Nagel's claim that a theory structurally is an abstract calculus with a set of interpretative rules. After all, Nagel is examining the theory's logical structure and the distinction is quite justified though it may not be too useful or significant. In any case, to avoid possible confusion, I shall call those linguistic entities connecting uninterpreted schema with interpreted schema 'rules of interpretation' and those linguistic entities connecting theoretical concepts and statements with observational concepts and statements 'rules of correspondence.' It may be the case that rules of correspondence are a subgroup of rules of interpretation (if descriptivism, for
instance, is correct).

Nagel and Carnap are concerned primarily with rules of correspondence rather than rules of interpretation. Thus the fundamental assumptions of the Bohr theory provide (empirically) "only implicit definitions for the theoretical notions employed in them." As mentioned previously, in Bohr's theory the theoretical notion of an electron jump or transition from one orbit to another corresponds to the experimental notion of a spectral line. The problem arises as to what is meant by 'corresponds.' Nagel does not believe that 'electron transition' is explicitly defined by 'spectral line.' If a term has been explicitly defined, then it "may always be eliminated from any context in which it occurs, since it can be replaced by the defining expression without altering the sense of the context." Hence Nagel does not believe 'electron transition' has the same meaning as 'spectral line,' although a descriptivist might.

Nagel holds, as a result, that the correspondence rules are not explicit definitions. His reasons for this view are: (1) when some theoretical notions in certain statements are replaced by their corresponding observational notions in those statements a change of meaning occurs and (2) since theoretical concepts are implicitly defined by the postulates of the theory containing them, "there are therefore an unlimited number of experimental concepts to which, as a matter of

\[13\text{Ibid., p. 95.}\]
\[14\text{Ibid., p. 97.}\]
\[15\text{Ibid., p. 98.}\]
For example, not only does 'electron transition' correspond to 'spectral line,' it also corresponds to 'temperature in black-body radiation.' Yet one might wonder if we do not have two distinct theories here: one in which 'electron transition' corresponds to 'spectral line' and another in which 'electron transition' corresponds to 'temperature in black-body radiation.' Of course, there is nothing in Nagel's presentation of the structure of theories and of the function of correspondence rules which entails that we have two distinct theories. Nagel in fact seems to assume that within the same theory one theoretical term may have many different correspondence rules.

But for the moment two possibilities seem to be open: (1) for any theory, every theoretical term has only one correspondence rule, and (2) for any theory, every theoretical term has or may have more than one correspondence rule. In showing that correspondence rules are not explicit definitions of theoretical terms Nagel is holding, as correct, the second possibility. Yet since both views, prima facie, seem to be equally reasonable, is one view more justified than the other?

Carnap also agrees with Nagel in holding that correspondence rules are not explicit definitions. Carnap notes that Hilbert's system of geometry is an uninterpreted system. Thus, even though 'lines,' 'point,' and 'plane' appear in that system they are simply uninterpreted terms. When, however, we connect these terms to terms describing something physical, then they become physically interpreted. The
connectors are the rules of correspondence. "We can say, for example, that the lines of the geometry are exemplified by rays of light in a vacuum or by stretched cords." Line is not explicitly defined as 'stretched cord' because, says Carnap, cords are finite and geometrical lines are infinite. Thus, at most, physical lines are approximately geometrical lines; they do not share completely the same properties. Yet it seems that one would only use the correspondence rule 'the lines of geometry are exemplified by stretched cords' to the extent that the geometric lines and stretched cords have the same properties, or else the rule would be false, useless, or misleading according to the linguistic status given to the correspondence rules. If it is correct that they do share certain properties, then it seems reasonable to assume that under certain conditions of investigation the geometric line and the stretched cord could be equivalent. For instance, as a trivial example, they both possess length. Furthermore, Carnap's argument that part of the reason why 'geometric line' is not equivalent to 'stretched cord' might be weakened by considering 'geometric line-segment' as equivalent to 'stretched cord.' In defense of their non-equivalence one could note that cords, stretched and unstretched, have thickness; neither geometric lines nor line-segments do.

Carnap's example of a correspondence rule has some important differences from Nagel's. Nagel does not present in any formal manner the correspondence rule, but it amounts to saying that there is a connection between an electron jump and a spectral line. Although a

stretched cord and a geometric line have some properties in common, what properties do electron jumps and spectral lines share? A spectral line does not seem to be nor has it been held to be an approximate electron jump. The cord can be considered as an imperfect copy, image, representation of the line. But such talk does not seem to make any sense between electron jumps and spectral lines. Perhaps these two examples of correspondence rules are evidence for Nagel's claim that there is no schema that represents all correspondence rules.

It may be the case that in Nagel's correspondence rule the connection is causal; i.e., electron jumps are at least partial causes of spectral lines, but geometric lines are not held to be causes, in any sense, of stretched cords. But this attempted distinction cannot be maintained. For it may be the case that some cords are stretched to illustrate some properties of geometric lines. So in a psychological sense geometric lines could be causally connected to stretched cords. Continuing in this manner one might justifiably hold that under certain conditions a spectral line represents or is an empirical approximation of an electron jump. So by this line of thought there may indeed be, upon analysis, a single satisfactory schema of all correspondence rules.

I want now to examine in a little more detail the view that correspondence rules are not explicit definitions. To hold that correspondence rules are not explicit definitions is to hold (1) that theoretical terms are not explicitly definable into observational terms, and (2) that theoretical postulates (observationally uninterpreted theoretical statements) are not logically equivalent to any observational statement or set of such statements. If we hold that the
connection between the theoretical and the observable is not one of logical equivalence, then what kind of connection is it? Since we are dealing with linguistic entities, the connection must of course be linguistic. If the connection is not logical equivalence, then perhaps it is one of material implication. Thus, if 'T' is the theoretical term or postulate and 'O' is the observational term of postulate, then 'If T, then O' or 'If O, then T' may be the correspondence rule of its equivalent. Nagel holds that correspondence rules may take on either form; and, in fact, he even holds that there are correspondence rules of the form 'T if and only if O.'

One might wonder about the nature of the equivalence in this rule. It certainly cannot be logical equivalence for then they would be mutually substitutable into any statement or set of statements without changing the truth value of the statement or statement set. This type of equivalence is denied, as we have seen, by both Nagel and Carnap. According to Nagel, such an accepted equivalence rule as the above one states "the necessary and sufficient conditions for describing an experimental situation in theoretical language." What Nagel seems to be saying here is that present in the theory are the necessary and sufficient conditions for the observational term or statement. If necessary and sufficient conditions are logical conditions (what else could they be?), then it follows that 'T' and 'O' are in some sense equivalent, because equivalence is the having of necessary and sufficient conditions.

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19 Ibid., p. 100.
Could '0' and 'T' be materially equivalent, i.e., as a matter of fact, have the same truth value, but not necessarily have the same meaning (connotation)? This is probably more what Nagel has in mind. He believes the correspondence rule which "coordinates the theoretical notion of an electron jump with the occurrence of a spectral line"\textsuperscript{20} is such a rule. 'Electron jump' does not mean, according to Nagel, the same thing as 'spectral line' but when the one occurs, so does the other. If only spectral lines are observable, one wonders how it is known that that coordination is correct? This question stated more generally is how do we justify the correspondence rules of a theory since only a part of the rules, regardless of their form, deal with what is observable? Since correspondence rules are parts of a theory, then their justification is involved in the justification of the theory. This question will be left unanswered until we examine the question of justifying theories.

As to correspondence rules of the form 'If T, then 0' and 'If 0, then T,' one may ask about the kind of implication they possess. Let us first consider entailment. Could it be possible that 'T' entails 'O' (or, conversely, 'O' entails 'T')? If either entailment holds, then correspondence rules become unnecessary since the linguistic connection between 'T' and 'O' follows merely on rules of logic and the meanings of 'T' and 'O' alone. Hence the implication cannot be entailment if one wants to maintain a logical distinction between theoretical terms and statements and observational terms and statements.

\textsuperscript{20}Ibid., p. 101.
Could the implication be material? If the connection is material, then 'If T, then O' ('T → O') would be equivalent to 'It is not the case, as a matter of fact, that T is true and O is false.' And similarly, 'If O, then T' ('O → T') would be equivalent to 'It is not the case, as a matter of fact, that O is true and T is false.' Now if 'O' is the antecedent, then we will have sufficient empirical evidence for 'T,' if 'O' and 'O → T' are true. But if 'T' is the antecedent, then all we will have is partial though necessary empirical evidence for 'T,' if 'O' and 'T → O' are true. In the latter case the truth of 'T' (provided that truth of 'O' is our only evidence) will always be uncertain.

Nagel presents examples for both kinds of correspondence rules. For 'If O, then T,' he says that:

this seems to be the form of the rule implicit in applying the theoretical notion of 'plane' to an actual surface that conforms to an experimental specification of what it is to be a plane.\textsuperscript{21}

And for 'If T, then O' he gives as an example the rule that:

under the experimental conditions obtaining in a Wilson cloud chamber, the condensation of water vapor in fine lines appears to be a necessary condition for describing this effect in terms of the theoretical notion of the passage of alpha particles.\textsuperscript{22}

It should be noted that the latter type of correspondence rule is weaker than the other one because it is always possible for a necessary condition to occur without that for which it is necessary to occur. Thus, the truth of 'O' does not guarantee the truth of 'T' in such rules.

\textsuperscript{21}Ibid., p. 101.

\textsuperscript{22}Ibid.
Nagel makes the claim that there are some theoretical notions which are not linked with observational ones. Thus such notions have no correspondence rules and are empirically meaningless. As examples he notes that (1) "there is no such rule for the notion of electrons moving with accelerated velocities on an orbit" and (2) "there is no correspondence rule for the theoretical notion of the instantaneous velocity of single molecules..." But it may be the case that with new developments in experimental techniques some theoretical notions will become empirically meaningful due to correspondence rules formulated on the basis of the new techniques. Nagel cites as an example the experimental determination of Avogadro's number. It may also be the case, as an instrumentalist would point out, that theoretical notions which have no correspondence rules are merely symbolic heuristic aids enabling the scientist to apply the theory as an explanatory and predictive device for phenomena or observable entities.

Section IV

Achinstein's Critique of Correspondence Rules

In any case Nagel's view that there are some theoretical notions that do not have correspondence rules can be criticized by noting a criticism of Peter Achinstein against the theory of partial interpretation. The thesis of partial interpretation is the thesis held by

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23 Ibid., pp. 101-102
24 Ibid., p. 102.
25 Ibid.
Carnap and Nagel as well as Carl Hempel ("The Theoretician's Dilemma") and Braithwaite (Scientific Explanation, ch. 3). According to Achinstein the thesis of partial interpretation is the thesis that the observational vocabulary of a scientific theory is explicitly defined, according to semantic rules, in terms of some empirical interpretation. But the theoretical vocabulary is not so explicitly defined in terms of the empirical, but gains

an 'indirect' and 'partial' empirical meaning in virtue of the fact that by means of certain postulates of the theory they are related to sets of observational terms. For example, it is in virtue of a postulate which connects a sentence, containing the theoretical term 'electron (jump)' to a sentence containing the observational term 'spectral line' that the former theoretical term gains empirical meaning within the Bohr theory of the atom.  

By 'postulate' in his article, Achinstein seems to mean what I have been calling a correspondence rule. One of Achinstein's criticisms of the thesis of partial interpretation is that every theoretical term has a correspondence rule. Take any theoretical term you please and call it 'T₁,' and take any observational term you please and call it 'O₁.' Now let us assume that 'T₁' occurs in sentence 'Sₜ,' in theory t and 'O₁' occurs in sentence 'S₀.' But 'Sₜ' entails the sentence 'S₀ ⊃ Sₜ,' which has the form of a correspondence rule (Nagel's 'If S₀, then Sₜ' when interpreted as a material implication). Thus the theory contains (in the sense of 'entails') the correspondence rules for all its theoretical terms and every theoretical term has empirical meaning.  

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27 Ibid., pp. 91-92.
Since there are a large number of true observational statements, there will be quite a large number of correspondence rules for every theoretical term. Thus we can truly say with Nagel that "there are . . . an unlimited number of experimental concepts to which . . . a theoretical notion may be made to correspond." The problem arises as to which rule we shall use. But I shall wait in considering that problem.

Achinstein's criticism should be carefully considered. It amounts to saying that any true observational statement is a sufficient condition for any theoretical statement in any assumed theory. Achinstein seems to have definitely shown Nagel to be incorrect when Nagel states that some theoretical terms do not have correspondence rules. (Unless Nagel denies, which he does not, that the implication is material.) But does this consequence entail any grave problems for Nagel and the others who hold the traditional view? One might be tempted to say that the criticism entails that any true observation statement justifies any and all theories, scientific and non-scientific, such as metaphysical ones. According to the traditional view, in testing a theory we must have correspondence rules and if the correspondence rule connects the theoretical part of the theory with an experimentally determined-true observation statement, then the theory is justified. But this is the case with any theory. Consequently, all theories are justified.

There are two ways of avoiding that conclusion: (1) modify the traditional view or (2) hold that correspondence rules cannot have the form "OT." Let us consider the second alternative. This leaves us with correspondence rules of the form "OHT" and "T0" ("O if and only

if T' and 'If T then O,' respectively). Are there any problems here? Since '0®T' is logically equivalent to "'O®T' and 'T®O,'" then the problem centers around 'T®O' because 'O®T' has been shown to be useless.

Given any true observation statement '0₁,' 'T₁®0₁' deductively follows where 'T₁' is a theoretical statement occurring in any theory t. Thus, if we limit ourselves to correspondence rules of the form 'T®O,' then 'T₁®0₁' could be such a rule. Furthermore, in testing a theory we would be limited to such statements in which the observable provides only a necessary condition for the truth of the theoretical. Thus, if '0₁' is true, and 'T₁®0₁' is the correspondence rule, then 'T₁' could only be inferred inductively. (The attempted deductive inference constitutes the fallacy of affirming the consequent.) However, if '0₁' is false and 'T₁®0₁' is the correspondence rule, the falsity of 'T₁' can be inferred deductively by modus tollens.

But as noticed above, from any true observation statement we may deduce a correspondence rule for any theoretical assertion. Thus we would have inductive evidence for any theory whatsoever. Does this mean that all theories would be justifiable? It may be the case that 'T₁®0₁' was not a correspondence rule of the original theory t. And since correspondence rules are, according to the traditional view, constituents of theories and not found apart from them, then 'T₁®0₁' need not be accepted as a correspondence rule for t if it was not in t originally. It may be the case that 'T₁®0₁' was one of t's original correspondence rules. Since '0₁' is true then it deductively follows that 'T₁' and, therefore, t are false. But what if there is no
correspondence rule for 'O' or '-O' in t? Could we not incorporate either \( T_1 \supset O_1 \) (or \( T_1 \supseteq O_1 \), but not both) without making t false? Assuming that t is compatible with \( O_1 \), this procedure, if permitted, would entail that only two kinds of observation statements are possible for a theory: (1) those which provide inductive evidence for it and (2) those which provide deductive evidence against it. Consequently, if a theory has not been refuted by any verified observation statement, then all the verified observation statements corroborate the theory. A completely established theory would have no correspondence rules connecting its theoretical statements with false observational statements.

Are there any grave problems in holding that every observation statement either provides inductive evidence for (confirms) or refutes every theory? Let us assume, for the moment, that t's original correspondence rules do not connect any theoretical statements with false observational statements. Furthermore, no theoretical statement in t is connected with the true observational statement 'O_1.' Furthermore, let us assume that 'T_1' is a theoretical statement in t. Now 'O_1' entails \( T_1 \supset O_1 \) and '-O_1' entails \( T_1 \supseteq -O_1 \). If we accept \( T_1 \supseteq -O_1 \) as a new correspondence rule for t, t is soundly refuted since 'O_1' is true. But if we accept \( T_1 \supseteq O_1 \) as the new correspondence rule for t, then t is confirmed. Which correspondence rule shall we accept? We cannot accept both because t would, from their mere conjunction, be false. (If rules \( T_1 \supset O_1 \) and \( T_1 \supseteq -O_1 \) were in t, they would be equivalent to \( T_1 \supset (O_1 \cdot -O_1) \).) But in order for t to be true both 'T_1' and \( T_1 \supseteq (O_1 \cdot -O_1) \) have to be true, since t is a conjunction of at least them, which is
impossible.) There does not appear to be any purely logical reason for accepting the one over the other. If we want to "save" the theory we should, of course, choose \( T_1 \). Yet this procedure of "saving" a theory seems to be a permissive ad hoc procedure. As long as a theory does not have originally in it correspondence rules for all possible observation statements and the theory has not yet been refuted in accordance with one of its original correspondence rules (the modus tollens technique), then any true uncoordinated observational statement is capable of either confirming the theory or falsifying it. It would seem, then, that we should not permit the addition of new correspondence rules to a theory. If we want to replace old correspondence rules or merely add others, then it would be best to formulate a new theory.

In conclusion, concerning rules of form \( T_0 \), no major logical problems seem to arise as long as we do not permit the addition of new correspondence rules to the theory. Both Nagel and Carnap, however, hold that we are free to add new correspondence rules to a theory as long as the rules are consistent with the original ones.\(^{29}\) The main reason for their wanting to add new correspondence rules is to enable new experimental findings and techniques to be incorporated into well established theories. But, as we have seen, if any of our correspondence rules are of the form \( T_0 \), the theory may be either confirmed or falsified.

One further comment: even if we do not permit the introduction of new correspondence rules into an established theory, a new theory

\(^{29}\)Nagel, \textit{op. cit.}, p. 102 and Carnap, \textit{op. cit.}, p. 238.
having all the established theory's rules in addition to other consistent correspondence rules is easily constructed, and may in the end produce the same result as incorporating new rules in an old theory. So perhaps one cannot very easily avoid the problem. One could set up as a rule of theory-formation that one cannot incorporate into a theory any correspondence rule which is known to provide a refutation of the theory (i.e., for example, if 'O_3' is known to be false and the introduced rule is 'T_1 \supset O_3'). One might note that this rule of theory formation provides in the theory's pretesting some ad huc-ness, i.e., the theory is set up in such a way that no known observation statements falsify it. But, then, who would want to seriously formulate a theory which was known to conflict with some observations?

Thus no serious problems seem to arise when we accept 'T \supset O' as the form of our correspondence rules and permit new rules to be introduced provided 'O' is known to be true. Of course if one considers inductive inferences to be problematic, then such rules are also problematic since they only provide inductive justification of a theory.

Is it possible that the connective of the correspondence rule be something besides material implication? Since material implication and inclusive disjunction are equivalent, some remaining possible logical connections are conjunction, entailment, and logical equivalence. In considering first conjunction, are there any problems with a correspondence rule of the form 'T \cdot O'? Since 'T \cdot O' is logically equivalent to 'T \cdot (T \supset O)' and 'T' is a part of the theory and 'T \supset O' a justifiable correspondence rule, 'T \cdot O' is just another way of expressing the theory-formation technique of the preceding examination. Consequently, 'T \cdot O'
is acceptable because it is logically equivalent to theory structure already acceptable.

But what about entailment and logical equivalence (here symbolized as, '⇒' and '⇔'). Entailment may be considered as the tautological material implication and logical equivalence, as the tautological material equivalence. So we are actually considering special cases of material equivalence and implication. Yet logical equivalence seems to be inappropriate as the form for a correspondence rule if we want to maintain the view that theoretical terms are not explicitly definable in terms of observational terms. Entailment likewise seems inappropriate since it would involve denying the logical distinctness between theoretical terms and statements and observational terms and statements. The reason why these two connectives seem inappropriate is that they break down the distinctions between the theoretical and observable held by the traditional view. Furthermore, the adoption of logical equivalence seems to make the case for descriptivism too easy—at least for that form of descriptivism which holds that theoretical terms are definable or logically reducible in meaning to observational terms.

Besides breaking down the theoretical-observable distinction, explicit definition of (high-level) theoretical terms is not permitted by the holders of the traditional view for another reason. As noted several times earlier, one of the functions of a theory is to explain. Of course it explains why or how certain phenomena occur in conjunction with other phenomena, e.g., the heating of a gas in a closed container and the increasing of the gas pressure in the container. So explanation is conceived by the holders of the traditional view as constructing
a theory that will explain a wide variety of phenomena. Such a function is to unify divergent phenomena under a few sets of theoretical postulates. This function can only be achieved if the theory:

is so formulated that no reference is made in it to any set of specialized experimental concepts. For otherwise the theory would be limited in its application to situations to which just these concepts are relevant.30

Nagel is actually talking about the theoretical postulates of the theory rather than the theory itself. As the postulates are set up in a theory to explain a greater variety of phenomena, they become more abstract from a specific subject matter. As a crude example, let us suppose we want to know why a certain man, Socrates, is bald. We can explain this, at least partially, by saying that men and all other mammals inheriting certain genes become bald. Here we have moved from the specific Socrates, a single mammal, to mammals in general in our attempt at an explanation. The abstract postulates become interpreted postulates when rules of interpretation are applied to them. Then the interpreted postulates are connected to observation statements by means of correspondence rules. A consideration of rules of interpretation brings up the third structural part of a theory (the parts are: (1) abstract postulates, (2) correspondence rules, and (3) the model or rules of interpretation for the abstract postulates).

30Ibid., p. 104.
Section V

Nagel's Views on Models

As was mentioned previously the abstract postulates of a theory are statement-forms rather than statements—which alone have truth-value. By a model of a theory Nagel understands an interpretation of the abstract postulates such that they become statements upon interpretation. As he puts it more formally:

Let $P$ be a set of postulates; let $P^*$ be a set of statements obtained by substituting for each predicate variable in $P$ some predicate that is significant for a given class of elements $K$; and finally, let $P^*$ consist only of true statements about the elements in $K$. By a model for $P$ we understand the statements $P^*$, or alternately the system of elements $K$ characterized by the properties and relations that are designated by the predicates of $P^*$.31

To have a model of a theory we need some modes of connection between the postulates and their interpreted statements. These modes of connection are the rules of interpretation. The rules of interpretation must be formulated in accordance with what counts as being a true statement for the elements of a class. Since the postulates are to be interpreted into true statements, the question arises as to whether the statements are logically or contingently true. Thus we ask whether the statements are definitions stating some of the conditions or properties anything must meet or have in order to be an element of the class in question, or whether their truth is to be ascertained in some other manner, e.g., investigating empirically members of the class in question and comparing the results of the investigation either with

31Ibid., p. 96.
the statements directly or with deductive consequences of the statements (alone or with empirically determined auxiliary statements enabling a deduction) derived from the abstract postulates. If one of the postulates has an existential quantifier, then not all the interpreted postulates are definitions. As for the remainder, the universally quantified interpreted hypothetical postulates, they could be either definitions or contingent assertions. The question is: What, if any, difference does it make if those universal statements are definitions or are contingent?

First of all, if the universal statements are definitions, then we do not have any connection between an entity of one kind and an entity of another kind. According to Brodbeck, universal statements that connect different entities or facts together are laws. Furthermore, laws unlike definitions (which are universal assertions of logical equivalence between one linguistic entity and another) have significance in the sense that the connection between one entity or fact and another can be used either for an explanation or a prediction. For instance, "If the temperature of a gas increases as its pressure remains constant, then its volume increases," can be used to explain why the gas volume increases or, under those antecedent conditions, to predict its volume increase. Talking about a law (of universal form), Hempel holds that:

it is a statement to the effect that whenever and wherever conditions of a specified kind F occur, then so will, always

and without exception, certain conditions of another kind G.\textsuperscript{33}

On the other hand, if the universal statements are definitions then they would lack both explanatory and predictive significance.

But if the model of the theory, according to Nagel, consists only of true statements about the elements in a certain class, can these statements be laws in Brodbeck's and Hempel's sense? In Hempel's definition it should be noted that 'F' and 'G' refer to different classes. However, in Nagel's explication only one class, K, is mentioned. But it is quite possible that some of the true statements about the elements of K (F) are statements that all K (F) are G. Consequently, it seems to make a difference whether the interpreted postulates (the model) are definitions or universally contingent laws. And, secondly, Nagel's view of a model leaves completely open the possibility of some of the interpreted postulates being universal laws rather than definitions (especially those which mention another class or have the form '(x) (Fx\supset Gx).'

Although what Nagel means by a model seems clear enough, other comments he makes about models raise some problems. In outlining the postulates of the Bohr theory of the atom Nagel comments:

It assumes that there are atoms, each of which is composed of a relatively heavy nucleus carrying a positive electric charge and a number of negatively charged electrons with smaller mass moving in approximately elliptic orbits with the nucleus at one of the foci.\textsuperscript{34}


\textsuperscript{34}Nagel, p. 94.
Although Nagel continues discussing other postulates, they are not relevant at this point. What is relevant to the issue under discussion is noting that Nagel says, after presenting in outline the postulates, that the Bohr theory is usually presented in statements rather than statement-forms (or the abstract set of postulates of a calculus). What Nagel presented above is, according to Achinstein, the Bohr model rather than the Bohr theory. In justifying his claim about Nagel, Achinstein states that Nagel tells us that the Bohr theory is only a partially interpreted calculus whereas the quote above from Nagel presents statements rather than statement-forms.

However, Nagel does not explicitly tell us he has actually presented a model of the theory rather than the theory itself. In fact what Nagel says is that he has presented the Bohr theory by means of a model. Furthermore, it should be remembered what are, according to the traditional view, the three structural parts of a theory: (1) an abstract calculus, (2) set of correspondence rules, and (3) an interpretation or model for the abstract calculus. Hence it would seem that when one is presenting a theory one must be presenting (among other things) a model. It is possible to distinguish between an interpreted theory (one having a model) and an uninterpreted theory (one that does not have a model, or it may have a model but the model is not being considered). Perhaps Achinstein has this distinction in mind.

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36 Ibid.
37 Nagel, op. cit., p. 95.
But to further complicate the issue, in the later discussion on analogy, Nagel states that "a model for a theory is not the theory itself" and there may be several models for the same theory. Also he holds that:

some inessential feature of a model . . . may be mistakenly assumed to constitute an indispensable feature of the theory embedded in it; and the model may be confused with the theory itself.

Since these quotes of Nagel are from his section on analogy rather than on structure of theories, it is best to consider first what he has to say about analogy.

Nagel divides analogies into two types: substantive and formal. A substantive analogy is constructed on the basis of similarity of entities and their properties. From an already existing theory about entities and their properties which are expressed in the laws of the theory, another theory is modeled which has similar entities, similar properties, and, therefore, similar laws. For example, the molecules, their properties, and the laws of the kinetic theory of gases are modeled on macroscopic spheres, their properties and their laws of motion. The analogy here is based on the similarity of entities in the different theories. Thus, on the basis of one theory another theory may be constructed which may prove to be very useful. In so far as the first theory is familiar to us, the construction of another theory by

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38 Ibid. p. 116.
39 Ibid.
40 Ibid., p. 115.
41 Ibid., pp. 110-111.
means of it to explain what has yet not been understood is an example of understanding the unfamiliar in terms of the familiar.

A formal analogy is constructed on the basis of similarity of structure of relationships that the entities and/or properties have to each other. From an already existing theory about entities and their properties which are expressed in the laws of the theory, another theory is modeled that may have different entities, different properties, but laws whose formal structure is similar to the formal structure of the original laws. As an example one can cite "Maxwell's example of the identity in structure of the mathematics of gravitation theory and the equations of heat conduction. . . ." Thus one can construct a new theory by using the mathematical formalism of another theory as a model.

It should be made clear in these two kinds of analogies as to what is the model. In both kinds the original theory is the model for the constructed theory. Is Nagel here using 'model' differently from his use of it when presenting the different parts of a theory? There seems to be two different uses of 'model.' When presenting the different structural parts of a theory, a model is held by Nagel to be an interpretation of an abstract calculus. But in the analogy presentation, Nagel claims that a model is a theory from which another theory is developed either substantially or formally. A question at this point concerns these two uses of 'model.' (Let us call the first use 'model₁' and the second 'model₂'.

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42 Ibid., p. 111.
In classifying his views on models, Nagel uses as examples the kinetic theory of gases, the wave theory of light, and the Bohr theory of the atom. In these examples he makes a distinction between the usual interpretation of the postulates of the theory (model_1) and the way in which the model suggests how the theory (its theoretical expressions) can be connected to experimentally observed data. It might seem that the model which suggests how to apply and extend the theory to phenomena is model_2. Yet model_1 can also suggest how to apply and extend its theory (the abstract calculus) to phenomena because of the theory's correspondence rules. What then does model_2 do that model_1 cannot do? Let us consider carefully the relationships between model_1 and model_2. Consider two theories A and B, and let us say that B is modeled on the basis of A. Now for both A and B, abstract calculi and models_1 interpreting their calculi have been constructed. Still one might say that model_1 of B was derived by analogy (substantially or formally) from model_1 of A. But where is model_2? Is A model_2? Since B is derived either substantially or formally from A, it would seem that A is model_2. And similarly, model_1 of A is model_2 of model_1 of B. Model_2 is merely a model of analogy whereas model_1 is a model of interpretation. Model_2 (namely A in our case) can suggest how to connect some of B's postulates to phenomena since model_1 of B is an analogue of model_1 of A. Hence the above function is what model_2 can do which model_1 does not do.

But we still have another problem. Nagel was quoted earlier as saying that models should not be confused with their theories and that

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^43 Ibid., pp. 113-114.
such confusion has led to some problems. As an example he cites using the classical particle as a model for the quantum particle.\textsuperscript{44} One can ascribe simultaneously a determinate position and velocity to a classical particle at any given time, but such ascription cannot be made at any given time for the quantum particle. The classical particle would be a substantive analogical model for the quantum particle. But the analogy has its limitations. Thus, for example, classical mechanics would be theory A and quantum mechanics would be theory B. Now the ascription of position and velocity to a classical particle would be part of model\textsubscript{1} of A and the ascription of position and velocity to a quantum particle would be part of model\textsubscript{1} of B. B's model\textsubscript{1} is derived from A's model\textsubscript{1} by using A as model\textsubscript{2}. But using model\textsubscript{1} of B derived from A's model\textsubscript{1} yields problems for theory B. Hence the analogy (A as model\textsubscript{2}) has its limitations for B. Thus when Nagel says that a model for a theory is not the theory itself, he can be understood as saying that theory A is not theory B (which is quite acceptable) and that model\textsubscript{1}, because it is one part of a whole (the theory), is not to be confused with the theory (its whole). It also seems to be the case that one cannot know beforehand whether a given model of a theory (in either sense) will help or hinder the development of the theory.

The preceding discussion was only concerned with one model for one theory. But Nagel notes that an individual theory can have several models. Now in what sense of 'model' is this claim correct? For any uninterpreted calculus a large number of interpretations may be given.

\textsuperscript{44}Ibid., p. 115.
As a simple example, \((x)(Fx \supset Gx)\) has as among its "realizations" or "models": 'All diamonds are soft,' 'All men are mortal,' 'All dogs are fat.' Since an uninterpreted theory can have many different interpretations, it is possible for the same theory (calculus) to have different models \(\text{model}_1\). Is it also possible for the same theory to have different model_2? It seems logically possible to construct a theory on the basis of analogy from at least two other theories. Following examples of Achinstein, one can claim that with regard to deriving laws about gases we use "the billiard ball model for deriving the gas law and the weakly-attracting rigid-sphere model for deriving the Van der Waal's equation."^45

Let us consider this example somewhat more formally. Let A represent the billiard ball model, B, the rigid-sphere model, and C, the gas theory. Now both A and B are models_2 of C. This means that there is some similarity between model_1 of A and model_1 of C, and some similarity between model_1 of B and model_1 of C. There need not be any similarity between the models_1 of A and B. Models_1 of A and C could be similar due to certain respects and models_1 of B and C could be similar due to certain other respects.

Section VI

Achinstein's Critique of the Traditional View of Models

Achinstein has criticized the traditional view concerning its distinction between theory, model, and analogy. By appealing to the

way 'theory,' 'model,' and 'analogy,' are used by scientists, Achinstein presents his criticisms. First of all 'a model of an x,' where x refers to a phenomenon, is frequently used by the scientist, according to Achinstein, to refer to a theory about x. In this sense 'model' and 'theory' are identical. By 'a theory of x' is meant "a set of assumptions or postulates describing physical objects, or phenomena, of type x."46 It should be noted that this sense of 'model' is compatible with Nagel's model. After all an interpreted calculus would (if empirical) be describing some kind of phenomena.

After citing different writings, Achinstein analyzes 'an analogy for x' as a comparison of phenomenon x with phenomena or objects of another sort.47 The comparison is, of course, based on some similarities between x and let us say, y. However, x and y are not considered to be identical (or else y would be a model for x). Thus when the scientist compares the molecules of a gas in a container to moving billiard balls in a box, he is not maintaining that molecules are billiard balls, though they do have some characteristics in common. Furthermore, it should be noted that some analogies of x compare x with a y that is imaginary. Maxwell, for example, compared the concept of the potential of an electric field at a given point to the pressure of an imaginary fluid at a given point.48 And Achinstein makes the point that from the history of science sometimes "what begins as an analogy

48 Ibid., p. 333.
(e.g., between optical and electromagnetic phenomena) develops into a
model as the two objects or phenomena are identified.” 49 It is, more-
over, sometimes the case that it is not clear whether a scientist is
presenting a model or a theory.

In his criticisms, Achinstein cites mostly Nagel’s and
Braithwaite’s views on models and analogies. Although both
Braithwaite and Nagel have been mentioned as holding the traditional
view of theories (especially with regard to function), they seem to
differ with their views on models. According to Braithwaite:

a model for a theory T is another theory M which corresponds
to the theory T in respect of deductive structure . . . a
model is another interpretation of the theory’s calculus.50

Unlike Nagel, who holds that the interpretation of a set of postulates
must be true statements of members of a specific class (in short, the
model must be true), Braithwaite continues by arguing that the model
need not be true or even believed to be true. Again like Maxwell and
Achinstein, he has in mind the imaginary models scientists sometimes
use.

Now in which sense of Nagel’s 'model' is Braithwaite's? First of
all, Braithwaite holds a model to be a theory and the reason why a
model is a model of a theory is that the theory and its model have the
same logical form (deductive structure, logical skeleton, etc.). This
concept of model is not model as I interpret Nagel. At most it would

49 Ibid., p. 334.

50 R. B. Braithwaite, "Models in the Empirical Sciences," Logic,
Methodology, and Philosophy of Science, ed. by Ernest Nagel, Patrick
Suppes, and Alfred Tarski (Stanford: Stanford University Press,
be model₂ of the formal type. The formal model₂ is established by similarity in the logical structure of two theories. Thus, Nagel and Braithwaite could agree in that M and T are theories, but Nagel need not hold that the logical structure of M and T are identical for M to be a model of T—merely similar in certain respects (after all, it is an analogy). Because Nagel's concept 'formal model₂' is not necessarily the same as Braithwaite's, I shall call Braithwaite's 'model₃.'

Brodbeck also seems to accept 'model₃' as one of many uses of the term 'model' when she states that theories whose laws have the same form can be considered as models of each other.⁵¹ One could consider model₃ as a special case of model₂: the case where the formal analogy becomes a formal identity. But for clarity it will be best to distinguish them.

Braithwaite's claim that "a model is another interpretation for a theory's calculus" seems to indicate that a model is not just an interpretation for an abstract calculus (model₁). It is an interpretation for an abstract calculus that has already been interpreted (the theory proper). Hence it seems that Braithwaite does not make use of the 'model₁' model.

Braithwaite, moreover, makes an epistemological distinction between a theory and a model.⁵² Logically, the theory and model have the same form: their axioms and theorems have the same or equivalent logical forms. So formally the model and theory are indistinguishable.


⁵²Braithwaite, Scientific Explanation, pp. 88-90.
But the epistemological difference between them is that, for the theory, theorems are interpreted first and the interpretation of the axioms or postulates is determined by interpretation of their theorems. On the other hand, in a model the axioms are interpreted first and interpretation of the theorems is determined by their axioms. Braithwaite's distinction becomes significant when one remembers that the interpreted axioms of a theory contain, according to the traditional view, theoretical concepts. The theorems of the theory are linked by explicit and operational definitions to observational statements whose truth can be ascertained empirically. In a theory theoretical terms are partially interpreted or coordinated with observational terms. Such an interpretation will guarantee the factual import of the theoretical terms. This procedure becomes important for the traditional view since it holds to the theoretical-observational distinction. The partial interpretation of the theoretical terms by means of observational terms will be accomplished by correspondence rules. With Braithwaite's distinction one can thus see the intimate relationship between correspondence rules of a theory and the interpretation of the theory's calculus.

But why does the interpretation of a theory's model proceed from axioms to theorems? The reasons seem to be that (1) models are aids in understanding more fully a theory, and (2) that the interpretation given to the calculus to produce a model is totally empirical. For example, billiard balls moving in a closed container serve as a model for molecules of a gas moving in a closed container. Hence a model's non-logical concepts are all observational or based on another model
whose non-logical concepts are all observational.\(^{53}\)

Since there are differences between Nagel and Braithwaite as to what a model is, it may be best to hold that the traditional view has no specific concept of model. Achinstein, however, seems to believe that the traditional view does have a specific concept. For one thing he agrees that, in so far as Nagel and Braithwaite distinguish a model from a theory (remember Achinstein does not), then they hold that:

(i) Models utilized by scientists consist of propositions describing the properties of certain entities which, in general, are not to be conceived of as identical with those entities described in the corresponding theory.\(^{54}\)

Yet if one is talking about model\(_1\), then Achinstein's claim for the traditional view is false. Model\(_1\) in fact describes the entities of its theory. Yet Achinstein's claim for the traditional view is correct if one is considering model\(_2\) or model\(_3\) since these may involve different subject matters (like the billiard ball theory being a model for the molecular theory). Thus it seems that Achinstein's comment has only affected Braithwaite's view. But I believe that I have found and justified two different senses of 'model' in Nagel's treatment of models and analogies. It is true that Nagel uses 'model' to refer to both model\(_1\) and model\(_2\), thus perhaps inviting a blurring of the distinction between the two. Furthermore, that model of Nagel's which I call 'model\(_1\)' seems to be exactly what Achinstein means by 'a model of an x,' since his examples are interpreted statement-forms (statements)


about phenomena. And hence that model of Nagel's which I call 'model₂' seems to be what Achinstein means by 'an analogy for an \( x \),' since the examples Achinstein uses for analogies and those Nagel uses for models₂ are the same.\(^5\)

Achinstein continues by saying that (i), above, must be modified to consider analogies that scientists use which describe subject matter different from the theories to which they are compared. But Achinstein's proposal simply recommends recognizing what Nagel calls substantival analogies (a type of model₂).

A second tenet of the traditional view of models is, according to Achinstein, that the model and its theory have the same calculus. As noticed above, both Brodbeck and Braithwaite hold this tenet. But it should be remembered that the model advocated by Braithwaite and Brodbeck is model₃ (the special case of model₂). This tenet is trivially true of model₁ which is identical to the interpretation of the calculus of the theory and will thus have the theory's calculus. But is it true of models₂ in general? In discussing model₂ Nagel only mentions that models₂ serve for the construction of theories either according to subject matter or form. He never says that they have the same calculus, though some (but not all) of his examples of models₂ and theories do have the same calculus. Consequently Nagel is quite aware of model₃ as a special case of model₂.

Achinstein's criticism of the second tenet is that, in drawing an analogy for a theory, the scientist does not suppose everything true.

\(^{5\text{Achinstein, Ibid., pp. 332-333; Nagel, op. cit., pp. 108-111.}}\)
in the analogy to be a corresponding truth in the theory.\footnote{Ibid., p. 336.} Thus, for example, even though billiard balls have some color or other, molecules are not held to have any color. In most analogies only certain properties or logical structures are comparable to their theories. Thus Achinstein believes that the second tenet is too rigid for the analogies scientists use most frequently. It should be noted that both Nagel and Braithwaite are aware of this problem: both agree that certain precautions should be kept in mind when using models. According to Nagel:

\begin{quote}
\par
\text{some inessential feature of a model (especially a substantive model) may be mistakenly assumed to constitute an indispensable feature of the theory embedded in it.} \ldots \footnote{Nagel, \textit{op. cit.}, p. 115.}
\end{quote}

Braithwaite notes that one should not identify a theory with a model lest one attribute to the theoretical entities properties belonging only to the entities of the model. When one uses models to understand or think about scientific theories one is always engaged in \textit{as-if} thinking: \textquote{hydrogen atoms behave (in certain respects) as if they were solar systems.} \ldots \footnote{Braithwaite, \textit{Scientific Explanation}, p. 93.}

It should be noted that Nagel, Braithwaite, and Achinstein are all talking about the dangers of identifying a model with its theory. Achinstein argues that, because of the above examples which warn against identifying models with their theories, it is not the case that for each concept in the theory there is a counterpart in its analogy and it is not the case that all of the laws of the theory have the same
formal calculus as the laws of its analogy. Achinstein concludes that the second tenet should be revised accordingly. Achinstein's argument seems to have merit if we are using one established theory to be the model of another established theory. On the basis of some similarity either in entities or formal structure the analogy or model is formulated. However, what if one thought up an imaginary model which was a theory about imaginary but intelligible entities and yet its calculus was identical to the theory for which we are constructing the model? We would still have an analogical model because the model has imaginary entities whereas its theory presumably has actual entities. The only significant warning one could give in the use of imaginary analogues is not to identify the imaginary with the actual. Hence if we use imaginary models, we will not have to modify the second tenet (which is what Braithwaite does\(^{59}\)).

Another point with regard to the second tenet should be brought out. If there is a model\(_3\) for a theory, then there will be a one-to-one correspondence between the descriptive terms designating entities of model\(_3\) and the theory, between the axioms of model\(_3\) and the theory, and between the theorems of model\(_3\) and the theory. On the basis of the one-to-one correspondence of the descriptive terms, translation or transformation rules can be formulated which will enable one to make inferences from the theory to the model and conversely. If one incorporates the translation rules either into the theory or into the model then, respectively, the model or the theory as a separate formulated system becomes unnecessary, since it will be logically contained in

the other along with the correspondence rules. Thus if for some reason or other the model is more intelligible (observational, mechanistic), one can dispense with the theory provided one has translation rules that will enable predictions to be made from the model that could have been made only from the theory. This procedure appears to be a way of eliminating the need for theories and theoretical concepts from science and thus of using only imaginary constructs—a procedure in the spirit of descriptivism (as reductionism) and instrumentalism. Yet 'appears' is important in the preceding sentence. It might be argued that one can set up a model for a theory and translation rules only if one has already formulated the theory. And once the theory has been formulated, what else is needed for intelligibility? Furthermore, what is there for the model?

Putting aside the fact that some people have certain preferences, like Kelvin, for mechanical explanations and models, it seems that the use of model becomes helpful when one has a theory that is not yet completely formalized and axiomatized. Since one is not clear as to the theory's range of explanation and prediction, a model is established on the basis of more familiar entities and laws. It will probably be best for the model to have the same calculus as the theory (in so far as the theory has been developed). But since the model deals with familiar entities and has the same calculus as the theory so far established, some translation rules can also be formulated and theorems from the model's assumptions can be deduced. These assumptions together with the established translation rules will enable one to formulate other translation rules enabling one to build up or add
to the structure and content of the theory.

Our discussion leads up to what Achinstein calls the third tenet of the traditional view, viz., that all a scientist has to do to discover an analogy for a theory is to construct a model having the same calculus as the theory but not being about the same entities. In criticizing this tenet Achinstein notes that some of the scientist's analogies are based on similarity of the entities and their properties rather than similarity of calculus. For instance, the analogies drawn between billiard balls and gas molecules are based on their having similar spatial, dynamic, and mechanical properties. But it should be noted that the distinction Achinstein is making between physical analogies (like the above) and formal analogies has already been formulated by Nagel as that between substantive and formal analogies, respectively. Hence Achinstein's criticism at most affects Braithwaite's treatment of models.

One may, however, wonder whether the distinction between physical or substantive and formal analogies is sound. If it is the case that two or more entities have similar properties, then is it not the case that the statements expressing the fact that each has that certain property have the same form? For example, "Billiard balls are elastic" and "Gas molecules are elastic" are both of the form \( (x)(Fx \equiv Gx) \). So it would seem that formal analogy is a necessary condition for physical or substantive analogy. Yet it does not seem to be the case that formal analogy is a sufficient condition for

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physical analogy. For example, "Gas molecules are elastic" and "Diamonds are hard" have the same form \'(x)(Fx\supset Gx)\', but do not express any physical or substantive analogy that is significant.

The fourth tenet of the traditional view according to Achinstein is that:

(iv) If a set of propositions about a group of familiar and intelligible objects has the same calculus as (a portion of) a certain physical theory, then the former set constitutes an analogy for (a part of) the theory and will furnish an important aid for understanding some of the theoretical concepts involved.61

In examining this tenet, Achinstein presents a calculus and two interpretations of it. The one interpretation is about light rays and their congruency and the second interpretation is about swans and their color equality. By means of this example, Achinstein argues that even though it is possible that our understanding of light rays' congruency may be helped by our understanding of swans' color equality, the analogy (in the sense of 'model') does not help us to relate 'light ray' and 'congruence' to physical phenomena. Achinstein concludes that tenet four is not always true. Just because, so he argues, we have a set of statements about concepts with which we are familiar and these statements have the same logical structure as the statements of a theory, it does not follow that the theory, on that account, will also be intelligible to us.62

Is Achinstein's argument sound? I think that he has been misled by his example into forgetting another important part of the traditional

\[61\text{Ibid., p. 342.}
\[62\text{Ibid., pp. 343-344.}\]
view. What enables us to relate statements and concepts in a theory are the theory's correspondence rules. Now according to the traditional view (as we have observed previously), one of the structural parts of a theory is that set of rules which connect the theoretical statements and concepts of the theory to its observational statements and concepts. But since these correspondence rules are part of the theory, their formal counterpart will also have to occur in the theory's model \(2\) or model \(3\). Thus Achinstein's two theories are misleading examples, since there are in neither theory correspondence rules connecting 'light ray,' 'congruence,' 'swan,' and 'same color' to observational concepts. In short, Achinstein's theories have no empirical content because only correspondence rules give empirical content to theories.

Now in replying to this criticism, it might be urged that statements, such as "At least two light rays are not congruent" and "At least two swans have different colors" do have empirical content since their non-logical concepts are observational. Although it seems correct that the two statements are observational, so too are all the others in his example. Hence when Achinstein interpreted the calculus, he acquired a set of observational statements instead of two theories. To sum up: if the concepts of the two interpretations of the calculus in question are theoretical concepts which have not been provided with correspondence rules connecting them to observational concepts, then the two interpretations are not theories in the traditional sense. On the other hand, if the interpretations of the theoretical concepts are explicitly empirical, then again there are no correspondence rules
because there are no theoretical concepts and, as a result, they are not theories in the traditional view. Thus Achinstein's use of these interpretations in criticizing the traditional view is irrelevant. It would have been better to criticize the tripartite structure of theories. Actually this is what Achinstein does in other articles where he criticizes the theoretical-observational distinction so vital to the traditional view. And because of that distinction correspondence rules are needed.

The fifth and final tenet Achinstein examines (which he believes is held by the traditional view) is that "analogies, construed in the logical sense, provide important guides in the formulation and extension of theories."63 By 'analogies' here Achinstein means 'models.' Achinstein believes that this tenet assumes that if we have evidence for a set of statements, then we have some reason for believing that any theory modeled on the basis of those statements will also be true. Furthermore, since the model usually is based on familiar objects and not all the true statements we know about the objects are set up as part of a model, the addition of some of those true statements to the model which were originally left out will enable us both to make additions to the theory and to deduce new theorems. In these two ways models will be guides in formulating, extending, and confirming theories.

In criticizing the fifth tenet, Achinstein argues that there is no justification for the claim that, if a statement of certain logical

63Ibid., p. 344.
form has either been determined as true or has been confirmed, then we have some reason for holding as true or confirmed any other statement of that form. Because a given sentence-form has many different possible interpretations (assuming that its form is contingent), many of these interpretations will be false and many others true. Achinstein's reasoning seems to be sound here, but the problem is whether Braithwaite who holds to the model conception has to make this assumption to hold tenet five. It should be remembered that Braithwaite, unlike Nagel, holds that:

the initial propositions of the model, which are correlated with the initial hypotheses of the theory, need not be true, or thought to be true; all that is required is that the rest of the model propositions must be logical consequences of the set of them.64

What Braithwaite has in mind are imaginary models. Thus Braithwaite is denying the assumption Achinstein imputes to him. Though Nagel holds that models are to be true interpretations, Achinstein's criticisms are ineffectual against Nagel's view because Achinstein and Nagel are actually in agreement on the status of models and analogies. The problem, then, is deciding who is correct in the above controversy: Braithwaite or Achinstein?

Achinstein argues that a theory, constructed from a model which has only the same calculus as the model, has no degree of plausibility given to it by the model even if the model is true. If, however, the entities of the theory are physically similar to the entities of the analogy then, if the model is true, some degree of plausibility is

conferred on the theory by means of the argument from analogy.\textsuperscript{65} Thus Achinstein holds that true substantive analogies rather than true formal analogies give plausible support to theories.

Since Braithwaite holds that models need not be true and that their truth-value need not even arise, Braithwaite is interpreted by Achinstein as saying that the model does not confer any degree of plausibility on its theory. And if the model does not confer any degree of plausibility on its theory, then the formulation of a theory on the basis of a formal calculus alone is "no better than any entirely arbitrary device for generating theories."\textsuperscript{66}

Braithwaite's reply might be the following: Achinstein's criticism rests on his belief that a substantive model confers plausibility by analogy to its theory. But Braithwaite holds that some analogical suggestions have led to valuable theories, whereas others have not.\textsuperscript{67} Braithwaite gives one the impression that the use of analogical argument is as arbitrary as any other method due to its quasi-success in the history of science. Whether a theory is plausible will have to be ultimately decided by testing the theory itself. All other forms of procedure are no more nor less arbitrary than any other.

Furthermore, Braithwaite would deny holding part of the fifth tenet. Nowhere does Braithwaite say that models are used to formulate or construct theories. According to Braithwaite a model is another

\textsuperscript{65} Achinstein, "Models, Analogies, and Theories," p. 347.

\textsuperscript{66} Ibid., p. 346.

interpretation of a theory's calculus. This seems to imply the
temporal priority of the theory to its model. Hence Achinstein's
criticism of using model₁ to formulate a theory does not affect
Braithwaite. Braithwaite does, however, hold that models may be used
to extend their theories. Braithwaite's attitude toward models is
that they are formulated by certain individuals to have a "picture"--
a more visualizable or familiar grasp--of the theory in question.
Since the model is based on objects and properties familiar to us,
some of their relationships not formulated in the model may later
suggest themselves to us so that they are eventually incorporated into
the model and their analogies into the theory. So in this manner
models help extend their theories. But, as noted before, Braithwaite
warns us that such extensions of theories must be tested by themselves
apart from the model. In conclusion, then, Braithwaite believes (1)
that models are utilized in helping people understand theories by pre­
senting the theory as if it were about familiar and empirical entities
and properties, and (2) that models can suggest, as discussed above,
extensions to theories, but that the plausibility of such extensions
are decided only by testing them--not by argument from analogy.

I believe that Braithwaite is correct in holding that any new
additions to a theory must be tested apart from the model. And I
believe that Achinstein would also agree. Arguing by analogy seems,
Furthermore, to be little more than one thing suggesting another due
to their similarities whether they be substantival or formal. In
summing up this portion of Achinstein's critique of the traditional
view of models, we have found that apart from model₁ to which everyone
holds, there seems to be no traditional view. Moreover, no specific view of model besides model₁ is entailed by the assumptions of the traditional view. As for what I have called model₂, I believe that there is no disagreement between Achinstein and Nagel, and hence that all of Achinstein's criticisms of Nagel on this score are due to misinterpretation. Thus the five tenets and their criticisms do not affect Nagel's view. As for Achinstein's criticisms of Braithwaite's view (that all models and analogies are of model₃-type), the only sound criticism is that scientists use models of the substantival type. But since the substantive analogy presupposes the formal, even this criticism is thereby weakened. The reason for the weakening is Braithwaite's claim that models are formally identical to their theories. Yet it does seem to be the case that most theories and their models are only formally similar (model₂) not formally identical (model₃). Braithwaite could reply that the model proper is the only part which has the same structure as the theory. The dissimilar parts will not be part of the model proper.

Section VII

Spector's Critique of the Traditional View of Models

Another criticism of Braithwaite's view of models has been offered by Marshall Spector.⁶⁸ Spector appeals to the ways physicists use models and believes that sometimes the entities of the models become

identified with the entities of the theory. Such a use of models is interpreted by Spector as falsifying Braithwaite's comment that identifying a model's objects with the theory's objects leads to making incorrect inferences since thinking about theories in terms of models is always as-if thinking.

Spector presents for his argument the kinetic theory of gases (the molecular theory) and its elastic sphere model. According to Spector, scientists found that gases behaved as though "they were in fact composed of small elastic spheres in incessant motion." On the basis of this similarity, he says that we may tentatively identify the elastic spheres with the molecules. And, furthermore, if it is the case that we have exhausted the different testings of the theory on the basis of the model and the tests agreed with the model, then (Spector argues) there would be no further point in saying that gases behave as if they were composed of small elastic spheres—they are composed of them. But, so Spector holds, if Braithwaite is correct, then such an identification would constitute a logical mistake. Hence Braithwaite's concept of model is mistaken.

Since the argument is based on whether or not one may identify a model with its theory, it will be best to examine carefully what Braithwaite says about such an identification. Braithwaite holds that formal similarity is all that is essential for the relationship of model to theory. If further substantive similarities are drawn, then one could easily be misled (and this has happened) into making false

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69 Ibid., p. 133

70 Braithwaite, *Scientific Explanation*, p. 93.
inferences from the theory. Yet we do not know whether substantive similarities hold unless we make such identifications tentatively and then test for the empirical results of such identification. If the results confirm the identification, then there does seem to be evidence that the so-called model and theory not only share the same calculus, but also the same subject matter. But when we reach this point one wonders whether we have a model of the theory.

Two points emerge from this discussion: (1) substantive analogies may be made between model and theory in so far as deductive consequences drawn from these analogies are subject to present empirical tests; (2) if no further tests are possible and the model has been successfully identified with the theory, we no longer have a model—just a theory. Spector is arguing for (1) and Braithwaite would hold (2). Both (1) and (2) seem to be sound points. Consequently what appears to be a model for a theory may in fact turn out to be the theory itself. Now since Braithwaite defines a model as another interpretation of the calculus of a theory, the identification that Spector says might happen between the model and the theory is logically impossible. It is logically possible only if the model is not a model in Braithwaite's sense. Since Braithwaite and Spector are not talking about the same kind of model, Spector's criticisms are not relevant against Braithwaite's view of model. Of course, Spector's use of 'model' is quite legitimate; in fact, it is probably closer to the scientist's use than Braithwaite's, but that difference in use does not constitute an effective criticism. Braithwaite's reply is adequate.

The final criticism of Spector is directed against the partial
interpretation thesis (part of the traditional view of theories) to which Braithwaite holds. Spector believes that when we have a model that does not enable us to deduce laws as accurately as we would like, the model sometimes has suggested how it could be modified so that new and more accurate laws are deducible from it. For example, from the elastic sphere model for gas, one can deduce the law "PV=kT" but it is not accurate enough. Yet upon re-examining the model we note that perhaps we should have considered the radii of the elastic spheres and also their noncontactual forces upon each other. When the model is so modified to incorporate these new considerations, the more accurate van der Waals law is deduced.\textsuperscript{71}

But what bearing does this example have on the partial interpretation thesis? According to the partial interpretation, theoretical terms acquire empirical meaning only (roughly speaking) by being connected with observational terms by correspondence rules. Now if our model is changed, argues Spector, then the theoretical terms become connected with new observational terms by means of new correspondence rules. But since the empirical meaning of theoretical terms is determined completely by the observational terms they are connected to with correspondence rules, then the meanings of these theoretical terms have changed from the elastic sphere model to the van der Waals model (rigid sphere model). Spector believes that if one accepts the partial interpretation thesis, then one must hold that the elastic sphere model is logically distinct from the rigid sphere model in the meaning of

\textsuperscript{71}Spector, \textit{op. cit.}, pp. 135-136.
their terms. Since, moreover, the observation language of the Bohr theory is different from that of the kinetic theory (so too, then, would be their correspondence rules), then Bohr's reasoning, "which depends on the identity of the meanings of these terms in the two theories, would be invalid"72 according to the partial interpretation thesis. And finally, Spector argues that if the partial interpretation thesis is correct, then the reduction of thermodynamics to statistical dynamics would be unsound. 73

Is Spector's argument sound? An advocate of the partial interpretation thesis could reply by saying that the meanings of the theoretical terms have not been changed due to the addition of new observational terms and new correspondence rules; the meanings of the theoretical terms have been extended due to the empirical extension of the model. Spector is misleading when he says that additional correspondence rules changed the meaning of theoretical terms. The old meaning is still present because of the unchanged connection with the older observational terms and older correspondence rules. But to the old meaning is added other empirical meanings. Thus it is more appropriate to say that the theoretical terms of the rigid sphere model have the same meanings as the theoretical terms of the elastic sphere model plus other empirical meanings lacking in the elastic sphere model. Furthermore, the advocate could argue that Bohr could not have developed his theory from the kinetic theory unless his model

72 Ibid., p. 138.
73 Ibid., p. 139.
contained either the entire kinetic model or a part of it including its observational terms and correspondence rules for those theoretical terms appearing in both Bohr's model and the kinetic model. And finally, an advocate could argue that of two theories $T_1$ and $T_2$, the one is reducible to the other if and only if all the correspondence rules and observational terms of the reduced theory are also contained in the reducing theory. In conclusion then, Spector's arguments against the partial interpretation theory are also inadequate.

Spector's argument would not even be applicable if we were considering two theories that contained in common some theoretical terms but had totally different observational terms and correspondence rules. These two theories would not have the same empirical meaning, and Spector's argument is applicable only to theories that do have some empirical meaning in common. But such common meaning can be explained on the basis of having the same observational terms, however anchored to the meaning basis, and the same correspondence rules.

Section VIII

Achinstein's Examination of Theoretical Models

In the preceding discussions we have been considering different views as to the relationships between models and theories and the nature of models. In another article, Achinstein discusses theoretical models. As examples of theoretical models, Achinstein presents:

74 Achinstein, "Theoretical Models," pp. 102-120.
the Bohr model of the atom, the billiard ball model of gases, the corpuscular model of light, the shell model of the atomic nucleus, and the free-electron model of metals. . . . the Crick-Watson model of the DNA molecule . . . and . . . the multiplier-accelerator model of economic growth.\textsuperscript{75}

The important characteristics of theoretical models are, according to Achinstein, five. First, a theoretical model is "a set of assumptions about some object or system."\textsuperscript{76} For instance, among other things, the billiard ball model assumes that gas molecules only exert forces on each other during contact. Secondly, the theoretical model describes the "object or system by attributing to it . . . an inner structure, composition, or mechanism."\textsuperscript{77} This inner structure enables the theoretical model to have explanatory import. For example, the billiard ball model describes gas molecules in such a way that the formula "PV=kT" is derived from it. This second characteristic is understood by Achinstein to be a procedure of analysis. The theoretical model analyzes the object or system into more basic components.

Thirdly, a theoretical model is a useful approximation for deriving known principles or statements of regularities (laws and formulae, for instance). This third characteristic of a theoretical model is presented as reasoning in this manner:

It is useful to represent x's as having such and such a structure, for then various known principles can be derived; moreover, the actual structure that x's have is something like this, though quite possibly more complex (or in many cases actually known to be more complex).\textsuperscript{78}

\textsuperscript{75}ibid., p. 102.
\textsuperscript{76}ibid., p. 103.
\textsuperscript{77}ibid.
\textsuperscript{78}ibid., p. 104.
For example, the billiard ball model (the elastic sphere model) enables one to derive the above formula for ideal gases. Yet the van der Waal formula has been found to be more precise and, as previously noted, was derived from a different model. The billiard ball model and the ideal gas formula ("PV=kT") are, however, sufficient if one only wants or needs a good approximation to the relationship between a gas's pressure (P), volume (V), and temperature (T) ("k" is the gas constant). On the other hand, the rigid sphere model is mathematically more complex and is used only if more accurate calculations are needed. It describes more accurately the nature of a gas than the billiard ball model. And because we have the rigid sphere model, we know that the actual structure of gases is more complex than the structure described by the billiard ball model. Moreover, with respect to different purposes, models differ as to utility.

On this basis Achinstein makes a distinction between a model and a theory. To have a model of an x is to have: (1) a way of describing at least some parts of x approximately, and (2) the possibility of describing x in more than one way. But to have a theory of x is to have an exact description of x's principles, not merely a useful approximate set of descriptions. What was once proposed as a theory may, upon further investigation, be shown to be an approximation. Hence this distinction of Achinstein helps explain why certain linguistic systems originally called theories are now held to be models that approximate the object or system. It should be noted, however, that if one has a theoretical model of an object or system, then one may have a theory (the most approximating model thus far) of the object or system. It
might be argued that it is possible to have several models of a system, none of which seem to be any more approximating than the others. In such a situation, the models would treat of different and presumably independent aspects of the system. Yet the models would not yet have been shown to be approximations in the sense of predicting something shown to be false though approximately accurate. It would seem, then, that the two models are merely parts of the theory of the system since they do not present different interpretations of the same aspect of the system.

Is it possible to have two models of the same system such that they provide different interpretations of at least one aspect of the system and both have not yet been shown to be theoretical model approximations? It seems to be possible provided the different interpretations are logically compatible. Thus, again, their conjunction would constitute the theory of the system in question. Which interpretation one uses would depend upon what one wants to explain or predict on the basis of the system. It should be noted that 'model' in these last two possibilities is not the same as 'theoretical model,' since a theoretical model is always known to be an approximation.

The fourth characteristic is that "a theoretical model is proposed within the broader framework of some more basic theory or theories." By this characteristic Achinstein means that when a scientist proposes a theoretical model about some objects, he invariably ascribes some

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properties to the objects which are within the broader framework of another theory. For instance, the billiard ball model ascribed some properties to molecules which are principles of Newtonian theory. The theoretical model uses these principles or assumptions of the more basic theory (claims Achinstein) in a simplified and approximate manner. Furthermore, when a theory is proposed, the scientist may also formulate a theoretical model for the theory which contains as an approximation the more important principles of the theory simplified to facilitate calculations or understanding.

The fifth and final characteristic is that:

A theoretical model is often formulated, developed, and even named on the basis of an analogy between the object or system described in the model and some different object or system. In the billiard ball model, an analogy is established between the gas molecules and elastic billiard balls. It is usually the case that the theoretical model is more familiar and simpler than its analogous theory. Reasoning from the model to the analogy, and conversely, is, of course, analogical.

Of these five characteristics of theoretical models, Achinstein proceeds to discuss the uses or functions. He holds that theoretical models can have the same functions as theories: they likewise can be "used for purposes of explanation, prediction, calculation, systematisation, derivation of laws and so forth." Yet theoretical models differ from theories in the manner by which they perform their function.

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81 Ibid., p. 106
Theoretical models provide the above functions only on a simplified and approximate basis. The theory provides such functions in a more complex and accurate basis. The theoretical models are used when: (1) there is no accurate theory for the object or system under investigation; (2) calculations from the theory are either too complex or impossible; (3) one is considering how to extend or modify a theory; and (4) one is explaining a theory: presenting the model first is usually the best didactic approach.

On the basis of the characteristics and functions of theoretical models, Achinstein attacks the traditional concept of model as presented by Nagel. Achinstein argues that Nagel's definition of 'model' is not acceptable presumably because it differs from Achinstein's definition of a 'theoretical model.' The task now is to determine the soundness of Achinstein's claim. First of all what relationship, if any, does Achinstein's theoretical model (call it 'model\textsubscript{4}') have with model\textsubscript{1} and model\textsubscript{2}? As I understand Nagel, a model\textsubscript{1} is an interpretation of the calculus of a theory. Hence it is identical with part of a theory's components. A model\textsubscript{2}, however, is a model formulated by analogy (substantially or formally) on the basis of a theory. Thus a model\textsubscript{2} definitely has (Achinstein's) characteristic four. Even if a model\textsubscript{2} is an arithmetic representation of an empirical theory it still consists of a set of imputed assumptions (about an object or system) which describe the different properties of that object or system. Thus model\textsubscript{2} will have characteristics one and two.

It is not, however, at all obvious that model\textsubscript{2} meets characteristics three and four. Let us say that A is model\textsubscript{2} of B. Is it then
the case that \( A \) is an approximation of \( B \)? \( A \) could be an approximation of \( B \) depending on how the analogy was constructed. The analogy would have to be substantive (and, therefore, at least partially formal) since the entities of \( A \) explain in approximation the entities of \( B \). Thus we can conclude that, with respect to characteristic two, theoretical models are special cases of model\(_2\), i.e., when \( A \) is a substantive approximate analogy of \( B \).

The preceding analysis also holds for characteristic four. When Achinstein says that a model is formulated within the framework of another theory, he does not mean the theory the model is modeling. For example, if \( A \) is a model\(_4\) of \( B \), then there is some theory \( C \) such that \( A \) is proposed on the basis of \( C \). Apparently \( C \) is more general than either \( A \) or \( B \); i.e., \( C \) is about other objects or systems as well as those of \( A \) and \( B \). Thus it is possible that one formulate \( A \), a model\(_2\) of \( B \), on the basis of \( C \) and the substantive analogy from \( B \). Again model\(_4\) would be a special case of model\(_2\). Since these are the five characteristics of theoretical models and since they can be all explained by the model\(_2\) concept, Achinstein's criticism of Nagel seems to be unsound. I believe that it is clear that model\(_2\) can have all the functions that Achinstein attributes to model\(_4\). In short, model\(_4\) are approximative substantive model\(_2\).

Would Achinstein's argument have better success if directed against Braithwaite's conception of model\(_3\)? On Braithwaite's grounds, if \( A \) is a model of \( B \), then \( A \) and \( B \) have the same abstract calculus. This leaves open the possibility of substantive similarities between the objects of \( A \) and \( B \). Braithwaite's model\(_3\) will definitely have
characteristics one and two. It is quite possible that it will sometimes have four. But what about five? A and B are not merely analogous with respect to logical form, they have the same logical form. But they can easily be analogous with respect to their objects. Thus model\(_3\) can have characteristic five. With regard to three, could A be an approximation of B? That it might be an approximation seems reasonable since that could be the reason why, as we have already noted, Braithwaite warns against identifying the model with its theory. It might also be argued that A could not be substantively analogous to B if A's objects did not approximate B's. With these two points in mind, I conclude that Achinstein's characteristics for model\(_4\) can also be characteristics of model\(_3\). Thus both Nagel's and Braithwaite's views can account for most models\(_4\). Braithwaite's cannot account for the situation where A is a model\(_4\) of B but is not a model\(_3\) of B; i.e., A and B have different calculi. The latter situation is not only possible; it is probably the usual situation between model\(_4\) and its theory. It may also be the usual situation in which the models (in the senses we have been considering) are considered to be true. We have noted Nagel's claim that the model be true whereas Braithwaite does not believe that truth of a model has any bearing on its proper function of clarifying theories. Braithwaite's attitude towards models is analogous to the fictionalistic agnostic's attitude towards theories and theoretical concepts: whenever one uses them in thinking, one is always as-if thinking. That some models and theoretical concepts have been imaginary has given these views some plausibility. There is also some similarity between these two views and Bergmann's model pattern
discussed earlier. According to that criterion, however, the entities of the model do not exist. Hence the model pattern is not as agnostic as are the other two.

Section IX

Hesse's Views on Models

The preceding discussion brings up the question about the relationships models may have to the referential and epistemological status of their theory's theoretical terms. The more important possible relationships have been considered by Hesse. The model that is related to a theory is called a theoretical model. Like most other philosophers of science, she argues that a theoretical model is an interpretation of a calculus that has deductive observable consequences. The model is an interpretation of something in terms of something else which is more familiar and better understood. (Sometimes it may even be diagrammatic or pictorial.) That which is more familiar and better understood is, of course, cognitively prior to the model.

According to Hesse, if A is analogous to B, then A and B are similar in certain respects (the positive analogy) and A and B are dissimilar in other respects (the negative analogy).\(^\text{82}\) She holds that analogy is the relationship that a model has to whatever it models. Like Achinstein and Nagel, she distinguishes what I have called formal

analogy from substantive analogy ('material' is her word for the latter type). The model is used to explain and predict, in terms of the familiar or more intelligible, the phenomena in question. Because of this function, Hesse claims that the model possesses surplus meaning. By having such surplus meaning, the model conveys by analogy to the system of true statements about the phenomena other statements of association and implication for the phenomena's concepts. It is this surplus meaning of the model that enables us, so she argues, to use the model for predicting new phenomena and explaining both old and new data.

In her analysis we have another explication of 'surplus meaning' which, as we have noticed with Feigl, is usually associated with realism. As a realist, Feigl explicates it in terms of factual reference; Hesse, in terms of the model's function and capabilities. In discussing instrumentalism, Nagel noted that the surplus meaning of a theory could be, for the instrumentalist, the theory's model (its pictoral meaning, for instance). As against Feigl, the instrumentalist could deny surplus meaning to the theory's or the model's entities by holding that the entities are either totally observational (no theoretical ones) or imaginary (in which case their existence would be irrelevant to the theory). Though it would seem that Hesse's analysis favors the instrumentalist's view in her holding surplus meaning for a model, she argues for realism. Hesse denies the distinction between a theory and a theoretical model. A theoretical model that has been

83 Ibid., p. 356.
well-confirmed is a theory.\footnote{\cite{Hesse:1966}, pp. 356-358.}

She explains the surplus meaning of a model on the basis of analogy. If there is any negative analogy between the model when it is formulated and true statements about the phenomena (explanandum), it is to be removed. However, with such removal it is not the case that there remains only the positive analogy between the model and the explanandum. If this were the case, then the model and explanandum would become identical. But such an identity cannot occur if the model possesses surplus meaning over the explanandum. In solving this problem, Hesse posits a set of properties belonging to the model whose analogy (positive or negative) to the explanandum is not yet known. She calls these properties the neutral analogy. Upon investigating these properties, one will find that they will be either positive or negative analogies. In such investigations the model (and, therefore, the theory) is extended so that new empirical tests can be made for either confirmation or refutation of the theory or model. The new negative analogies (if there be any) will be removed from the model, of course, but such a removal will automatically modify the model itself. On the other hand, the new positive analogies enable the model to make new predictions and to incorporate new data into the explanandum. In this manner Hesse explains how models and theories become revised and extended. Presumably the model will never become exhausted of surplus meaning. Like the atomic theory from Democritus and Dalton to Quantum mechanics today, the model undergoes a variety of changes; yet its predictive and explanatory import steadily increase.
A question arises as to this analogical relationship which the model has to the explanandum. In the other views of models, we noted that models have analogies to theories, not necessarily to what the theories are supposed to predict or explain. The relationship between the theory and the explanandum was considered to be deductive not analogical. It would seem, then, that Hesse is denying that the relationship between a theory and its explanandum is deductive; she does not hold to the traditional view. I doubt if she would deny some deductive relationships between the theory and the explanandum, but it seems that in extending the theory's domain one must reason analogically. As we have noted earlier Braithwaite denies that analogical reasoning is any better than any other method of theory extension. The issue between Hesse and Braithwaite is similar to that of the Campbellian and Duhemist in Hesse's dialogue on the function of models. The Campbellian (a follower of N. R. Campbell) holds that models are essential for a theory's predictivity and explanation whereas the Duhemist (a follower of Pierre Duhem) holds that models are not logically essential for any function of a theory--at most they are psychologically necessary for those having trouble understanding the theory. If one can show that theories exist which have predictive value but no models, then the Campbellian thesis would be false. Hesse cites the fact that quantum theory has been so interpreted.


The issue between the Campbellian and the Duhemists is related to the realist-nonrealist controversy. If models are merely heuristic devices, then their referential status need not arise at all. However, if theories must be developed in terms of models, then the model may have factual import. Hesse presents some of the arguments against this realist interpretation of the function of models as well as her own replies. (It should be noted that this realist interpretation is not the only possible realist interpretation. After all, there have been many different uses of 'real' as previously analyzed.)

The first criticism of this realist view of models is that one should not identify the model with its theory since "the model may have implications that turn out to be untrue of the explanandum." We have noted that Nagel and Braithwaite make this criticism. Hesse replies by saying that it overlooks the fact that negative analogies have been either removed from the model or are ignored. But Braithwaite's reply would be: on what basis, except testing, do we know which properties are positive and which are negative? Furthermore, Braithwaite might wonder just what Hesse is distinguishing when she distinguishes a model from a theory. Like Achinstein she seems to be saying that a theory is the true formulation of the explanandum and a model is our attempt to represent that formulation. But if theories contain at least one universal unrestricted statement, then we will never know whether we have a theory or a model as long as the statement has not been falsified. The distinction seems to be empirically useless.

Another criticism of this realist view is that even if models are
essential to theories, it does not follow that their surplus meaning should be identified with factual reference. This latter point is, of course, the heart of the controversy.

In the next section, I will consider in some detail the different views as to the function and justification of theories. One of the points brought out in this last discussion is whether theories function deductively or inductively in explaining and predicting phenomena. It is to this problem that I now turn.
CHAPTER III
THE THEORY IN EXPLANATION AND PREDICTION

Section I
The Functions of Theories

At the beginning of the last chapter, I cited references supporting the view that a theory's two functions are to make both explanations and predictions of phenomena. There may be other functions, such as to unify our knowledge of diverse phenomena, but these two are definitely the most discussed in the literature. Henryk Mehlberg, for example, distinguishes the empirical functions of a theory from its theoretical functions.¹ As to the empirical functions, he lists four: (1) the theory's summarizing function, (2) its predictive function, (3) its controlling and explanatory functions, and (4) its informational function. By the summarizing function, Mehlberg understands that a theory either is a statement or entails a statement that summarizes or condenses the empirical characteristics of a set of phenomena.² For example, he claims that from the law of least action all other mechanical laws are derivable, and that these laws in turn


²Ibid., p. 276.
describe the characteristics of all mechanical entities. That which is summarized by a theory or law, however, is only what has occurred. Thus the summary will not include any predictions the theory or law may be used to make. At this point the need for the predictive function arises. A theory must be capable of predicting either a new law or an as yet unobserved event. The new law in its turn will be used to predict other events.\(^3\)

Mehlberg also holds that an acceptable theory should enable us to control and modify our environment. Furthermore, an acceptable theory should tell us why "a fact known to have taken place actually did so, why a law known to be valid . . . is actually valid."\(^4\) This latter function is, of course, the theory's explanatory function. The last function, the informational, concerns the kind of knowledge theories deal with, namely, knowledge of our observable environment.\(^5\) The theoretical functions of a theory, on the other hand, seem to be the logical conditions necessary for a theory to be able to perform the above four empirical ones.

I want to comment on these four empirical functions. With regard to summarizing, a theory will perform this function only if some of its statements are generalizations (or entail generalizations) since statements of generalization are at least partially equivalent to statements of particular facts. As to control, it seems that prediction is a

\(^3\)Ibid., p. 277.

\(^4\)Ibid., p. 278.

\(^5\)Ibid., pp. 278-279.
necessary condition for our being able to control something. What we predict are statements about entities we hope to experience under certain conditions. Thus in making a prediction scientifically we must have knowledge of the relationships between one set of observables and another set. Hence if we are able to modify one set, we may also be able to modify the other. In this manner are prediction and control related. One could, however, also argue that unless one knew why the observables were so related one would not be able to modify and therefore control them. So it seems that explanation is also essential for control. Explanation and prediction though are not sufficient for control. It must be the case that one also has the ability to modify the phenomena that one explains and predicts. As to the relationships of prediction to explanation, Mehlberg sums up the traditional view on this matter:

> the predictive and explanatory functions of a scientific theory are inseparable from each other: a theory cannot be predictively successful unless its explanatory function is satisfactorily discharged, and vice versa.

Mehlberg's view can only be assessed if we carefully examine the logic of scientific explanation and prediction.

**Section II**

*The Logic of Explanation for Individual Events or Processes*

Throughout Chapter II, I have been examining the structure and function of the traditional view of theories for one reason: to find

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a criterion of adequacy the cognitive views must all have in order to explain the structure and function of scientific theories. Thus I have started with the most influential view if not the most widely held one. After examining and evaluating the criticisms of this traditional hypothetico-deductive view, I hope to arrive at some essential characteristics a theory must have in order to be scientific. I shall evaluate descriptivism, instrumentalism, and realism on the basis of their being able to explain adequately these essential characteristics as well as on their being internally consistent. If each view explains adequately those essential characteristics and seems to be internally consistent, then these views will have to be examined on some other grounds.

It may be truistic that in explaining something one presents an explanation. Explanations are sometimes given as the answers to questions of the form "Why is that X?" Yet they can also be given as the answers to questions of the form "How was Y able to do Z?" It has been argued that not all requests for explanations are why questions. For example, Michael Scriven asks: "'How can the sun possibly continue to produce so much energy with a negligible loss of mass?"' Scriven argues that that question can only be rephrased as a why question with difficulty. But I find no difficulty in the following: "Why can the sun possibly continue to produce so much energy with a negligible loss of mass?" Whatever explains the one will also explain the other.

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Hence they roughly are equivalent questions. William Dray argues that the historian gives explanations to what questions rather than why or how (unless the specific what question is equivalent to a why or how question). For example, "What happened in France, during 1789?" is the kind of question Dray believes the historian attempts to explain. It does seem to be correct that in this example there will be difficulty in rephrasing it into either a how or a why question. Upon answering the what question, however, one can then ask why or how. For example: What happened in France, during 1789? Answer: A revolution. But why did it take place? and how? (The latter two do not seem to be equivalent questions. Explaining why the revolution took place may be presenting the economic turmoil and motives of the people. Explaining how it took place may be presenting the types of rioting, trials, and executions that took place.)

It seems that there are no specific linguistic indicatives that can tell us whether we have a request for an explanation. Only the context of the question tells us. All of the above requests for an explanation do have one thing in common, namely, they are requests for explanations of particular events or particular processes or occurrences. Yet we can also ask why it is the case that ice floats on water. Here we are not asking why a particular piece of ice is floating on water, but why all pieces of ice, if they were put in or on water, float. Such a request for an explanation is a request for the explanation of a universal statement, and in this example the universal

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statement is a law. Thus it would seem that we have at least two basic kinds of explanation: (1) explanation of particular events, processes, or situations and (2) explanations of universal statements. On the basis of this division let us consider the traditional view on the logic of explanation.

The traditional view on the logic of explanation is presented by Carl Hempel and Paul Oppenheim as having to meet four criteria (the first three being logical conditions of adequacy and the fourth being the empirical condition of adequacy). Hempel and Oppenheim differentiate two constituents of an explanation: (1) the explanandum which is the statement describing the explained phenomenon and (2) the explanans which is the statement or set of statements that are presented to account for the phenomenon. The four criteria are the following:

(R1) . . . the explanandum must be logically deducible from the information contained in the explanans . . . .
(R2) The explanans must contain general laws, and these must actually be required for the derivation of the explanandum . . . .
(R3) The explanans must have empirical content; i.e., it must be capable, at least in principle, of test by experiment or observation . . . .
(R4) The sentences constituting the explanans must be true. 9

With regard to (R1), Hempel and Oppenheim believe that only if the explanans entails the explanandum will the explanans adequately explain the explanandum. With regard to (R4), Hempel and Oppenheim realize that it sounds too restrictive. Why not state that the explanans

should be highly confirmed and has no known disconfirmations? They reply by saying that in the past many highly confirmed statements were later shown to be false. Thus if such a statement were part of an explanans, we would have to say that any explanation which contained it was a good one until it was found to be false. Hence, they argue, we would have to hold that a correct explanation can become an incorrect one, and they believe that such a use of 'explanation' is not in accord with sound common usage. Therefore they hold (R4). So if a part of an explanans is eventually falsified, the explanation was never a correct explanation. But, as is usually the case in scientific matters, if the explanans is to contain a law, and laws are universal unrestricted statements, then we will never know with certainty whether we have a correct explanation. So if we follow their account, all our explanations will be tentatively correct explanations—always subject to revision or rejection.

Although Hempel and Oppenheim present (R1) as a condition for all adequate explanations, they do hold that the logical relationship can be inductive since some laws used in explanations are statistical.\(^\text{10}\) By permitting both deductive and inductive relationships between the explanans and the explanandum, two main kinds of explanations can be distinguished: (1) the deductive explanation (called the deductive-nomological by Hempel) and (2) the inductive explanation (called the inductive-statistical explanation by Hempel). These two types of explanation can be distinguished from each other by their logical

\(^{10}\text{Ibid.}, p. 251; 278.\)
forms. Furthermore their explanantia may be distinguished by the fact that all the laws in the deductive explanation must be universal, unrestricted laws (sometimes called 'deterministic'), and that at least one of the laws in the inductive explanation must be statistical (about a probability that ranges from greater than 0 to less than 1). An example of a statistical law is that the probability of any radium-226 atom to decay by 1620 years is one-half.\textsuperscript{11} An example of a universal law is that all men are mortal. Because both types of explanation contain laws, Hempel calls his view of explanation the covering-law model\textsuperscript{12} since whatever is being explained is explained in terms of laws.

If the explanandum is a statement about a particular then, whether the law be universal or statistical, at least one more statement which is about a particular is needed in the explanans. Thus we have the two logical forms for the explanation of a particular:\textsuperscript{13}

\[
\begin{array}{ccc}
\text{Deductive-Nomological} & \text{Inductive-Statistical} \\
C_1, C_2, \ldots, C_k & p(F,G) \text{ is high} \\
L_1, L_2, \ldots, L_r & \text{explanans} & i \text{ is } G \\
E & \text{explanandum} & i \text{ is } F
\end{array}
\]

The statement represented by \('C_1, C_2, \ldots, C_k'\) is the conjunction of statements about particulars and the statement represented by \('L_1, L_2, \ldots, L_r'\) is the conjunction of the universal laws. It is


\textsuperscript{13} Ibid., pp. 336, 383; also Hempel, Philosophy of Natural Science, pp. 51, 67.
always possible that the deductive-nomological (D-N) explanation have only one statement about a particular and only one law. The single line separating the explanans from the explanandum is Hempel's way of representing that the explanans entails the explanandum.

With regard to the form of the inductive-statistical (I-S) model, the statement represented by 'i is G' is that a particular 'i' has the property denoted by 'G.' The statistical law is represented by 'p(F,G) is high' and is to be read as the probability of something being an F if it is a G is high. By 'high' is meant greater than \( \frac{1}{2} \) and less than 1. 'E' in D-N and 'i is F' in I-S represent the statement to be explained. The double line separating the explanans from the explanandum in I-S is Hempel's way of indicating that the explanans only confirms or inductively supports the explanandum.

An extensive literature has come forth evaluating, criticizing, and defending the covering-law model of explanation—most of it concerned with the D-N model. I want first, to consider whether explanation is deductive or inductive or both. The view of probabilistic realism, which Feigl discussed, would hold that all explanation is or can be inductive, and hypothetical-deductive realism would hold that all explanation is or can be deductive. However, according to the traditional view as presented by Hempel, explanation can be both. This view of explanation is accepted by Carnap,\(^{14}\) Pap,\(^{15}\) and Nagel.\(^{16}\)

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\(^{14}\)Carnap, op. cit., pp. 7-8.

\(^{15}\)Pap, op. cit., pp. 345-346.

\(^{16}\)Nagel, op. cit., pp. 21-23.
is considered by them to be an appropriate form of explanation. Yet Brodbeck,¹⁷ Braithwaite,¹⁸ and Popper¹⁹ hold that all explanation is essentially deductive. Earlier, when presenting the main characteristics of the traditional view, I noted that a theory is considered to be a deductive system which entails observational statements. Furthermore, since theories are supposed to explain phenomena, and since some explanations seem to be inductive, then either (1) not all theories are solely deductive systems, or (2) there are no inductive explanations, or (3) theories are deductive systems and there are inductive explanations, but not all explanations are deductive. Let us examine carefully these three alternatives. If we accept the last, we will have to hold that there are some phenomena that cannot be explained by a theory. But since one of the functions of a theory, according to the traditional view, is to explain phenomena, it does not seem to be the case that an advocate of the traditional view could accept this alternative. This argument leaves the advocate with choosing either that not all theories are deductive systems or that there are no inductive explanations. Are there any good arguments for accepting the latter alternative?

Popper rejects inductive explanation, because he finds "Hume's


¹⁸Braithwaite, Scientific Explanation, pp. 342ff.

refutation of inductive inference clear and conclusive;"20 in short, induction is unjustified. Braithwaite explains the function and testing of statistical laws and hypotheses on the basis of the class-ratio deductive theory.21 Thus, according to Braithwaite, all laws whether universal or statistical, explain by being a part of a deductive explanation. Brodbeck likewise argues in detail for explanation as deductive. According to Brodbeck, "either the explanation is deductive or else it does not justify what it is said to explain."22 Thus, with Popper, Brodbeck holds that inductive reasonings are unjustified. Brodbeck believes that deduction provides the only logical connection between the explanans and the explanandum. The philosopher is concerned only with the logical relationships between statements in an explanation rather than the beliefs or behavior of an individual or group as to what satisfies them as an explanation. She is presenting an analysis of explanation based on the logical relationships between statements rather than on people's attitudes about them.23 Nevertheless, Hempel could reply that he was doing the same when he presented the I-S form of explanation; i.e., in the I-S form the relationship between the explanans and explanandum is one of likelihood rather than entailment. When Brodbeck examines the I-S explanation, she seems to advocate the view that those areas which use statistical laws are areas that are

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20 Ibid., p. 42.

21 Braithwaite, Scientific Explanation, p. 117.


23 Ibid., pp. 370; 373.
about subject matters which we do not completely understand. Our lack of understanding is due to not knowing all the variables relevant to the investigated subject matter. Because of such lack of knowledge predictions are not exact, since we do not know enough about the initial conditions on the basis of which we make our predictions. Consequently, if our understanding of a subject is deficient, so too will be our explanations and predictions based on it.

Our deficient knowledge of such areas is compensated by developing statistical hypotheses and laws about the phenomena of the areas. These are statements that have the form \( p(F,G) \) is high. A statistical law states that a certain percentage or ratio (probability) of the members of a particular class (named by \( 'G' \)) are also members of another class (named by \( 'F' \)): for example, 'the probability of recovery in cases where a person is suffering from streptococcal infection and the person is treated with large doses of penicillin is close to 100\%.'

Just for a comparison, consider the universal statement: 'All cases where a person is suffering from streptococcal infection and the person is treated with large doses of penicillin are cases in which the person recovers.' First of all, in our comparison let us consider the grammatical subject of each law. The universal law is concerned with cases in which people are suffering and are having certain treatment, whereas the statistical law is about a certain probability of recovery in the above cases. Consequently, their subjects and what they refer to are not the same. However, just because two statements do not have the same subject does not mean that the statements are not logically equivalent. The two statements "John loves Mary" and "Mary is loved
by John” have different subjects ("John" in the first; "Mary" in the second), yet they are logically equivalent. Furthermore, most people would hold that any statement of the form 'All A are B' is logically equivalent to '100% of the members belonging to A also belong to B.' The first statement-form is that of a universal law; the second, of a statistical law because of the subject's reference to a percentage. However, is it the case that a statistical law of the form 'Less than 100% of the members belonging to A also belong to B' is ever logically equivalent to a universal law? If it is, then it may well be the case that we do not need the I-S form of explanation.

As we have seen, Brodbeck holds that statistical knowledge is a sign of imperfect knowledge. Braithwaite, however, notes that perhaps statistical hypotheses are the normal whereas the so-called universal hypotheses are those statistical ones that are about percentages very close to 100%, or to 0% ('No A are B' form). Braithwaite cites the opinion of some quantum physicists who hold that the statistical laws of quantum mechanics will never be replaced, even with an increase in knowledge, by universal ones. But even if they would be replaced at some later time, today they represent the best in scientific thought "and this cannot be ignored."24 However, this point is recognized by both Pap and Jaegwon Kim. Pap argues that if one upholds the D-N model for scientific explanation, he is likely to frustrate the social scientists since most of their laws are statistical.25

24 Braithwaite, Scientific Explanation, p. 117.
25 Pap, op. cit., p. 160
Kim argues that since some of the basic laws of science are essentially statistical, then we must recognize besides the D-N model the I-S model of explanation. However, even if it is true that statistical laws are essential for scientific explanations, it does not deductively follow that those explanations containing statistical laws cannot be deductive. In fact Brodbeck argues that they must be deductive. If Brodbeck is correct, then there must be some way of interpreting an I-S explanation into an equivalent explanation that permits a deduction of the explanandum from the explanans.

How is such an interpretation to be presented? First of all, consider the form of any inductive argument. It can be expressed quite simply as:

\[
\frac{p}{q}
\]

where 'p' is supposed to give some kind of support or confirmation to 'q.' This argument-form may be easily converted into a deductive one by the addition of the single statement-form 'if p, then q.' Thus we would have:

\[
\begin{array}{c}
\text{if p, then q} \\
p \\
q
\end{array}
\]

as our new argument-form (modus ponens, of course). By analogy, then, take the argument form of I-S:

\[
\begin{array}{c}
p(F,G) \text{ is high} \\
i \text{ is } G \\
i \text{ is } F,
\end{array}
\]

---

and add the statement-form 'if \( p(F,G) \) is high and \( i \) is \( G \), then \( i \) is \( F \)' to it, and we acquire the valid deductive argument-form

\[
\begin{align*}
\text{if } p(F,G) \text{ is high and } i \text{ is } G, \text{ then } i \text{ is } F \\
p(F,G) \text{ is high} \\
i \text{ is } G \\
i \text{ is } F.
\end{align*}
\]

The major objection to this deductive conversion of I-S will center on the added hypothetical premise. Is it justified? Let us consider an example from Brodbeck: "'60 per cent of all cigarette smokers develop lung cancer.'"\textsuperscript{27} This statement was arrived at by taking a large sample of smokers and discovering how many out of the total number of smokers acquired lung cancer. The ratio of the number of smokers who acquired lung cancer in the sample to the total number of smokers of the sample was found to be 60 per cent. From the observations and ratio (frequency) of the samplings, the statistical law was generalized. According to Brodbeck, the statement that 60 per cent of all cigarette smokers develop lung cancer:

is a generalization or universal statement, for it says of all cigarette smokers, past, present, and future, those observed and those as yet unobserved, that 60 per cent of this group will suffer from cancer of the lungs.\textsuperscript{28}

Thus, in the class of cigarette smokers, the frequency of lung cancer being found amongst its members is 60 per cent. Brodbeck claims that the statistical statement is about all cigarette smokers. Let us assume that Joe is a cigarette smoker. Thus, according to Hempel we could set up the I-S explanation:

\textsuperscript{27}Brodbeck, "Explanation, Prediction, and 'Imperfect' Knowledge," p. 377.

\textsuperscript{28}Ibid.
The probability of a cigarette smoker suffering from lung cancer is 60 per cent.

Joe is a cigarette smoker.

Joe suffers from lung cancer.

However, the deductive version would be:

If the probability of a cigarette smoker suffering from lung cancer is 60 per cent and Joe is a cigarette smoker, then Joe suffers from lung cancer.

The probability of a cigarette smoker suffering from lung cancer is 60 per cent.

Joe is a cigarette smoker.

Joe suffers from lung cancer.

Is it possible that an individual could be justified in holding the preceding I-S version but not its deductive version? The difference is, of course, the hypothetical statement of the deductive version. It is a contingent statement. Since 60 per cent is a rather low percentage, some cigarette smokers will not suffer from lung cancer and Joe may be one of them. But to say that the statement is contingent in no way detracts from its being accepted. For if Joe does not suffer from lung cancer (and the non-hypothetical premises are true), then the hypothetical will be false whereas the I-S version will have all true premises and false conclusion. It might be objected that since the hypothetical is false, the deductive version cannot be an explanation, though the I-S could still be. But such an objection is incorrect. The only reason why the hypothetical would be false is that Joe did not suffer from lung cancer. But if he did not suffer from lung cancer, then the I-S explanation which purports confirmation of Joe's having lung cancer would not be an explanation either: a purported explanation of something false is no more an explanation than one which itself is false.
Another objection against the deductive version might be the following. The deductive model is a trivialization of the I-S model, because the conclusion is merely repeated amongst the premises. The deductive model can be symbolically represented as modus ponens:

\[
\text{if } p, \text{ then } q
\]

\[
\begin{array}{c}
p \\ q \end{array}
\]

Now the conjunction of the premises 'if p, then q' and p' is logically equivalent to 'p and q' in which 'q', the conclusion appears. Thus, the deduction fails to be a candidate for an explanation model. If this argument is sound, how could it be the case that there are any deductive explanations? In re-examining Hempel's form for D-N we find that it contains a universal law—a law lacking in the deductive version of the I-S model. Now could it be possible to convert the hypothetical statement in the deductive version to a universal law? Consider the statement 'For any x, if x is a cigarette smoker and the probability of a cigarette smoker suffering from lung cancer is 60 per cent, then x suffers from lung cancer.' This statement entails the hypothetical statement in the deductive version. Yet it could be the law we are looking for. But it cannot be the law of a good explanation since it is false. It is false because it entails that, since the probability of a cigarette smoker suffering from lung cancer is 60 per cent, then all cigarette smokers are suffering of lung cancer (commutation and exportation on the law). If it is true that only 60 per cent suffer, it cannot be true that all (100 per cent) suffer. This argument could be applied mutatis mutandis to any other I-S explanation that is converted into the deductive version presented. Consequently, it would
seem that if such statistical laws are to be used in non-trivial
deductive explanations, the deductive version must be something else.

Perhaps the preceding conclusion was drawn too quickly. The argu-
ment against the deductive version presupposed that a deductive argu-
ment containing a universal law is not trivial. However, is the argu-
ment used against the deductive version also applicable against the D-N
model? If our law is of the form '(x)(Ax>Bx)' and the statement of
initial condition 'Aa' ('individual a is a member of class A), we can
explain a statement of the form 'Ba' by using the argument form:

I : (x)(Ax>Bx)
   ____________
  Aa           Ba

Yet the above argument form is logically equivalent to the argument
form

II : (x)(Ax>Bx)·(Aa·Ba)
    ____________
       Ba

in which Ba, the statement-form of the explanandum appears as a premise
conjunct. Therefore, it seems that either all deductive explanations
are trivial or we need not accept the I-S model. But there is a dif-
fERENCE. The deductive version of the I-S explanation would be:

III : Aa>Ba
     ____________
    Aa           Ba

which is logically equivalent to

IV : Aa·Ba
   ____________
      Ba.

Now even though I and II are equivalent and III and IV are equivalent,
there are differences between II and IV. In II the universal law
appears as a premise conjunct, and is necessary for II being equivalent
to I, but no hypothetical statement appears in IV that is necessary for IV being equivalent to III. Such a difference between II and IV constitutes a disanalogy between D-N forms of explanation being trivial and the deductive version of I-S being trivial. Consequently the conclusion—that if statistical laws are to be used in non-trivial deductive explanations, then the deductive version must be something else than III—was not hastily drawn.

As another attempt consider the following example of a law: 'For any x, if x is a cigarette smoker, then the probability of x suffering from lung cancer is 60 per cent.' One can use this law in deriving the statement: 'If Joe is a cigarette smoker, then the probability of Joe suffering from lung cancer is 60 per cent.' That statement in conjunction with 'Joe is a cigarette smoker' entails 'the probability of Joe suffering from lung cancer is 60 per cent' which is an instantiation of the statistical law with respect to Joe. But now we are right back with the premises (or their instantiations) of the I-S explanation. Have we made any progress? If we were seeking a deductive explanation of 'the probability of Joe suffering from lung cancer is 60 per cent' in terms of a universal law, then we have found it in the above law. This deductive model has the following form:

\[
\begin{align*}
\text{For any } x, \text{ if } x \text{ is an } A, \text{ then } p(Bx) \text{ is } Z\%.
\end{align*}
\]

\[
\begin{align*}
\text{\underline{x is an A}} \\
\text{\underline{p(Bx) is } Z\%}.
\end{align*}
\]

'p(Bx) is Z%' is read as the probability of x being a B is Z%. What we have explained is why the probability of (say) Joe's suffering from lung cancer is 60 per cent, and not, as in the I-S form, that Joe is suffering from lung cancer. This conclusion is in agreement with
Brodbeck who states that, from a statistical law and statement of initial conditions, we can only explain the probability that an event of a certain group is also a member of another group.\(^{29}\) If Brodbeck is correct (and the preceding arguments indicate that she is), there is no deductive explanation of an individual event by means of a statistical law. But can there be, as Hempel holds, inductive explanation of an individual event by means of a statistical law?

Let us consider again the I-S explanation of why Joe suffers from lung cancer: Joe is a cigarette smoker and the probability of a cigarette smoker suffering from lung cancer is 60 per cent. But is this actually an explanation of why Joe is suffering from lung cancer (if he is)? Even in the light of the purported explanans, the available evidence, I can still intelligibly ask: Why does Joe (not, What is the probability frequency of his having lung cancer?) have lung cancer? Telling me what the probabilities are, no matter how high (just so long as they are less than 100 per cent), and that Joe is a smoker does not tell me why Joe has cancer. This argument is a form of the open-question argument. It seems that anyone who uses it in this context is asking for the cause of the Joe's cancer. The explanans do not provide any such cause: only a less than 100 per cent correlation between the cancer and cigarette smoking. In extending this argument to all I-S explanations, we can still ask why intelligibly of their explanandum even knowing and accepting the explanans.

Hempel does have a reply to this argument. He believes that

\(^{29}\text{Ibid., p. 378.}\)
denying the I-S form as a proper kind of explanation is scientifically too restrictive.\textsuperscript{30} It would mean that many scientific laws and theories that are statistical would not have any explanatory import. Secondly, he seems to agree with Mises that eventually we will become so used to the I-S type of explanation that we will become satisfied with it as a mode of explanation.\textsuperscript{31} Given such satisfaction, the open question argument would not arise again. Let us consider these replies in order of their presentation.

First, all that would follow from rejecting I-S explanations would be the non-explanation of the occurrence of individual events, not the explanation of the probability of something of one class also being of another class. Thus it is not correct to say that rejection of the I-S model would mean that statistical laws and theories would have no explanatory import. They can deductively explain probabilities, but not individual occurrences. Of course, Hempel holds that certain statistical laws and theories do explain the occurrence of individual events. His argument is based on three examples from science and by recent probability theory. I shall first examine his examples.

His first example is from the principles of Mendelian genetics. One of the principles dealing with peas is that 75 per cent of the offspring of parents that are hybrids of a pure white-flowered strain and a pure red-flowered strain will have red flowers and 25 per cent will have white ones. This statistical law, Hempel holds, can be used in an I-S explanation to explain or predict "the approximate percentages of


\textsuperscript{31}Ibid.
red- and white-flowered plants in the sample." However, Hempel is not saying that the law can be used to explain why an individual pea plant's flower is red or white; it can only explain the percentages. But this claim is just what Brodbeck has been saying: only frequencies or percentages can be explained statistically. Thus Hempel's claim is definitely not established by this example and his interpretation of it. The deductive-statistical form (D-S) (Brodbeck's interpretation) of the explanation would be:

For any x, if x is the offspring of parents that are hybrids of pure red- and white-flowered pea plants (H), then x will have 75% red-flowered plants and 25% white-flowered plants.

a is H.
a has 75% red-flowered plants and 25% white-flowered plants.

Hempel's next example is taken from the statistical laws of radioactive decay of the elements. The I-S argument presented by Hempel is the following:

1. For any x, if x is an atom of radon, then the probability for x to decay radioactively (lose mass) within 3.82 days is 1/2 (statement of radon's half life).
2. "The decay of different radon atoms constitutes statistically independent events." (This means that if the probability of x decaying in 3.82 days is 1/2, in 7.64 days it is 1/4.)
3. The atoms of a sample of radon have together a mass of 10 milligrams, and the number of atoms in this sample exceeds $10^{19}$.
4. In 7.64 days the atoms of the sample will have together a mass between 2.4 mg. and 2.6 mg. (1/4 of 10 mg.)

According to Hempel, the explanans in conjunction with mathematical probability theory, entails that the explanandum is extremely highly

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32 Ibid., p. 391.
33 Ibid., p. 392.
probable. He holds that on the basis of the explanans it is rational to expect the explanandum. But to rationally expect something is not necessarily to rationally explain something. We expect the explanandum because the explanans entails its high probability. One might say that the explanans enables us to predict the explanandum because of its entailing the high probability of the explanandum. But expecting or predicting the occurrence of an event is not the same as explaining it.

Hempel's third example concerning Brownian movement, though different in content from the second, makes basically the same point as the radon example. Since he does not examine it in detail, I shall not either.

Hempel's second major argument for I-S explanation is based on recent mathematical theory of statistical probability. Such a theory is supposed to explain the statistical aspects of random processes or random experiments. By a random experiment Hempel roughly means:

a kind of process or event which can be repeated indefinitely by man or by nature, and which yields in each case one out of a certain finite or infinite set of "results" or "outcomes" in such a way that while the outcomes vary from case to case in an irregular and unpredictable manner, the relative frequencies with which the different outcomes occur tend to become more or less constant as the number of performances increases.35

Examples of such frequencies were those presented by the cigarette smoking example, pea flower example, and radon atom decay. The peculiarity that arises in this analysis, over and against D-N model, is

34 Ibid.
35 Ibid., p. 386.
that the explanandum is said to be expected in accordance with the frequency determined by the explanans. Furthermore, in examining some of Hempel's formulations of the frequency interpretation of statistical probability, we find that they are expressed on the basis of how certain we are that an individual event will occur, given a random experiment and a statistical law connecting the experiment with the occurrence of the event by a certain frequency. The important point to note in all of Hempel's attempted justifications of the I-S model is that he only shows that the event to be explained is to be expected or will occur in accordance with a certain frequency. Such phrases as 'it is to be expected' or 'it will occur' are indicative of predictions, not explanations. Hempel's further argument that people will eventually become satisfied with I-S forms as explanations tends to indicate that he thinks predicting on the basis of an I-S form is explaining on the basis of an I-S form. Arthur Pap is also of this opinion: in presenting the schema of statistical explanation, he holds that the event described by the explanandum is to be expected with a certain probability. It seems, then, that statistical explanation and statistical prediction are the same.

But what about having a statistical explanation of an event that has already occurred? In giving an explanation of it one presents statistical evidence which shows why the event was to be expected; i.e., that the event could have been predicted earlier if one had available the information contained in the explanans. But since showing that

\[\text{Ibid.}, \ p. \ 389.\]
\[\text{Pap, op. cit.}, \ p. \ 346.\]
something has a high probability seems to be the same as showing that it was to be expected, and conversely, then it would seem that the D-S model also shows what to expect since it deduces the probability or frequency of events. Let us consider, again, the argument forms of I-S and D-S:

**I-S**

\[
\begin{align*}
p(G,F) & \text{ is high} \\
i & \text{ is an } F \\
i & \text{ is a } G
\end{align*}
\]

**D-S**

<table>
<thead>
<tr>
<th>(x) is an (F)</th>
<th>(p(Gx)) is high</th>
</tr>
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<tbody>
<tr>
<td>(x) is an (F)</td>
<td>(p(Gx)) is high</td>
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As we have seen Hempel believes that the explanans of I-S shows the explanandum event is to be expected with a high probability. However, the D-S argument explicitly states what Hempel holds is confirmed by the I-S model. Is there, then, any reason for using the I-S model rather than the D-S, especially since the D-S model seems to make explicit what the I-S attempts to show? If by explaining an event statistically is meant showing why it is probable or to be expected, then it seems that the I-S model has no specific virtue over the D-S model; in fact, the converse is true. Furthermore, since the relationship between the explanans and the explanandum is inductive, one cannot explain inductively anything more than what is likely or to be expected. But the D-S explains the latter deductively. In summary, then, it has been found that whatever Hempel believes to be explained by the I-S model can be explained explicitly by the D-S model.

Before leaving statistical explanations there is a problem that sometimes arises with the I-S model. The I-S model is a type of statistical syllogism and such arguments have been known to yield contradictory conclusions from true premises. Thus, from 'Most thirty-
year-old people survive for five more years' and 'Joe is a thirty-year-old person' we may inductively infer 'Joe will survive for five more years.' Let us assume that the premises are true. Furthermore, let us assume that Joe has lung cancer and that most people with lung cancer do not survive for five more years. Thus, from 'Most people with lung cancer do not survive for five more years' and 'Joe has lung cancer' we may inductively infer 'Joe will not survive for five more years.' This latter conclusion conflicts with the former. Hempel's comment on such inconsistencies is that they are peculiar to inductive arguments: the premises are true yet both conclusions cannot be true. On the other hand, if the premises of two deductive arguments were true and both have valid argument-forms, then it is not logically possible for them to have inconsistent conclusions. If we interpret 'most' as 'high probability,' then the two statistical syllogisms may be formulated in the D-S model as the following:

(i): For any x, if x is a 30 year old person, then the probability of x surviving 5 more years is high.

Joe is a 30 year old person
The probability of Joe surviving 5 more years is high.

(ii): For any x, if x is a person with lung cancer, then the probability of x not surviving 5 more years is high.

Joe is a person with lung cancer.
The probability of Joe not surviving 5 more years is high.

Note that the conclusions of (i) and (ii) are inconsistent. However, both (i) and (ii) have valid argument-forms, and both are deductive. Since we have assumed as true 'Joe is a 30 year old person' and 'Joe is a person with lung cancer,' then it would seem that the universal

hypothesized of (i) or (ii) is false. But these hypotheticals are my
way of interpreting the statistical hypotheses of the above two sta-
tistical syllogisms. Is it the case that my interpretations are incor-
rect? Let us consider 'Most thirty-year-old people survive for five
more years.' Using Hempel's symbolic schema and the above substitu-
tion for 'most' we acquire: the probability of all thirty-year-olds
surviving for five years is high ('p(S,T) is high', where 'T' stands
for being a thirty-year-old and 'S' stands for surviving for five years).
Is there any significant difference between that probability statement
and the universal hypothetical of (i)? Both statements in conjunction
with the same statement of initial conditions will enable the same con-
clusion to be inferred: 'the probability of x (whoever he is) surviv-
ing five more years is high.' Consequently, there does not seem to be
any significant difference between them.

Since the same statements of initial conditions appear in both
the I-S model and the D-S model and are assumed to be true, then the
problem does not seem to be with them. Let us reconsider, then, the
I-S example. Due to the clash between the I-S examples, a new statis-
tical law emerges, viz., 'Most thirty-year-old people with lung cancer
do not survive five more years.' The interpretation is: 'For any x, if
x is a thirty-year-old person with lung cancer, then the probability of
x surviving five more years is not high.' This latter law like the law
of (ii) will yield a conclusion inconsistent with (i). Nevertheless
the new hypothetical statistical law will be the one most people will
want to accept as being the proper one for this context. In that case,
is the law of (i) false? (Remember that the statistical laws of the
conflicting I-S models are held to be true.) We could say that it is false or inapplicable when our information about the initial condition (in other words, the context) can be used to yield a contradictory conclusion on the basis of another law. When such inconsistencies arise, at least one of the laws is false or inapplicable with regard to the context. In determining which one is false, new laws will have to be tested which will yield the observed results. Those laws which are confirmed will be accepted as applicable to the situation while the disconfirmed ones will be rejected from this context.

In justifying my claim and defense of the D-S model reconsider the laws of (i) and (ii). Law (i) entails (iii) 'For any x, if x is a thirty-year-old person with lung cancer, then the probability of x surviving five more years is high.' Law (ii) entails (iv) 'For any x, if x is a thirty-year-old person with lung cancer, then the probability of x surviving five more years is not high.' As we have noted, the latter derived law (iv) will be the one found acceptable upon experimentation; law (iii) will not be. Notice, too, that the acceptable law can be deduced from one of the formulations of the D-S model. Thus under conditions expressed by the statements 'Joe is a thirty-year-old person,' 'Joe has lung cancer,' the confirmed laws are (ii) and (iv); the disconfirmed are (i) and (iii).

However, in the I-S model, the statistical law from which (i) was derived is of no use (because of the other statistical law and evidence of initial conditions). Given the conjunction of their premises, conflict in evidence arises. This conflict is presented in the D-S model as contradictory probability claims. Whether the law of (i) is false
or useless, it is certainly inapplicable to the individual under investigation, because of the additional information. From all the additional information about the individual and its kind, a new law can be deduced that will enable the D-S model to proceed in its explanation. At most, then, law (i) is false in the light of our additional evidence of Joe including that evidence which enables us to formulate (iv).

The conclusion of this analysis is that (1) my interpretations of the statistical laws are correct, but (2) the interpreted statistical laws (as well as those of the I-S model) are applicable only for certain contexts: those in which the available evidence does not yield inconsistencies. When inconsistencies result, at least one law is false or inapplicable to the context being investigated, and the new statistical laws have to be formulated in order to determine which has the most justified probability, e.g., that the probability of Joe surviving is high or the probability of Joe not surviving is high.

I shall now summarize the points in this section on explanation. If we assume that there are two justifiable forms of scientific explanation: the D-W model and the I-S model, then, provided my interpretation is correct, the I-S model can be set up in terms of the D-S model which expresses more clearly just what the I-S model is supposed to explain, viz., the probability of a certain event happening, not the occurrence of the event itself. Thus it would seem that all explanation can be construed as deductive: either with a universal law or a statistical law of the form for which I have argued. This view seems to be in agreement with Braithwaite, Brodbeck, and Popper. I have not, however, even begun to argue for Popper's view that all reasoning or
inference is deductive. All that I have argued is that, if explanation has the form of an argument or inference, then it can be deductive. The deductive version of the I-S does not presuppose anything not presupposed by the I-S model; in fact, the D-S will presuppose less if inductive arguments all presuppose some sort of general or universal principle of the uniformity of nature.

The covering-law aspect of Hempel's models was kept in order to avoid having a trivial explanation. There have been many articles in the literature about the logical requirements for D-N explanation maintaining that non-scientific explanations fail to meet all of the requirements for the D-N model. Even though some of these articles have been critical of the D-N model and its requirements, I shall accept the requirements as being at least logically necessary for a scientific explanation, and I shall accept the D-N and D-S models as being sufficient for scientifically explaining anything.

Section III

The Logic of Prediction for an Individual Event or Process

In their article on scientific explanation, Hempel and Oppenheim state that their four necessary conditions for scientific explanation also apply to scientific prediction.\(^{39}\) Hempel and Oppenheim argue that the difference between scientific explanation and scientific prediction is pragmatic. If the event described by the explanandum has

occurred, then the explanans is used to explain the event; however, if
the event described by the explanandum has not yet occurred, then the
explanans is used to predict the event. Consequently, whatever logical
conditions hold for explanation also hold for prediction. Thus, if all
scientific explanation can be construed as deductive, then all scienti-
tific prediction can also be construed as deductive. This view of
Hempel and Oppenheim's is called the thesis of the structural symmetry
or identity between scientific explanation and prediction (identity
thesis, for short). As Hempel puts it in a later study:

the phenomenon . . . is explained, and thus understood, by
showing that its occurrence was to be expected under the
specified circumstances in view of certain general laws. . . .

The specified circumstances are described by the statements of initial
conditions. What Hempel says here should be compared with what both he
and Pap said in defending the I-S model of explanation: it explains
by telling us what is to be expected or what is more probable. Thus,
especially in his defense of the I-S model, we observed the symmetry if
not the identity of explaining an individual event and predicting it.
We can explain any event whatsoever that has occurred if we know the
law(s) describing events of its kind and applicable to events of the
kind described in the statements of initial conditions. We can predict
any event whatsoever that has not yet occurred if we know the law(s)
describing events of its kind and applicable to events of the kind
described in the statements of initial conditions. This temporal dif-
fERENCE between explaining an event and predicting it is what Hempel

and Oppenheim mean when asserting that explanation and prediction differ only pragmatically.

The symmetry or identity thesis has been formulated by Hempel into two sub-theses:

(i) that every adequate explanation is potentially a prediction . . . ; (ii) that conversely every adequate prediction is potentially an explanation. ¹

As a criticism of the identity thesis, Israel Scheffler has noted that a prediction is "simply the assertion of something about the future." ²

However Scheffler's notion is misleading; not all statements about the future are predictions. The principle of induction, viz., the future will resemble the past, is not usually uttered as a prediction. What Scheffler wants to say is that a prediction is merely a statement to the effect that some particular event or process is going to happen. An explanation, on the other hand, according to Scheffler, does require general principles such as laws that describe connections between phenomena. Thus it would seem that a prediction is merely one statement that has a temporal reference and an explanation is a set of statements in which no temporal reference is necessary. Hence scientific predictions and explanations are structurally divergent.

Hempel's reply to Scheffler's criticism is that the identity thesis holds between explanatory arguments and predictive arguments (justifications for predictions in Scheffler's sense). Hempel holds that the arguments in question are either of the D-N or I-S type.

¹Hempel, p. 367.

Other criticisms of the identity thesis can be considered by examining the two sub-theses separately. Let us consider criticisms against the first sub-thesis. According to Stephen F. Barker, Darwin's theory of evolution explains why the plants and animals exist as they do now, but we would not have been able to predict in advance that they would exist as they do now. He also cites some explanations of Freud for explaining the hysterical behaviour of a patient as well as Pirenne's theory for presumably explaining why feudalism arose in Europe. Yet neither would enable us to predict beforehand any of those things which they explain. Another example of Barker's is that of a coroner who reports to us that a woman died because she was bit by a wasp, but "that she was stung by a wasp would not have enabled us . . . to predict . . . that she was going to die as she did." Stephen Toulmin also uses Darwin's theory of evolution as an example of a theory that has explanatory import, but initially no predictive import. Toulmin likewise uses this example against what he calls the first predictive thesis, viz., that the explanatory power of a theory is its predictive success. Since both Toulmin and Barker believe that Darwin's theory of evolution is a counter-example to sub-thesis (i), let us consider Hempel's reply.


Hempel attempts to distinguish between the story of evolution and the theory of evolution.\textsuperscript{46} By the story of evolution he understands the description or the narrative of the different evolutionary stages. The story tells us when certain organisms evolve, possibly from what other organisms, and when those organisms became extinct. The story does not, however, explain why the organisms came into existence nor why they became extinct. It is only recently that advances in genetic theory of mutation have enabled some predictions that tend to support the theory of evolution.\textsuperscript{47} Hempel holds that these predictions are too inconclusive as yet. Furthermore, he argues that in order to explain the extinction of, say, dinosaurs, we need to know both their physical and biological environment as well as "the species with which they had to compete for survival."\textsuperscript{48} Hempel argues that if these factors were known, then they, in combination with Darwin's theory of natural selection, would provide us with at least an I-S explanation of the extinction of dinosaurs. But such knowledge would have enabled us to predict the dinosaurs' extinction if we had been living then. In light of this distinction, Hempel holds that Toulmin has identified the story of evolution with its explanation in attempting to refute the first predictivist's thesis.

Hempel's criticism of evolution as a counter-example to the identity thesis can be used mutatis mutandis against Barker's use of Freud's and Pirenne's so-called theories as other counter-examples.

\footnote{\textsuperscript{46}Hempel, "Aspects of Scientific Explanation," p. 370.}
\footnote{\textsuperscript{47}Ibid.}
\footnote{\textsuperscript{48}Ibid.}
The question before us now is, Who is correct? A careful reading of Hempel shows that what the scientists are lacking with regard to evolution are true statements of the initial conditions that preceded the origin or extinction of an organism. Darwin gave us a general law which is more or less identical to the story of evolution. Besides that law, the theory of evolution will contain true statements of initial conditions and other more specific laws that will enable us to explain or predict, at least, the probability of a species' or organism's origin or extinction. The story of evolution may psychologically satisfy many people as to why certain animals are living now and others have become extinct, but that does not constitute for Hempel a scientific explanation.

Barker's wasp example will now be examined. Hempel would reply to it by saying that being stung by a wasp is not sufficient to explain the woman's death because most such cases are not fatal. The coroner (assuming that he is competent and reputable) has probably found that the woman was severely allergic to wasp stings. Thus we now have a covering-law explanation to the effect that most, if not all, people who are severely allergic to wasp stings die if they are stung by a wasp. Had we known all of the initial conditions with respect to the woman, we could have predicted her death. To put the point more formally, consider the two following arguments:

(1) Jane is bitten by a wasp at time $t$. 
   Jane will die at time $t + 1$.

(2) All people who are severely allergic to wasp stings die from wasp stings if bitten by a wasp earlier. 
   Jane is severely allergic to wasp stings. 
   Jane is bitten by a wasp at time $t$. 
   Jane will die at time $t + 1$. 
Now in the situation that Barker places us, anyone, even the coroner, is not justified in making inference (1). However, upon examination the coroner discovers the allergic symptoms and because of his knowl­edge could have made inference (2) had he known of Jane's being allergic to wasp stings. By confusing (1) and (2), Barker believes he has refuted the identity thesis.

Some of Barker's comments could easily be interpreted to suggest that we construe adequate explanation as any statement of fact that psychologically satisfies the individual who asks for an explanation.\(^9\) Such a view of explanation does not require the covering-law model which Barker holds is a groundless dogma.\(^5\) Of course, any statement, no matter how fantastic it be, can be used to psychologically satisfy many individuals about some matter-of-fact: references to gods, grem­lins, animal magnetism, and orgonic force, are such examples. Barker is, therefore, required to distinguish between a reasonable explanation and an unreasonable one. A reasonable explanation would be one founded on experience and justified by some accepted rule of inference. As an example, he cites someone asking why Smith is a teetotaler.\(^5\) The reply (the explanation) is that Smith is a vegetarian and Blake, Jones, and Brown are both vegetarians and teetotalers. (The rule of inference used here is analogy.) Since we can publicly observe these individuals and the properties of being a vegetarian and being a teetotaler,

\(^5\)Ibid., p. 360.
\(^5\)Ibid.
Barker's analysis would eliminate occult entities as enumerated above in reasonable explanations. Note that the explanation does not make any reference to a universal or statistical law. Furthermore, on the basis of knowing that Blake, Jones, and Brown are both vegetarians and teetotalers, and also that Smith is a vegetarian, we could predict (by analogy) that Smith will be found to be a teetotaler. Thus reasonable predictions would not need to involve universal or statistical laws.

Who, then, is correct: Barker or Hempel?

It is undoubtedly true that explanations and predictions have been given as Barker presents them. Nevertheless, it is also true that explanations and predictions have been given as Hempel presents them. Is the issue between them a verbal issue? Of course, it is possible either to add premises to Barker's example or to modify insignificantly the given premises to meet the D-N, or I-S or D-S models. But then by a reverse process these models can be converted by an elimination of premises into Barker's model of analogical explanation and prediction. In evaluating these two opposing views of explanation, let us consider their major disagreement: the use of laws in explanation and prediction. Why use laws? Because it is the law which connects the statements of initial conditions and the statement describing the event to be explained (the prediction). Barker's possible reply is that only the analogical rule of inference is necessary to connect the statements in question to each other. Scheffler also presents in some detail the argument I am drawing from Barker's example against Hempel's view.\footnote{Scheffler, The Anatomy of Inquiry, pp. 42-43.}
First of all, an adequate law must have been confirmed. Let us say that our law has the form \((x)(Fx \supset \exists Gx)\). Furthermore, \(Fa \cdot Ga\), (individual \(a\) is an F and \(a\) is a G), \(Fb \cdot Gb\), and \(Fc \cdot Gc\) confirm \((x)(Fx \supset \exists Gx)\). Let it be the case that we can easily determine whether an individual has F independently of whether it has G. We know that besides \(a\), \(b\), and \(c\) having F that \(d\) also has F. Therefore, we predict that \(d\) also has G. This prediction can be made in one of two ways:

D-N model:
For any \(x\), if \(x\) is an F, then \(x\) is a G.
\[
\begin{array}{c}
\text{d is an F} \\
\hline
\text{d is a G},
\end{array}
\]

or the Inductive Analogical model:
individuals \(a\), \(b\), \(c\) and \(d\) are F's.
individuals \(a\), \(b\), and \(c\) are G's.

If our evidence for "For any \(x\), if \(x\) is an F, then \(x\) is a G" is \(Fa \cdot Ga\), \(Fb \cdot Gb\), and \(Fc \cdot Gc\), then one wonders whether these two models constitute two different ways of presenting the same information. While Barker argues that predictive and explanatory arguments can both have the inductive analogical model (I-A), Scheffler argues that only predictive arguments can have the I-A model (he still accepts the covering-law model for explanatory arguments), and Hempel argues against models like I-A for both predictive and explanatory arguments.

In determining which position is correct we should consider the significance of the issue. There are several views of theories which hold that a theory is a deductive system, and the traditional view which has been serving as our model of analysis and critical evaluation is such a view. On the other hand there are views, such as probabilistic realism, which hold that a theory is an inductive system. But this
point of difference is not the only significant point. Notice that if the I-A model is as useful as the D-N model, then we seem to have found a procedure in which laws are not logically necessary for a scientific prediction and explanation. Such a view will be appreciated by the descriptivist and instrumentalist because the statements of the I-A model contain only logical and observational terms. It may, then, be the case that theoretical terms are superfluous in scientific reasoning. It is especially this latter point that is of significance for the realist-nonrealist controversy.

The I-A model has replaced the law with additional statements of initial conditions (or positive analogical instances) and the analogical rule of inference. It would seem, then, that the law from the I-A point of view is a rule of analogical inference. Now Hempel does consider the view of laws as inference rules, but classifies them only as material rules of inference. Yet the analogical rule of inference of the I-A model is a formal rule of inference. Thus Hempel's criticisms do not directly apply to the I-A model. They may, however, be shown to apply indirectly; or it may be the case that material rules of inference can be replaced by formal rules without re-instating the law as a premise in the argument. If the latter is the case, then Hempel's argument would fail against the view under investigation. Let us first consider Hempel's criticisms.

Hempel grants that laws of the form '(x)(Fx \rightarrow Gx)' can be replaced by material rules of inference of the form "'Gx' is inferable from 'Fx,'" but most scientific laws have a form more complex than the above
one, and this fact renders dubious any such replacement. Take, for example, the law that every metal has a specific melting point:

for every metal there exists a temperature T such that at any lower temperature and at no higher temperature the metal is solid at atmospheric pressure.

Letting 'Mx' stand for 'x is a metal,' 'Tyx' for 'x has temperature y,' 'S' for 'solid at atmospheric pressure,' '...<--' for '... is lower than --,' and '...>---' for '... is higher than --,' the form of the above law is:

\((x) \{Mx \rightarrow (\exists y) \left[ (Tyx) \cdot \left( z \left( Tzx \Rightarrow \left[ (TzxTyx) \parallel Sx \right] \cdot \left[ (TzyTyx) \parallel -Sx \right] \right) \right] \} \)\)

Hempel argues that the material rule of inference derived from this law would have to be something like the following: "from 'a is a metal' one may derive 'there exists a temperature T such that at any lower temperature and at no higher temperature a is solid at atmospheric pressure.'" But the latter part of the rule of inference is a statement involving both existential and universal quantifiers which have the form of laws. If one attempts to set up a rule of inference for the latter part of the above rule of inference, it will be found that it cannot be done because of the existential quantifier. Only those laws which are exclusively universal can be construed as rules of inference, but not all laws are exclusively universal. Thus Hempel believes that he has refuted the claim that laws are material rules of inference.

At this point I want to consider two things: (1) whether laws of the above form fit into the D-N model and (2) whether the I-A model has

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54 Ibid.
been affected by his criticism. I do believe that Hempel has shown that laws of the basic form \( (x)(Fx \supset (\exists y)(Gy \cdot Hyx)) \) cannot be successfully construed as rules of inference. However, is it the case that such laws can be used in predictive and explanatory D-N arguments? From 'a is an F' and the above law we can deduce a statement of the form \( (\exists y)(Gy \cdot Hya) \). But what kind of a prediction or explanandum is this? It is not a statement of any individual event. It simply states that there is some individual or other \( y \) which has \( G \) and \( a \) is related by \( H \) to \( y \). Nothing definite is explained or predicted by this law even given as true the statement of initial conditions whose form is 'a is an F.' Statements of the form \( (x)(Fx \supset (\exists y)(Gy \cdot Hyx)) \) are said to be neither completely verifiable nor completely falsifiable (assuming that statements of individual events are completely verifiable and are completely falsifiable, and statements of the form \( (x)(Fx \supset Gx) \) are completely falsifiable but not completely verifiable). It would seem, then, that laws of this form are extremely close to metaphysical assertions if by a metaphysical assertion is meant any non-analytic statement that cannot be completely falsified.

Popper, however, uses a law in making a prediction—a law that seems to have the form of laws Hempel is considering. Popper's predictive argument is as follows:

'For every thread of a given structure \( S \) . . . there is a characteristic weight \( v \), such that the thread will break if any weight exceeding \( v \) is suspended from it.'—'For every thread of structure \( S \), the characteristic weight \( v \), equals 1 lbs.'

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55Popper, The Logic of Scientific Discovery, p. 60.
Then if we know that the thread we are investigating is of structure \( S \), and the weight suspended from it is 2 lbs., then by means of the above two laws and the statements of initial conditions we can predict that the thread will break. Popper is correct in saying that the prediction deductively follows. Let us, however, examine more closely how the laws enabled the prediction to be deduced. The two laws in question can be symbolized as:

\[
(1): (x)\left( S_x \implies (\forall y)\left( W_y \implies (z)\left( W_z \land G_{zy} \implies B_{zy} \right) \right) \right)
\]

\[
(2): (x)\left( S_1 \implies (W_a \implies (z)\left( W_z \land G_{za} \implies B_{za} \right) \right) \right)
\]

where '\( S_x \)' stands for ' \( x \) has structure \( S \),' '\( W_x \)' stands for ' \( x \) is a weight,' '\( G_{xy} \)' for ' \( x \) weighs more than \( y \),' and '\( B_{xy} \)' for ' \( x \) breaks \( y \).'

Now one of the statements of initial conditions is 'the weight suspended from the thread is greater than the thread's characteristic weight,' which is symbolized as '\( W_a \implies (W_b \land G_{ba}) \)' where \( a = 1 \) lb. and \( b = 2 \) lbs.; and the other statement of initial condition is 'this is a thread of structure \( S_1 \),' which can be symbolized as '\( S_1 \)' Thus, symbolically, '\( B_{bc} \)' follows deductively. But what role did law (1) play in the deduction? None whatsoever. Only (2) in conjunction with the above statements of initial condition entail the prediction. In fact (1) does not even entail (2) although it would provide inductive evidence for (2). On the other hand (2) has only universal quantifiers and like laws of the form '(\( x \))(F_x \implies G_x)' it is falsifiable.

Hempel considers the possibility of having two laws of the form '(\( x \))(F_x \implies (\exists y)R_{xy})' and '(\( x \))(\( \exists y \))R_{xy} \implies G_x).'\(^6\) It may be the case that

an explanatory or predictive argument contain laws having these forms which makes possible the explanation or prediction of an individual event. The reason why it is possible is because any two laws of the above forms entail by hypothetical syllogism another law of the form \((x)(Fx \supset Gx)\). From a law of the form \((x)(Fx \supset Gx)\) and a statement of initial condition having form \(F_a\), a statement describing the explained or predicted event of the form \(G_a\) deductively follows. It would seem, then, that laws of the more complex form can be used to yield explanations and predictions of individual events. Yet that conclusion is not quite correct. First of all, if the only law or universal assertion in the explanans is of the form \((x)(Fx \supset (\forall y)Rxy)\), then no such explanation or prediction follows even if we know that an individual has \(F\). Consequently, only if we have two or more laws of the form Hempel considers above, can we use them in making such explanations and predictions. But they can be used to make such explanations and predictions only because they entail a law of the form \((x)(Fx \supset Gx)\).

With regard to individual events, then, their explanatory and predictive import is logically equivalent to the explanatory and predictive import of laws having the \((x)(Fx \supset Gx)\) form. In conclusion, therefore, we have found that Hempel's critique of the view that laws are material rules of inference, though sound, can be turned against Hempel's view itself. One cannot generate rules of inference from statements of the form \((x)(Fx \supset (\forall y)Rxy)\), and one cannot use them singly to predict or explain individual events.

How does the I-A model fare with regard to these issues? First of all, the I-A model does not hold laws to be material rules of
inference. Secondly, is it possible to use analogy to eliminate laws of the form '(x)(Fx\exists y Rxy)'? I think that it definitely has been shown that laws of the form '(x)(Fx\exists Gx)' can be eliminated by analogical inference in predicting and explaining individual events. Let us consider how an advocate of I-A model would analyze Popper's thread example. First, evidence must be gathered. We have examined three threads of the same structure and found that they break when the weight suspended from them begins to exceed one pound. Next we observed that there is a fourth thread having the same structure as the other three. By analogy, we predict that this fourth thread will break when the weight suspended from it begins to exceed one pound. Thus, again, it appears that laws are not necessary for making predictions. Consequently, it seems that any individual event that can be explained either by means of the D-N model or the I-S model can also be explained by the I-A model. Of course whether or not everything can be explained by means of the I-A model is another question. (A rigorous criticism against the I-A model and all other inductive processes will be examined under the heading, the theoretical-observational distinction, in Chapter IV, below.)

The preceding discussion arose out of Hempel's defense of the first sub-thesis of the identity thesis. I believe that Hempel has successfully shown that an adequate explanation is potentially a prediction. This sub-thesis can be held regardless of which model one accepts: D-N, I-S, D-S, or I-A. Hempel does have some doubts, however, with respect to the second sub-thesis, namely, that every adequate prediction is potentially an explanation. He examines the
predictive argument by analogy which both Scheffler and Barker hold as being a justified predictive inference. Hempel notes that according to some theories of probabilistic inference, such as Carnap's, it is logically permissible to infer from statements of observed phenomena to statements of as yet unobserved phenomena without the use of any laws.\textsuperscript{57} But, conversely, Hempel also notes that there are certain theories of statistical probabilistic inference which hold that such an inference is justified only on the assumption of a statistical law guaranteeing randomness of experiment such as appears in the I-S model.\textsuperscript{58} If the first type of theory is correct, then the second sub-thesis is false with regard to the covering-law model. But if the second type of theory is correct, then so is the second sub-thesis (i.e., every predictive argument is a potential explanatory argument). Because the issues between these two types of theories of probability have not been successfully clarified to date, Hempel concludes this discussion by saying that it is still an open question whether the second sub-thesis is correct.

In commenting on Hempel's reply, it should be noted that the first theory of probabilistic inference confirms the I-A model whereas the second theory confirms the covering-law view. If all I-S models are actually D-S models, then the controversy between these two theories of probability seems to be ultimately whether there is any justified inductive inference (definitely held by the first theory). Although I do not wish to become involved in that issue, I shall explore in the

\footnotesize{\textsuperscript{57}\textit{Ibid.}, pp. 375-376.  
\textsuperscript{58}\textit{Ibid.}, p. 376.}
following some problems arising which may decide the issue between the
deductive and the inductive models.

We have been considering the D-N model as a predictive argument. Let us now consider the I-S model as one. Hempel argues that if our explanans is set up in consistency with the knowledge of the phenomena under investigation (to eliminate as far as possible inductive inconsistencies), then it would be rational to expect the occurrence of the event described by the explanandum if it is supposed to occur, but has not as yet, in accordance with the inductive support of the explanans. But the event is predicted only with probability. Thus in the D-S model, what would be predicted is that an event has a certain probability, given the explanans as true. It would seem that if one can justify the identity thesis under the D-N model and for events for which we have deterministic or nonstatistical evidence, then one can justify the identity thesis under the I-S (and D-S) model and for events for which we have statistical evidence. Such does seem to be the case.

For example, N. R. Hanson cites what he takes to be a counterexample:

it can be explained why atoms in a cluster of carbon14 will decay, but it cannot be predicted which one(s) will decay in a given period of time.\(^5\)

But to this criticism of Hanson's, Hempel replies that the reason why one cannot predict which atom will decay is due to the law which describes their behavior: it is a statistical law. Thus one can

predict only the probability with which an atom will decay. But since the law is a statistical one, all that anyone will be able to explain is the probability (or the number of decaying atoms over total number of atoms in carbon$^{14}$) for any given time. Perhaps Hanson also has confused a story with its theory, i.e., the story of radioactive decay with the statistical explanation and prediction of such decay. It appears that Hempel's reply is justified.

We have previously observed how the I-A model can be used in place of the D-N model. The question now is whether it can replace the I-S model or the D-S model. I shall, in my analysis, consider only the argument forms for these models. The argument form for I-S is:

\[
p(G,F) \text{ is high} \\
\bar{i} \text{ is } F \\
\bar{i} \text{ is } G.
\]

The comparable I-A form might be:

\[
a, b, c, d, e, h, \text{ and } i \text{ are } F. \\
a, b, c, \text{ and } d \text{ are } G \\
e \text{ and } h \text{ are non-}G \\
i \text{ is } G
\]

Is the latter form genuinely comparable to the former? For a statement of the form \(p(G,F) \text{ is high}\) to be used justifiably, one must have tested a sample of individuals that were \(F\)'s and found them to be \(G\)'s. Such findings are represented by the premises of the I-A model. It would seem then that any prediction or explanation made by the I-S model can also be made by the I-A model.

Let us now consider the D-S model. The argument form for D-S is:

For any \(x\), if \(x\) is an \(F\), then \(p(Gx)\) is high.

\[
i \text{ is an } F. \\
p(Gi) \text{ is high.}
\]
The comparable I-A might be:

\[
\begin{align*}
& a, b, c, \ldots, h, \text{ and } i \text{ are } F, \\
p(G_a), \ p(G_b), \ p(G_c), \ldots, \text{ and } p(G_h) \text{ are high} \\
p(G_i) \text{ is high.}
\end{align*}
\]

It seems to be the case that the I-A model again is a good comparison of the D-S model. If one wonders how 'p(Ga),' for instance, is to be determined, it is to be determined on the basis that a is an F and that the ratio of F's found to be G's to all F's in a sample was found to be high. In short, it is determined in exactly the same way as determining any statement of the form 'p(G,F) is high.'

The important question remaining is whether the universal and statistical laws have been eliminated by the I-A model. Upon examination, it may seem that such elimination is illusory. The statistical laws, whether of the I-S model or the D-S model, have as their evidence the premises of their comparable I-A model. In other words, the evidence that an advocate of the D-S or the I-S model has for his law is exactly the statements of the premises in the I-A model. Laws are not, however, merely summaries of their evidence. All evidence is a set of statements about past or (at least) present events. Laws refer to events past, present, and future. And it is because of such total temporal reference that they provide the link between the past and present (statements of initial conditions) and the future (predictions). But, then, if laws are lacking in the I-A model, how does it provide the link between the evidence and predictions? The only answer is to be found in the rule of inference itself: the analogy inference. Thus the evidence, together with the analogical rule of inference, appears to be logically equivalent to the universal and statistical law.
Hence the difference between the I-A model and all the others seems to be illusory.

An argument might be brought forward in an attempt to show that this difference is not illusory. For example, in making an inference by using the D-N model or the D-S model, sometimes our conclusion (especially if it is a prediction) is found to be false. Since the statements of initial conditions are true (they have been verified empirically) and the deductive rule of inference is tautological, then the falsity of the conclusion can only be explained by the falsity of the law. Let us assume that we have inferred the same conclusion by means of the I-A model. Now the I-A model also contains the statements of initial conditions contained in the D-N model or the D-S model. The other premises of the I-A model are those statements of evidence for the laws contained in the D-N model and the D-S model. These statements are also true. Since the premises of the I-A model are true but its conclusion is false, then how do we account for this falsity? The analogical rule of inference cannot be construed as a hypothetical tautology. Could it be, then, what has produced a false conclusion from true premises? Apparently it is, since it is quite possible and has often been the case that analogical arguments have led to false conclusions from true premises. Shall we, then, reject the analogical rule? If we reject a law we can always replace the rejected one with a new law. But with what shall we replace the analogical rule? If we replace it with a deductive rule, then we would cease having an I-A model and we would have to add a law as a premise. If we replace it with another inductive rule, we would cease having an I-A model since
all the other inductive inferences require a law either as a premise (such as the I-S model) or as a conclusion (such as inductive enumeration). A further point with regard to replacing the analogical rule with another inductive rule of inference is that we will still have true premises and false conclusions some of the time. Thus the major difficulty would not have been avoided in such a replacement.

To this criticism (namely, that we would have to reject the analogical rule of inference) an advocate of the I-A model could reply that, in the deductive models, we only know when a law is false by testing it in accordance with the truth of the predictions that deductively follow from it and the statements of initial conditions. If the prediction is false, then the premises of our predictive argument must be modified. But this situation also holds with regard to the I-A model. If a prediction comes out false, then, instead of rejecting the analogical rule, that prediction becomes incorporated into the premises. From this new set of premises and the analogical rule, a new prediction is inferred. In this manner the I-A model can account for false predictions without appealing either to laws or to a different rule of inference. It seems that the I-A model can meet objections as well as any other models.

I want now to consider in detail some logical requirements necessary for making a scientific prediction. It might seem that the term 'scientific prediction' is redundant. How could a prediction fail to be scientific, especially if it turns out true? Scriven has noted, in agreement with Scheffler, that:

a prediction is what it is, simply because it is produced in advance of the event it predicts; it is intrinsically nothing
Adolf Grünbaum's criticism of Scriven is that a scientifically warranted prediction is something "more than a mere pre-assertion of the event." A prediction is scientifically warranted only by means of a predictive argument. There need be no controversy between Scriven and Grünbaum over this point, because Scriven is talking only about a prediction as an assertion and Grünbaum is talking about justifying a prediction. Both points seem to be correct. Now, all the models of predictive arguments which we have been considering have one thing in common which I take to be an essential characteristic for any scientific predictive argument: at least one premise is a statement about a past or present observed (or being observed) event. It is from such a statement either in accordance with a law and/or rule of inference that the prediction is inferred. To put it in other terms, one cannot have a justified predictive argument whose premises all refer to unobserved events. I do not mean merely that the premises cannot all refer to unobservable events; I also mean that they cannot all refer to unobserved events. At least one of the premises must describe an event or set of events which was observed or is being observed by someone or other though not necessarily by him who presents for predictive purposes a predictive argument. This condition for a justified prediction is, of course, not the only condition necessary, but it is probably the most important since all the models have definitely met it.

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60 Scriven, op. cit., p. 177.

A substantial amount of the material discussed thus far has dealt with either theories or laws. As noted previously the traditional view holds a theory to be a deductive set of statements that explain and predict phenomena (or statements describing the phenomena). We have also seen that there is disagreement between the advocates of the traditional view as to whether theories can also be inductive systems. Hempel, especially, has argued in favor of such a possibility; Brodbeck against it. We have also noticed that laws have been used, according to the traditional view, to explain and predict phenomena. Apparently, then, according to the traditional view certain laws, and the subject matter which they comprise, in turn comprise theories as axiomatic systems of laws. But what specifically are the characteristics of statements of scientific law? Let us consider some of the more important logical characteristics of laws. From examining the statements of law presented in Hempel's two models, it can be said that all laws, whether universal or statistical, assert an invariance between two or more properties. If all the members that have one property also have another, then the law stating such a complete or universal invariance between the members of those classes or properties is a universal law. On the other hand, if only a certain percentage (less than 100 per cent) of the members that have one property also have the other, then the law stating such a percentage invariance (i.e., that the number or percentage remains constant), is a statistical law. Thus a law is a statement that asserts either a total invariance or a statistical
invariance. If there is neither kind of invariance between two specific properties or classes, then there is no law describing such relationships.

Statements of invariance can be expressed as conditional statements; in fact, that is how I have interpreted both universal and statistical laws. Now at this point a serious problem arises. There are some laws, such as Newton's first law of motion, of which we do not know whether anything in the universe satisfies them. Furthermore, there are some laws that refer to ideal (non-existent) entities and conditions. As examples Pap cites frictionless motion in the principle of conservation of mechanical energy and isolated systems in the first law of thermodynamics. Because of our uncertainty about such laws (when expressed in conditional form) having anything satisfying their antecedents, such laws would become vacuous truths if construed as truth-functional material implications. In order to avoid such a trivialization of scientific laws, Pap holds that laws are to be construed as subjunctive conditionals. Thus, instead of stating 'For any \( x \), if \( x \) is a body moving freely from the actions of external forces, then \( x \) will move with a constant velocity,' we state, 'For any \( x \), if \( x \) were a body moving freely from the actions of external force, then \( x \) would move with a constant velocity.'

The question arises as to what assurance we have for accepting such subjunctive statements. Of course, there does not seem to be any problem if we have observed an instance of the antecedent. For instance,

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\(^{62}\)Pap, op. cit., p. 302.
the law, 'For any x, if x is copper, then x conducts electricity,' is known to have true instances such as 'this is a piece of copper and it conducts electricity.' Pap attempts to answer the above question by making a distinction between direct evidence for a law and indirect evidence for a law. By direct evidence or confirmation of a law, Pap means all the entities referred to by the law which have been observed have all the properties and relationships the law asserts of them. For example, the statement describing our observation that a specific piece of copper conducts electricity is direct evidence or confirmation of the derived law.

I believe that 'direct evidence' is clear enough, but some comments should be made about 'indirect evidence.' It is not clear whether the directly confirmed law should entail or inductively support by itself the other law for which we are supposed to have indirect evidence. Such inferences are usually trivial in any case. For instance, from a law of the form '(x)(Fx*Gx)' ('For any x, if x were an F, then x would be a G'), we can deduce without any other statements a law of the form '(x)(-Gx*-Fx).'

(Any preceding discussion of laws is independent of the view that laws are universal material implications or universal subjunctive conditionals.) Thus whatever would directly confirm the first law would indirectly confirm the second. But let us suppose that we also have another law of the form '(x)(Gx*Hx).'</p>

From the law of the form '(x)(Fx*Gx)' and the law of the form '(x)(Gx*Hx),' another law is entailed having the form '(x)(Fx*Hx).' Now since we have direct

\[63\text{Ibid.}, \text{ also p. 421}\]
evidence for the law of the form \((x)(Fx*Gx)\) do we have indirect evidence for the law of the form \((x)(Fx*Hx)\)? It may seem obvious that we would have indirect evidence for the law of the form \((x)(Fx*Hx)\) if we also had direct evidence for the other law of the form \((x)(Gx*Hx)\). However, such is not always the case. An observational statement of the form \('Fa*Ga'\) is direct evidence for the law of the form \((x)(Fx*Gx)\). And an observational statement of the form \('Ga*Ha'\) is direct evidence for the law of the form \((x)(Gx*Hx)\). However, the conjunction of these observational statements is also direct evidence for the law entailed by the other two: the one whose form is \((x)(Fx*Hx)\).

But if our evidence for the law of the form \((x)(Gx*Hx)\) were an observational statement of the form \('Gb*Hb'\) instead of the form \('Ga*Ha'\), then we would only have indirect evidence for the law conclusion.

Furthermore, if we had direct evidence for one of the laws and indirect evidence for the other, it would seem we could only have indirect evidence for their conclusion. But what if we have no evidence for one of the premises? Would we have any evidence for their conclusion? I believe that the best evaluation to draw is that we also do not have any evidence for the conclusion, regardless of whether our evidence for the other law is direct or indirect. A conclusion cannot be confirmed more than the least confirmed or non-confirmed premise from which it is derived.

Some other definitions of 'indirect evidence' are discussed by Nagel. According to one sense of 'indirect evidence,' we will have indirect evidence for a law if it is derived together with other laws

\[\text{Note:} \quad \text{Nagel, op. cit., pp. 64-65.}\]
from a set of laws, and we have direct evidence for the other laws.

The schema for such a situation could be the following:

\[ M \] (a set of laws) entails \( L_1, L_2, L_3, \) and \( L_4 \). \( L_1, L_2, \) and \( L_3 \) are directly confirmed. Therefore, what directly confirms \( L_1, L_2, \) and \( L_3 \) indirectly confirms \( L_4 \).

Although the above schema represents Nagel's first sense of 'indirect evidence,' one wonders if we would have indirect evidence for \( L_2, L_3, \) and \( L_4 \) if we only had direct evidence for \( L_1 \). I presume we would. The reason is as follows. Since we have direct evidence for \( L_1 \), then we would have indirect evidence for \( M \). If we have indirect evidence for \( M \), then we would also have indirect evidence for anything else derived from \( M \), for which we have no direct evidence.

The second definition of 'indirect evidence' is that we have indirect evidence for a law provided it enables us to derive other laws from it in conjunction with statements of initial conditions. For example, the law for freely falling bodies is derivable from Newton's laws of motion in conjunction with the gravitational constant \( g \). Agreeing with Nagel, Pap sums up his discussion of evidence and law by saying that:

a universal statement is lawlike to the degree that it is indirectly confirmed by instances that directly confirm more general hypotheses from which it follows or less general statements that follow from it.\(^{65}\)

Since there are several different senses of 'indirect evidence' it would be best to distinguish between them. I shall call that indirect confirmation, 'indirect-confirmation' \( 'I-C_1' \), in which a law is

\(^{65}\)Pap, op. cit., p. 302
said to be confirmed due to the fact that it is derived from another law which has been directly confirmed. I shall call that indirect confirmation, 'indirect-confirmation\textsubscript{2}' ('I-C\textsubscript{2}'), in which a law is said to be confirmed due to the fact that it and at least one other law are derived from a third law and that other derived law has been directly confirmed. I shall call that indirect confirmation, 'indirect-confirmation\textsubscript{3}' ('I-C\textsubscript{3}'), in which a law is said to be confirmed due to the fact from it in conjunction with other assumptions another law is derived which is directly confirmed. And finally, I shall call that indirect confirmation, 'indirect-confirmation\textsubscript{4}' ('I-C\textsubscript{4}'), in which a law is derived from another which is itself indirectly confirmed in any of the above three senses.

Besides the two general notions of direct confirmation and indirect confirmation I would like to discuss the notions of direct disconfirmation and indirect disconfirmation. If our law is of the form \'(x)(Fx \supset Gx)\' or \'(x)(Fx \& Gx),\' then it is directly disconfirmed by the statement of the Form 'Fa \& -Ga.' Unlike 'direct confirmation' 'direct disconfirmation' is a deductive relationship between statements. Thus, if a statement of the form 'Fa \& -Ga' is true, then it deductively follows that both statements of the form \'(x)(Fx \supset Gx)\' and \'(x)(Fx \& Gx)\' are false. On the other hand, indirect disconfirmation seems to be an inductive relationship. As with indirect confirmation, we may recognize four different kinds of indirect disconfirmation. If a law is derived from another law that has been directly disconfirmed, then the derived law is indirectly-disconfirmed\textsubscript{1} (I-D\textsubscript{1}). If a law and at least another one are derived from a more general law and that other derived
law is directly disconfirmed, then the law is indirectly-disconfirmed\(_2\) (I-D\(_2\)). If a law is used in conjunction with other assumptions to derive another law and that other law is directly disconfirmed, then the law used in the derivation is indirectly-disconfirmed\(_3\) (I-D\(_3\)). And finally, if a law is derived from another law which is itself indirectly disconfirmed in any of the above senses, then the derived law is indirectly-disconfirmed\(_4\) (I-D\(_4\)).

Since there are several different ways in which a statement can be either indirectly confirmed or indirectly disconfirmed, one wonders whether it is possible for any statement to be indirectly confirmed but not indirectly disconfirmed. It would seem that a true law would have to be such a statement. However, consider the statement 'All men are mammals.' Let us suppose that we have a way of determining whether something is a man independently of whether it is a mammal, and conversely. Thus the statement would not be analytically true. If it is not analytically true, most people would probably hold it to be a candidate for a law. However, it can be indirectly-disconfirmed\(_1\) because it is entailed by the directly disconfirmed statements: 'All men are plants' and 'All plants are mammals.' It would seem that, for any statement presented as a law, one can find other statements which entail it but are directly disconfirmed. Thus every statement that is true or has not been directly disconfirmed is I-D\(_1\). The consequence of this discussion is that I-D\(_1\) is useless in enabling us to determine what statements are true laws from those which are false. Consequently, we will have to rely on the other indirect disconfirmations. Let us now consider I-D\(_2\).
If from a statement or set of statements M, \( L_1 \) and \( L_2 \) are derived and \( C_1 \) directly disconfirms \( L_2 \), we may say that \( C_1 \) indirectly disconfirms \( L_1 \). If M consists of 'All men are plants,' 'All plants are mammals,' and 'All mammals are insects,' then 'All men are mammals' and 'All plants are insects' are derived. Since 'All plants are insects' is directly disconfirmed, then 'All men are mammals' would be indirectly-disconfirmed. The same criticisms I made against I-D\(_1\) are applicable against I-D\(_2\). I-D\(_2\) appears to be useless too. Yet there may be a way of modifying I-D\(_1\) and I-D\(_2\) to prevent such criticisms. Let us add the qualifier: a statement cannot be derived from other statements which are already known to be false or directly disconfirmed. Since it is true that \( a \), a man, is a non-plant, \( b \), a plant, is a non-mammal, and \( c \), a mammal, is a non-insect, then we have direct disconfirming evidence for 'All men are plants,' 'All plants are mammals,' and 'All mammals are insects,' respectively. However, the above statements of evidence simultaneously directly confirm 'All men are non-plants,' 'All plants are non-mammals,' and 'All mammals are non-insects.' The first two preceding statements entail 'All plants are neither mammals nor men' and the last two entail 'All mammals are neither plants nor insects,' both of which are directly and indirectly confirmed. It seems, then, that our discussion about indirect disconfirmation is somewhat redundant. If a statement confirms one statement, then it also simultaneously disconfirms another, and conversely. All significant discussion about indirect disconfirmation is found in all significant discussion about indirect confirmation. Let us now consider a problem of indirect confirmation or indirect evidence.
The concept of indirect evidence is quite important in deciding what universal conditionals are to be considered as laws. Let us assume that we are determining whether 'All crows are black' is a worthy candidate for a law. Furthermore, let us assume that we have direct evidence for the statement and no direct disconfirmations of it. Thus if it is a candidate for any kind of law, then, if a law, it would be a universal, not a statistical law. But do we have any indirect evidence for 'All crows are black,' i.e., from the statement 'All crows are black' and another statement for which we have evidence can we derive a true statement; or, is the statement 'All crows are black' derived from other statements for which we have evidence? From 'All crows are black' and 'Charlie is a crow' we can deduce 'Charlie is black.' Does the truth of 'Charlie is black' indirectly confirm 'All crows are black'? Note that 'Charlie is black' and 'Charlie is a crow' together directly confirm 'All crows are black.' Furthermore, 'Charlie is black' is not a generalization (or of the form of a law itself). Therefore, 'Charlie is black' does not indirectly confirm 'All crows are black'; it directly confirms it along with the premise 'Charlie is a crow.'

Is any universal conditional derivable from 'All crows are black'? An additional premise is needed which begins with 'All black things are . . . .' But is there any significant (i.e., non-analytic) generalization about black things that is directly or indirectly confirmed? Nagel argues that there appears to be no indirect evidence for it. 66

66 Nagel, op. cit., p. 66.
Is Nagel correct? Consider the following universal statements: 'All crows have feathers,' 'All feathers that emit light waves of angstrom units $a$ (or fail to emit light waves of any angstrom units, depending on how one defines 'black') are black,' and 'All crows have feathers that emit light waves of angstrom units $a$.' We can have direct evidence for these three statements and together they entail 'All crows are black.' Consequently we can have indirect-evidence for 'All crows are black.' Similarly, from 'All crows have feathers,' 'All feathers that emit light waves of angstrom units $a$ are black,' and 'All crows and ravens have feathers that emit light waves of angstrom units $a$' we can infer both 'All ravens are black' and 'All crows are black.'

Since we can directly confirm 'All ravens are black' we can indirectly-confirm 'All crows are black.' Since any of the above laws from which 'All crows are black' is derivable may in turn be indirectly confirmed too, then 'All crows are black' is indirectly-confirmed. Is the statement indirectly-confirmed? To meet this notion of indirect evidence, a universal statement which is directly confirmed must be derivable from it. 'All crows are black' in conjunction with 'All black animals are usually found only in non-polar regions' one can infer 'All crows are usually found only in non-polar regions' which can be, and has been, directly confirmed. Thus it would seem that there are some acceptable and non-controversial statements that permit 'All crows are black' to be indirectly confirmed in all four ways. Hence Nagel's claim seems to be incorrect.

However, another claim of Nagel's does seem to be correct, viz., we have indirect evidence against the claim that all crows are
black. For one thing it is known that a large number of other birds have feathers of colors other than black. This fact of color variance indicates that being black is not a lawful characteristic of crows. Furthermore, the phenomenon of albinism which has been discovered in most species of birds and mammals also indicates that being black is not a lawful characteristic of crows. Such laws or generalizations constitute indirect disconfirming evidence for 'All crows are black.'

Perhaps the most important reason why the statement 'All crows are black' need not be accepted as a law is that, in rejecting it, we will not have to modify any of our knowledge or law claims in science. As John Hospers notes:

a crow that was not black would change no other laws known to us; but a crow that was immortal (or even one that lived a thousand years) would excite considerable scientific surprise, because it might force us to reconsider the many other laws (about deterioration of tissue, etc.) with which it is interlocked.

According to Hospers, Nagel, and Braithwaite, the laws of science are so interconnected to each other by means of indirect confirmation that any change in one will invariably force a change in most of the others. It is because of such coherence (mutual support) between laws that Nagel says that scientists are reluctant to abandon a law in spite of direct disconfirming evidence—it is so indirectly confirmed by other laws that to reject the law would force the scientists to drastically revise their science. Assuming that the experiment was

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67 Ibid.
69 Nagel, op. cit., p. 65.
conducted properly, the reorganization might be avoided by reinterpreting the supposed disconfirmations in such a way that they are no longer disconfirmations. The classic example is the postulation of the neutrino in order to maintain the law or principle of conservation of energy.

In considering how we were able to indirectly confirm 'All crows are black,' a criticism, especially against Hospers, could be raised. If in finding a crow that does not grow old we are forced to modify the laws of tissue deterioration, then would we not have to modify the law that all feathers that emit light waves of angstrom units $a$ are black if we found a non-black crow? The answer is that we definitely do not have to modify the light wave law because a non-black crow does not directly disconfirm it; whereas, 'a crow not growing old' directly disconfirms 'all living creatures grow old' since a crow is a living creature. Thus in using Hospers' criterion a statement is a candidate for a law if its contradictory directly disconfirms another statement that is confirmed directly or indirectly. Consequently, besides asserting a universal or statistical invariance and being at least indirectly confirmed, a candidate for a law must be such that its contradictory if true would directly disconfirm an already established law.

Another criticism of this explication of law might be that it is circular. We can only determine whether a statement is a law provided it has certain relationships to laws. First of all this criticism is at most directed only against Hospers' criterion, not Nagel's and Pap's of indirect confirmation. Secondly, Hospers' criterion is used to...
decide when a new statement should be considered as a law. But this criterion does raise the problem as to how the old laws or the first ones were formulated. The problem can be resolved by noting that a set of statements can be formulated such that some of them are directly confirmed and are related to other statements in such a way that the other statements are indirectly confirmed. This set becomes the foundation of a scientific theory and other statements are added to them (or subtracted) in accordance with Höpser's criterion as well as Nagel's and Pap's. In this manner we can explain how laws are formulated without involving either an infinite regress or blatant arbitrariness.

The above discussion on direct and indirect evidence arose in considering what assurance we have for accepting subjunctive statements (laws being construed by Pap as subjunctive). Our assurance in such statements is, for the most part, indirect, since some of them have contrary-to-fact antecedents. Although I do not wish to discuss the literature concerning such statements, I do wish to discuss Nagel's treatment of them.\(^7^0\)

According to Nagel the subjunctive that is related to a law is a metalinguistic statement. If the form of our subjunctive is 'If \(a\) were a \(C\), then \(a\) would be a \(B\),' then, Nagel holds, the subjunctive is asserting that a statement of the form '\(a\) is a \(B\)' (the indicative of the consequent) logically follows from a statement of the form '\(a\) is a \(C\)' (the indicative of the antecedent) in conjunction with a statement of the form '\((x)(Cx\supset Bx)\)' (or its equivalent; or any set of universal

\(^7^0\)Nagel, op. cit., p. 72
truth-functional conditions that entail either it or its equivalent). But this cannot be all, since it is trivially true. The subjunctive presupposes that the statement of the form \((x)(Cx \rightarrow Bx)\) is to be indirectly confirmed and not known to have any directly disconfirming instances (i.e., true statements of the form \(Ca \cdot -Ba\)).\(^7\) This analysis is comparable to the one we have been considering in the covering-law models of explanation and prediction. The question arises as to whether there are any difficulties with regard to the material conditional. There does not seem to be any difficulty if we add the condition that the implication be indirectly confirmed. Those laws which have known false antecedents are accepted on the basis of utility where approximation is sufficient. On the other hand, those laws whose antecedents refer to unobserved entities are accepted on the basis of indirect confirmation. And of course, it is possible to have indirect confirmation of laws dealing with idealized situations or even imaginary ones.

Yet the most interesting feature in construing laws as subjunctive conditionals is that such a construction does not preclude any of the views taken with respect to the controversy. To put it in Braithwaite's terms, subjunctive thinking is as-if thinking: we do not commit ourselves to the existence of entities mentioned in the antecedent and consequent. What is the difference between 'If a gas were composed of small elastic spheres, then its temperature would be directly proportional to the product of its volume and pressure' and

\(^7\)Pap, op. cit., pp. 303-305.
'If a gas is composed of small elastic spheres, then its temperature will be directly proportional to the product of its volume and pressure'? Let us assume that the temperature of a gas is directly proportional to the product of its volume and pressure. From that assumption and the above two statements we can deduce that the gas acts as if it were composed of small elastic spheres. This latter statement leaves open the possibility of gas not being composed of small elastic spheres. But since the above two statements are hypothetical, then they too leave open the same possibility. If it is asked why the temperature, pressure, and volume of a gas are so related, one could say that it is because the gas is composed of small elastic spheres; i.e., one appeals to the antecedent of the indicative hypothetical. The subjunctive hypothetical seems to function as a contrary-to-fact conditional or a conditional whose antecedent is not known to be true. The truth of subjunctive hypothetical is, for the most part, established indirectly. (If it is contrary-to-fact, then its evidence must be indirect.) Thus Newton's first laws of motion in conjunction with other statements entail statements that are directly confirmable. The indicative hypothetical, on the other hand, can be directly as well as indirectly confirmed.

J. J. C. Smart has also analyzed subjunctive statements. His analysis is comparable to Nagel's. Instead of saying 'If x were A, then x would be B,' we can say the same thing logically by asserting the metalinguistic statement "'If x is an A, then x is a B' follows from a well-tested theory."^2 By 'a well-tested theory' he means

'a set of well-confirmed statements of law and initial conditions.'

The qualification in Nagel's analysis that the statement of the form

\((x)(Fx \supset Gx)\)

be at least indirectly confirmed is another way of saying that the statement follows from a well-tested theory.

Smart also has some interesting comments on vacuously true statements, i.e., material conditionals that are true simply because there are no instances of their antecedents. Smart claims that a vacuously true statement is harmless whether in a theory, explanation, or prediction since it "can not enable us to deduce any falsehoods."\(^7^3\) In considering Newton's first law of motion as a vacuous truth, Smart claims that it is irrelevant to Newtonian mechanics\(^7^4\); in fact, all vacuously true statements are irrelevant and harmless if we do not "discuss non-actual but possible worlds."\(^7^5\)

Having considered some of the essential characteristics of a law, especially that a law is a statement of an invariance that is indirectly confirmed, let us consider the explanation of laws. The explanation of a law should be distinguished from the evidence we have for it. Statements of evidence are always statements of observed phenomena. But an explanation is an explanatory argument. If the law to be explained is a universal hypothetical, then its explanation will be deductive. Nagel argues that the explanation of all laws, universal and statistical, is deductive.\(^7^6\) With regard to the explanation of a statistical

\(^7^3\)Ibid., p. 166.

\(^7^4\)Ibid., p. 165.

\(^7^5\)Ibid., p. 170.

\(^7^6\)Ibid., pp. 33 and 509.
law, Nagel holds that some of the premises of such explanations will be statistical statements themselves. In general, the explanatory argument will contain at least two premises both of which are essential in the deduction of the law being explained, "and the premises, taken singly or conjointly, do not follow logically from the explicandum" (explanandum).77 Furthermore, at least one of the premises (which is also a law) can be used in conjunction with different statements to explain other laws.78 This latter condition is required for having divergent phenomena indirectly confirm a law, in this case we have divergent laws indirectly confirming a more general law.

Another condition Nagel considers necessary for the explanans of a law is that none of the premises are statements of initial conditions.79 Two points of importance arise: (1) Is Nagel's claim correct? and (2) What bearing does this view have on the identity thesis? First of all, it seems that Nagel's condition is too restrictive. Consider the relationship between Newton's laws of motion and gravitation and Galileo's law for freely falling bodies and Kepler's law of planetary motion. The latter laws or, more correctly, their approximations are deducible from Newton's laws in conjunction with statements of initial conditions, such as the radius and mass of the earth, and gravitational attraction of the sun and other planets on the motion of any particular planet. Such an explanatory argument does explain these laws even though statements of initial condition are contained in the explanans.

77Ibid., p. 34.
78Ibid., p. 36.
79Ibid., p. 34.
This same point can be made in general. Consider a universal law of the form ' \( (x)(Fx \supset (Ga \cdot Hax)) \)'. It is explained, by a law of the form ' \( (x)(Fx \supset (\exists y)(Gy \cdot Hyx)) \) ' and a statement (or identification of the measurement) of the form ' \( (x)(\exists y) [(Gy \cdot Hgx) \cdot (Cy = a) \supset (Ga \cdot Hax)] \) ' and a statement of initial conditions of the form 'Cy = a' (which represents: 'under condition where y is identical to a'). Thus it appears that statements of initial conditions are sometimes essential as premises of explanatory arguments for laws.

Let us now consider the second point, viz., what bearing this view has on the identity thesis. According to the identity thesis, predictive and explanatory arguments have the same logical structure. Therefore, as Scriven notes, if we have explanatory arguments for laws, then apparently we also have predictive arguments for laws. But what does it mean to predict a law? Scriven considers as a possible reply to the question that:

if explaining a law consists in explaining the over-all pattern of events, past, present, and future, predicting a law . . . could be regarded as compounded out of such predictions and postdictions. . . . explanations of laws only have a correlate among predictions if we extend the meaning of the notion of prediction to include postdiction. Scriven argues that such an extension of prediction to include postdictions as well would change its initial meaning. He also holds that the procedure of inferring postdictions is essentially the same as the procedure of inferring predictions, but the results are different. I do not believe that Scriven's comments so far invalidate or even weaken

81Ibid., p. 180.
the identity thesis. For remember that Hempel and Oppenheim argue that the difference between an explanatory argument and a predictive argument is pragmatic, not logical. Thus the results of an explanatory argument are to explain; of a predictive argument, to predict; of a postdictive argument, to postdict. Furthermore, 'predictive argument' could be defined as 'an argument in which an inference is made from statements of initial conditions occurring at time \( t \) to a statement of conditions occurring at a time earlier than \( t \) (a postdiction), or a time later than \( t \) (a prediction).'. In such a maneuver, 'prediction' would not be extended in meaning as Scriven claims, but 'predictive argument' would be. Perhaps a better word could be used for 'predictive argument'—such as, perhaps, 'temporal argument.'

We still have not, however, answered our initial question. Do we have temporal arguments for laws? Kim answers the question in the affirmative by saying that "if the laws used in the explanans were known independently of the law being explained," then we could predict the new law on the basis of the old. To put it more accurately: if we have evidence for a law, we infer that another law (which we have not yet confirmed) will be confirmed. But are we predicting a law or its confirmation? Or are they the same? It might seem that the predicting of a law and the predicting of its confirmation need not be the same since it is quite possible for us to predict a law and yet not have any confirmation of it or know how to confirm it. But if the laws from which the predicted law is derived are confirmed, then the predicted law, by the very act of predicting it, is indirectly-confirmed.

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(or possibly, I-C₄). Yet the indirect confirmation of the derived law
is not predicted; it is already known and used as the basis for assert-
ing the explanans. Nevertheless, if we are predicting that the derived
law will be directly confirmed or indirectly confirmed in some other
way, then it is correct to say that the predicting of a law and its
confirmation are not the same.

What does it mean to predict a law? If our law is of the form
'(x)(Fx⇒Gx),' then to predict such a law is to predict that anything
that is an F will also be a G. There is, however, a problem peculiar
to postdiction. Some predictions will eventually become directly con-
firmed or directly disconfirmed. Yet postdictions are at most indi-
rectly confirmed and indirectly disconfirmed.

It would seem, then, that there is no difficulty in maintaining
the identity thesis with regard to laws. But it should be noted that
if the logical structure of the explanation of laws is deductive then,
according to the identity thesis, the logical structure of the predic-
tion of laws is also deductive.

Here one might wonder: What bearing does the explanation of laws
have on the I-A model? The I-A model either denies laws or holds that
they are irrelevant in explanation and prediction. It follows that if
laws are irrelevant in explanation and prediction, then the explanation
and prediction of laws is irrelevant. This raises the question as to
why laws should be explained. Some people, for example, want to know
why the temperature of a gas is directly proportional to its pressure
and volume. Such a question has sometimes been considered as a
request for an explanation of the gas law. Furthermore, such a
request seems to be quite appropriate. How, then, will the I-A advocate handle this situation? If asked why in a certain sample of hydrogen gas its temperature, pressure, and volume are so related, the advocate will tell us because other samples were observed as so related. If asked why the temperature, pressure, and volume of hydrogen gas are so related, the advocate will tell us that this is because other samples of different kinds of gas were observed as so related. If we ask about gases in general, the advocate could tell us about the analogous behavior of liquids and solids with regard to temperature, pressure, and volume. Although his procedure is analogous to that of the traditional view advocate's, the I-A advocate still remains within the inductive analogical domain, and any universal statements he uses as premises can thus be construed as summary statements of verified observational statements. Thus for the I-A advocate 'All hydrogen gas's temperature, pressure, and volume are related in a certain manner' is equivalent to 'sample a of hydrogen gas was found to be so related, sample b of hydrogen gas was found to be so related, . . . .'

But what if a solid is found (as has been found) whose temperature varies inversely with its volume—a metal that contracts upon being heated. How can the I-A advocate explain this anomaly? Of course, finding other metals, liquids, and gases that behave in a manner similar to the initial anomaly may begin to help him explain why the metal contracts upon being heated; but, how can he explain that some contract and some expand upon being heated? One way out of this quandary would be to say that, at this point, we have reached the limits of explanation: it is a brute fact that some materials contract when heated and
some materials expand when heated. Although this answer may not be satisfactory to many, it does have some merit. A traditionalist at this point would probably begin to explain the variance in terms of the difference in the microstructure of the materials. But the I-A advocate could ask: Why do the materials have different submicrostructures? The traditionalist, instead of being driven to hold either that ultimately there are brute facts (and as a consequence is little better off than the I-A advocate) or that explanation entails an infinite series, can hold that for any particular phenomenon or anomaly there is a sufficient explanation complete in itself. Of course, one may want an explanation of the phenomenon's explanation, but that is another problem. The difficulty with the I-A view, so the traditionalist might argue, is that it can offer no explanation of the above anomaly.

But the I-A advocate is not so easily vanquished for, after all, he does not have to say that the variance is inexplicable. If an anomaly occurs, the I-A advocate can attempt to find the same anomaly holding with respect to other phenomena. Upon the discovery of such similar anomalies, any one can be explained by analogy on the basis of the others. But, of course, one cannot explain the anomaly unless one has discovered other individuals also possessing it. However, this is not a limitation of the I-A model, since the traditionalist will need both a law and a statement of initial conditions to explain it. But what if the I-A advocate is unable to find other phenomena, especially of different kinds, to explain the anomaly? This situation is similar to the traditionalist's not being able to find a law adequate to explain the anomalous occurrence. At this point one could, if ingenious enough,
formulate a model on the basis of which the anomaly is explained analogically. As long as the model is not directly confirmed (if it were it would, of course, cease being a model), it could be considered as an imaginary construct set up to explain an anomaly. If we discover that it can also be used to explain other phenomena, then it becomes indirectly-confirmed. But as long as the model is only indirectly confirmed, explaining in terms of it will always be analogical—as if thinking. Since we have already observed that the traditionalist can hold analogical models, there may be a tendency to blur the formulation of a model according to the traditionalist and its formulation according to the I-A advocate. There is definitely a difference. The model of the traditionalist can be expressed, at least partially, in laws; the model of the I-A advocate can only be expressed in data-statements, i.e., statements of the existence of the properties of particular entities—including imaginary ones.

Rollin W. Workman criticizes the view that models serve as explanations by claiming that the model merely rephrases the request for the explanation. What is still unexplained, he argues, is why the phenomena acts in accordance with the model. According to Workman, the model is explanatory only if it is true or accepted as true (rather than being imaginary). Is Workman’s thesis correct? Workman seems to be asking for a justification of the model. The only justification he accepts is its being true or its being accepted as true. But these two alternatives do not seem to exhaust all the adequate available justifications.

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It could be that the model in question is the only available one regardless of its being true or accepted as true. It could be the simplest model (in any common notion of 'simplest') available and yet be false regardless of being so used. It could be the only model that is indirectly confirmed (which is not a necessary condition for truth). Thus there seems to be no good reason for accepting Workman's thesis (which would bias the issue in favor of realism anyway).

A problem of some note does arise, however, if we accept for some of our explanantia laws and models which are at most indirectly confirmed. A law or a model for which we have no direct confirmation cannot be used in making a prediction of an individual event. In the discussion on prediction we presented as a necessary condition that one of the statements of initial conditions be a statement of observed fact. Thus on the traditionalist's model, if we have a law of the form \((x)(Fx \supset Gx)\) to predict a statement of the form 'Ga,' we must have observed the referents of the statements of the form 'Fb \cdot Gb,' 'Fc \cdot Gc,' . . . , and 'Fa.' 'Fa' in both cases is essential. But if our law or model is about imaginary entities, then no predictions can be made from them. Furthermore, if we hold that such laws or models are adequate explanations, then we have violated the identity thesis. The identity thesis can be maintained if we modify our analysis of such explanations. We could say that a law or a model which at most has only been indirectly confirmed presents us with a hypothetical or as-if prediction. For instance, if we were observing gas molecules, then we could predict that the gas' volume would be directly proportional to its pressure and volume. The difference between as-if explanations
and predictions and non-as-if explanations and predictions is not one of logical structure but of confirmation or justification. The as-if ones are at most only indirectly confirmed; the non-as-if ones are both directly and indirectly confirmed. Thus the identity thesis is preserved, and the traditionalist view and the I-A view are still worthy opponents.

At this point I wish to summarize my discussion on laws. Laws or their candidates have been found to be statements of universal or statistical invariance which are at least indirectly confirmed. To say that they are indirectly confirmed is to say that they are interconnected in a variety of ways (the four kinds of indirect confirmation) with other laws or candidates. Furthermore, a law is indirectly-confirmed only to the extent that the least confirmed statement from which it is derived is confirmed. (If one of the deriving statements is not confirmed, then the derived statement is not confirmed either.) There does not seem to be too much difference between construing laws as material conditionals and as subjunctive conditionals as long as one holds that they must be indirectly confirmed.

It also seems to be the case that indirect confirmation is a better guide in deciding whether a statement might be a law than direct confirmation. Any set of relevant observational statements directly confirms a universal statement and directly disconfirms its contrary; i.e., a statement of the form \( F \land \neg G \) directly confirms a statement of the form \( \neg F \lor G \) and directly disconfirms a statement of the form \( (x)(F \lor \neg G) \). The final important characteristic discussed is that a candidate for a law must be such that its contradictory can be used, if
true, to directly disconfirm an already established law. Of course, no law candidate can be known to be false.

I have also argued that laws of the form '(x)(Fx\supset Gx)' are essential in explaining and predicting individual events. Laws of more complex form can be used in such explanations and predictions only if they entail a law of the form '(x)(Fx\supset Gx)'. Of course, such entailment is not essential for the explanation of laws, especially if one wants to explain one of the more complex laws.

Section V

Some Final Comments on Explanation and Prediction

In this section I want to consider some criticisms of the traditional view on explanation and prediction. First, Willard C. Humphreys has argued that Hempel and Oppenheim's conditions for explanation are not sufficient for distinguishing what needs to be explained from what does not. According to Humphreys only anomalies need to be explained, whereas the criteria for explanation formulated by Hempel and Oppenheim does not distinguish between anomalies and normalities. Humphreys defines an anomaly as an observed state of affairs (or a statement of that state of affairs) which is contradictory to a person's set of beliefs (or a statement of his beliefs that conflict with the statement of the state of affairs). Thus something is an anomaly to us if we

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84. Willard C. Humphreys, Anomalies and Scientific Theories (San Francisco: Freeman, Cooper & Co., 1968), pp. 74-79.

85. Ibid., pp. 81-82.
cannot explain it consistently on the basis of our present knowledge. Consequently, an anomaly can only be explained by revising our present theories, laws, or belief about past and present initial conditions. It is on the basis of this analysis that Humphreys wants to add to Hempel and Oppenheim's criteria two more rules: (R5) the explanandum must describe an originally anomalous state and (R6) the revision of the original beliefs must be as minimal as possible for an adequate explanans. The last condition is a rule for scientific conservatism. It insures the least intellectual disruption.

It is (R5) which I wish to discuss. (R5) is a rule distinct from the four given by Hempel and Oppenheim. Is it, however, a distinct rule apart from their view on explanation and prediction? May it not be the case that other portions of their view entail (R5)? An anomalous state is definitely an unexpected, an unpredicted state. Now the purpose of an explanatory argument is to provide us with an understanding of what we did not initially understand; and the purpose of a predictive argument is to provide us a reason for expecting something that we did not initially expect. All of these observations are in line with Humphreys' analysis as well as Hempel and Oppenheim's. According to Hempel, predictive and explanatory arguments have enabled man to improve his position in the world, especially by helping him to predict and control his environment. Another way of putting Hempel's point

86 Ibid., p. 89.
87 Ibid., p. 78.
88 Ibid., p. 80.
is to say that we use these arguments to eliminate the anomalies we find in our environment. For if there were no anomalies, such arguments would never have been needed: we would have always known correctly in advance.

But the most important part of the traditional view which affects (R5) is the identity thesis. To explain what was originally an anomaly is to state the conditions under which it is to be expected and then show that such conditions prevailed. Now according to the identity thesis 'explanation of x' has the same extension as 'prediction of x.' Thus to explain, according to the identity thesis, why a certain event occurred is to state the conditions under which it is to be expected and then show that such conditions existed. Consequently (R5) is a special case of the identity thesis: to explain an anomaly is to show how it could have been predicted. In summary, then, although Humphreys correctly notes that (R5) is distinct from the other four rules, it essentially finds expression in the identity thesis and in the purposes for presenting explanatory and predictive arguments according to the traditionalist. Thus Humphreys' view is not contrary to the traditionalist's; it simply makes the latter more explicit.

One wonders how the more restrictive part of Humphreys' view (viz., that explanation is concerned only with anomalies) can adequately explain the explanation of laws? Can a law be an anomaly? Not in the way Humphreys defines 'anomaly' since it refers to an unexpected conflicting state of affairs which is always something particular or individual and non-linguistic. Yet it seems quite possible that someone has a set of beliefs which conflicts with an adequately established
statement that is either a law or an excellent law candidate. In such cases a law could be an anomaly in the sense that its being well-confirmed (a state of affairs) conflicts with our beliefs. Could Humphreys argue that upon becoming aware of the states of affairs that confirm it and explain those states of affairs, the law would lose its anomalousness? Nevertheless, showing that a law is well-confirmed is not explaining it. Furthermore, it is on the basis of the law in question (unless one adopts the I-A model) that the confirming instances are explained. Thus we would be explaining a law in terms of itself which is a petitio.

Humphreys' alternative, it seems, is to adopt the I-A model of explanation in which the issue of law-explanation need not arise. Yet this alternative is not open to him since he holds that all reasoning is deductive. 90 Hence there is no room in Humphreys' view for the explanation of laws. Since the explanation of laws seems to be an appropriate scientific endeavor (especially in the systematization and unification of old laws and the development of new ones through predictive arguments) Humphreys' view needs to be modified. One possible line of modification would be to hold that the only events to be explained are anomalous ones, leaving open the possibility of explaining some laws.

The other criticism of the traditional view I wish to consider is that of Scheffler's. According to Scheffler, the identity thesis is associated with the thesis that the central purpose of science is to

explain and predict, especially phenomena. Although it is true that the advocates of the traditional view have discussed and examined in detail explanation and in slightly less detail prediction, they do not claim that the above thesis is the central purpose of science. As an example, consider Mehlberg's presentation discussed earlier (in Chapter III, Section I). Even if the central-purpose-thesis were held, it seems to be logically distinct from the identity thesis. Nevertheless, is there anything in favor of the central-purpose-thesis? Being able to make successful predictions (predictions which have become verified) is very important in enabling us to control our environment and test our laws and theories. Popper has argued that the scientist's main interest is in finding sound explanatory arguments for phenomena, and that the scientist is interested in formulating predictive arguments only because such arguments enable him to test his laws and theories. It is by means of predictive arguments that one acquires direct and indirect evidence for laws.

It is definitely true that the scientist is concerned with the explanation of phenomena. But mythologists, occultists, demonologists, and astrologers also present what they as well as their followers believe to be explanations of phenomena. For instance, let us suppose that we observe an anomaly and want to have it explained: 'Why did Joe, who seemed to be in perfect health, suddenly die?' A demonologist tells us that if Satan wills the death of anyone, then that person dies and, in fact, Satan willed the death of Joe. On the other hand, a

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91 Scheffler, The Anatomy of Inquiry, p. 46.
coroner tells us that if someone ingests a pound of arsenic they usually die, and that Joe had, as a matter of fact, ingested a pound of arsenic. The first explanation is of the D-N type (disregarding the fact that its law refers to a specific individual) and the second explanation is of the I-S type where the probability is very close to 100 per cent. Now upon what basis do we decide between these two explanations? The demonologist's law may be interconnected with a variety of other general statements that form a coherent theory of demons and their activities. Hence claiming that the law is not interconnected with others is not an adequate criticism. But if it is the case that we cannot observe Satan in the act of willing the death of someone, then the only part of the law which is accessible to observation is its consequent through its instances, such as particular people dying.

On the other hand, we can observe instances of both the antecedent and consequent of the scientist's law; and because of such observability, we can use the scientist's laws to make a predictive argument founded on what we observe. For example, we observe Jane ingesting a pound of arsenic (instance of antecedent) and by means of the law we predict her death (instance of the consequent). Thus the scientist's law has predictive import whereas the demonologist's does not. In generalizing from this example, one might be tempted to say that a scientific explanation can be distinguished from a non-scientific explanation in that the former has predictive import but the latter does not. If one does not wish to admit non-scientific explanations, one could appeal to the identity thesis by saying that an explanatory argument must be a
potential predictive argument. Given the necessary characteristic of a predictive argument for an individual event (viz., that one has observed an instance of the law's antecedent), then any purported explanation which always fails to meet that characteristic would be a pseudo-explanation. This analysis shows that the predictive arguments and predictions are essential in distinguishing the scientific from the non-scientific. Whether prediction is the primary function of science or simply an essential requirement for sound explanations (which is the primary function of science, according to Popper) will be discussed in more detail in the chapter on instrumentalism.

Although the preceding discussion may seem reasonable enough, it does present a serious problem. According to the traditional view a theory is composed of three different kinds of statements: theoretical, correspondence rules, and observational. The theoretical statements refer to entities or properties that are incapable of being observed. Therefore, if one adopts the identity thesis and the above necessary condition for scientific predictions, theoretical statements might not have any role in explaining or predicting phenomena. It is time now to consider the theoretical-observational distinction so central to the traditional view.
CHAPTER IV
THE THEORETICAL-OBSERVATIONAL DISTINCTION

Section I
Carnap's Views on the Distinction

I shall begin the presentation of the distinction between theoretical concepts and observational concepts by citing references from those who hold the view to show in what way they hold it. In Foundations of Logic and Mathematics, Carnap makes a distinction between the non-logical terms in empirical science on the basis of abstractness.\(^1\) Some terms are more abstract than others; the ones which are less abstract he calls 'elementary' and the more abstract 'abstract.' By an elementary term, Carnap means any term that we can apply "in concrete cases on the basis of observations in a more direct way than others."\(^2\) In determining whether the more abstract terms are applicable, the procedures we have to go through are more complex even though they must be made on the basis of observation.

Carnap holds that there are many intermediate levels of abstractness and instead of giving a definition for 'degree of abstractness,' he presents a list of such terms proceeding from the more elementary


\(^2\)Ibid.
to the more abstract:

bright, dark, red, blue, warm, cold, sour, sweet, hard, soft . . ; coincidence; length; length of time; mass, velocity, acceleration, density, pressure, temperature, quantity of heat; electric charge, electric current, electric field; electric potential, electric resistance, coefficient of induction, frequency of oscillation; wave function.

Apparently the semicolons distinguish one level of abstractness from the other. Carnap, in being a traditionalist, holds that a theory is a calculus that contains terms like those in the above list and rules of correspondence connecting some of them to observable properties of things. He further holds that all of the terms of a theory are interconnected (if a term were not interconnected with other terms it would be useless in explaining or predicting anything).

The question of importance, according to Carnap, is which terms are going to have semantical rules of interpretation? Because of their interconnectedness we do not need such rules for all the terms. (It should be remembered that such rules constitute model. These are rules which give empirical meaning to the terms, i.e., rules which connect them with phenomena.) In answering his question Carnap believes that the interpretation rules used might be simple enough to enable a layman to use the theory for explaining and predicting phenomena. In carrying out this purpose it will be the elementary terms that are given interpretation rules. If it is asked how we know which terms are elementary terms if they have not previously been interpreted, it can be replied that the rule of interpretation itself determines how elementary a term is going to be in correlating it to phenomena. On the basis of these

3Ibid., p. 62.
rules of interpretation the layman, who initially did not know any physics, could come to have some knowledge of physics provided he knows the correlated phenomena and can study them.

Carnap holds that any term in a theory can be given a rule of interpretation. But he decides on giving only elementary terms rules of interpretation because of the layman's abilities and knowledge prior to knowing any physics. It would seem both from his examples and his appeal to the layman that the rules of interpretation for the elementary terms are made on the basis of the observable properties of things. (It should be noted that not all rules of interpretation need be empirical, e.g., consider rules of interpretation for some geometries.) But if the elementary terms are given empirical rules of interpretation, then so are all the terms of the calculus because of the definitions that interconnect them. However, the degree to which they are interpreted empirically or observationally may vary since their degree of abstractness varies and is determined by the rules of interpretation.

In presenting his view Carnap uses a diagram such as the following:

\[\text{abstract terms: } '\text{electric field}', '\text{temperature}', '\text{length}' \ldots \]
\[\text{elementary terms: } '\text{yellow}', '\text{hard}' \ldots \]
\[\text{interpretation of elementary terms: observable properties of things}\]

\[\text{first method: order of primitive terms}\]
\[\text{second method: primitive terms}\]

\[\text{Ibid., p. 63.}\]
It can be seen from the diagram that the terms and the statements comprising them (the theory) form a hierarchy of abstractness and, since it is deductive, a hierarchy of generality. Those statements containing the more abstract terms entail those statements (in accordance with definitions) containing the more elementary terms. Because of this hierarchical system two possibilities are before us in theory construction and definition: the first and the second method. Since a theory is an axiomatic system, some of its terms will have to be taken as primitives and the other terms will be defined by means of them.

If we accept the first method, the elementary terms are taken as the theory's primitives and the abstract terms are defined in accordance with the elementary primitives. However, since this method of definition proceeds from the less general and abstract to the more general and abstract, the primitive elementary terms cannot explicitly define (i.e., are not logically equivalent to) any abstract non-primitive terms; at most, they conditionally define it (the conditional definition thesis). For example, 'having colorless crystals' is only part of the definition of 'rock salt' since other chemicals can also have colorless crystals. Since a theory, as a deductive system, is tested on the basis of confirmations and disconfirmations, if we have found some colorless crystals, then we only have inductive evidence (partial evidence) that we have rock salt. If one would want the having of colorless crystals be the completely defining characteristic of rock salt, then the theory would begin to lose its hierarchical structure since 'having colorless crystals' would be logically equivalent to 'rock salt.' Furthermore, our theory would also begin to lose its
explanatory and predictive value since explicit definitions by themselves explain nothing and enable no significant predictions. If we do accept the first method of definition, then we have, according to Braithwaite (as discussed earlier in Chapter II, Section VI) a theory since the derived formulae (the elementary statements) are epistemologically prior (due to the rules of interpretation) to those from which they are logically derived.

The second method of interpretation consists in taking the more abstract and general terms as primitives and in defining the less abstract and the elementary terms by means of them. Since the statements containing these primitives entail the statements containing the less abstract terms, only explicit definitions are needed. In this situation the abstract terms explicitly define the elementary terms. Thus, from 'x is rock salt' we can infer 'x is colorless and crystalline.' If we accept this second method of definition, we do not have (according to Braithwaite) a model since the derived formulae are still epistemologically prior to those from which they are logically derived because only the elementary terms have been given interpretation rules. These two methods are methods of theory construction and definition, not of theory interpretation. Thus Carnap holds that a theory, regardless of how its definitions proceed, is to be given an observational interpretation at the elementary level. But why at the elementary level? Because observational statements are singular statements, i.e., statements of the form 'Fa' (a, a specific individual has the property F or is a member of the class F'). Furthermore, such statements are less general and are entailed by the more general statements, such as
laws in conjunction with other observational statements. These singular statements are observational statements because their forms can be and have been given an observational interpretation.

A theory can be considered in its uppermost level as a system of explanatory arguments for laws. Thus, at this level we find statements of the form, 

\((x)(Fx \supset Gx),\) 
\((x)(Gx \supset Hx),\) 
and 
\((x)(Hx \supset (\exists y)(Iy \cdot Jxy))).\)

Statements of these forms only entail other statements of more or less the same form. However, at the lower levels we can find statements of the form \('Fa \cdot Ga,' 'Ga \cdot Ha,' 'Ha \supset (Ib \cdot Jab).'</\>

Given the rules of interpretation for 'being an F,' 'being a G,' etc., we can empirically determine whether a statement of the form 'Fa' is true and then predict, e.g., that a will also be a G, by means of the statement of the form

\((x)(Fx \supset Gx).''\) If elementary statements of the form 'Fa' and 'Ga' are verified, then so is the statement of the form 'Fa · Ga'; and we have directly confirmed the law of the form \((x)(Fx \supset Gx),''\) and indirectly confirmed a law entailing it or derived from it. Although Carnap's discussion has enabled us to understand the structure of a theory more clearly, he has not given us here a meaning analysis of 'observational' except as that which is less complex and understood by a layman.

Could it then be the case that observational concepts are those concepts which all or the majority of people apply to what they experience? Spector has criticized this claim by saying that if everyone or most everyone came to understand all the terms in physics, for instance, then physics would no longer contain any theoretical terms.\(^5\)

identifies 'theoretical terms' with 'abstract terms' and 'observational term' with 'elementary term.' Although this identification seems harmless enough (it is harmless provided the rules of interpretation are only observational), we may wonder whether Spector's criticism is sound.

Even if we were all experts in physics, the distinction between observational terms and theoretical terms could still be drawn since it is a logical distinction; viz., observational terms will have their own rules of interpretation and theoretical terms will not. The theoretical terms will be connected to the observational terms by correspondence rules. These observational terms will be given empirical interpretations. Theoretical terms have empirical interpretations only indirectly—through their connections with observational terms. Since one can maintain the distinction between theoretical and the observational terms by means of how they are interpreted and connected with other terms, Spector's criticism seems rather weak.

In another work, Carnap defines 'observable' in the following way:

A predicate 'P' of a language L is called observable for an organism (e.g. a person) N, if, for suitable arguments, e.g. 'b', N is able under suitable circumstances to come to a decision with the help of few observations about a full sentence, say 'P(b)', i.e. to a confirmation of either 'P(b)' or '-P(b)' of such a high degree that he will either accept or reject 'P(b)'.

In this explication of 'observable' we again note the appeal to some

6Ibid., p. 92.

mode of experimentation that can be simply performed. Furthermore, as with the abstract and the elementary, he holds that the distinction between the observable and the unobservable is one of degree although he believes that the above analysis draws an arbitrary line through the degree continuum. Such an arbitrary decision is justified for the sake of simplicity in presenting other concepts such as 'confirmable' and 'testable,' since by means of it we will be able to know in advance whether a predicate denotes an observable for an individual. Thus for a normal color-sensed person 'red' will designate an observable property while for a blind man it may not.

On the other hand 'an electric current of 10 amperes' does not designate an observable property for anyone, since special instruments and detailed prior observations are needed to determine whether the instruments are indeed the kind that measure electric current. Thus, the term would designate an unobservable to a layman, since he would not know how to measure electric current; also, the term would designate an unobservable to a physicist since he would have to conduct some prior experiments to determine whether the instruments are appropriate. In either case, that the electric current is 10 amperes, cannot be decided, so Carnap believes, with the help of a few observations. Due to the relativity or variability of 'with the help of a few observations' Carnap's analysis of 'observable' is vague in the sense that there is no precise point in degree of experimentation when the word is applicable and when it is not.

8Ibid., pp. 63-64.
Spector argues that distinguishing what is observable from what is not observable by what can or cannot be done on the basis of a few preliminary observations is faulty, because a physicist does not have to conduct many observations before he can decide whether the instrument at hand is an ammeter. Perhaps he has read the label of several instruments sitting on the shelves and picked that one labeled 'ammeter' from those labeled 'voltmeter' and 'thermometer.' The question that arises at this point is whether label-reading is adequate in determining whether or not an instrument is an ammeter. If it is adequate, then Spector seems to be correct; but if it is not, then due to its external similarity to a voltmeter the ammeter will have to be internally examined to determine what parts it has and if they are properly connected for ampere reading. It could also be argued, in Spector's favor, that if a scientist had to test all of his instruments prior to using them to insure that he is using the correct instruments, then little research would and could be accomplished. The scientist reads the labels or determines his instrument by its physical shape, connects it to what he is testing, and then performs the experiment. If, however, the experimentation significantly deviates from his predicted results, then he could go back and thoroughly examine his instrument to determine whether it is the correct instrument for the experiment and, if it is, to determine whether it is properly functioning. Since such testing of an instrument is posterior to the experiment, then the property measurable would still be, according to

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Carnap's analysis, observable.

Is there anything we can say in Carnap's defense? First of all, even if the scientist who is using the instrument has not pre-tested the instrument himself, he is assuming that someone has, some laboratory assistant upon whom he is depending at least for the sake of saving time. We could now add to Carnap's criterion: if the instrument used in the experiment had to be submitted to a detailed pre-testing or it is presupposed that it was so tested, then the property it enables us to determine qualitatively or quantitatively is unobservable. Although this defense seems to weaken Spector's criticism it produces some curious results. For example, eye glasses, telescopes, and microscopes are subjected to detailed pre-testing before they are used as instruments. Thus it would seem that those properties which are determined by means of such instruments, such as red, would be unobservable whereas Carnap holds that they are observable properties. Spector notes, moreover, that there are certain stars which are observable only through telescopes. Thus these stars would have to be classified as unobservables. To Spector's example we can add all those microanimals and microplants which can only be observed through microscopes. On Carnap's account and defense they too would be unobservables.

To further complicate the matter, the question arises as to whether the word 'star' is an observational or an unobservational (theoretical) term. One suggestion (based on the micro distinction) is that there are naked-eye stars and there are telescope stars, i.e., stars which are visible to the naked eye and stars which are visible

\[10\] Ibid., p. 14.
only through the telescope. It should be noted that some people's eyes are better than others with respect to observing stars, i.e., some people can observe with the naked eye fainter stars than other people. Thus the distinction between naked-eye stars and telescope stars would be vague in Carnap's sense.

Although it may appear absurd to distinguish stars in the above fashion, astronomers do call things they can only observe by means of the radio telescope 'radio sources.' Due to the structure of these sources, they are called either 'radio stars' or 'radio galaxies.' Such designation is definitely made by analogies based on telescopic and naked-eye observations. Similarly in biology we distinguish between macroanimals, i.e., naked-eye animals, and microanimals. The latter are so-called because of their similarities to the naked-eye animals. Naked-eye stars would thus be observable stars and telescope stars would be unobservable stars.

In examining the preceding line of defense for Carnap's criterion for 'observable,' Spector notes that Carnap would have to hold that 'naked-eye star' would have a rule of interpretation whereas 'telescope star' would not, "thus assuming that the meanings of these terms are of a radically different nature." Spector believes that according to astronomical theory both terms have the same meaning. In defending Carnap at this point one can say that statements containing 'telescope

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11 Ibid.


star' are connected by definitions to statements containing 'naked-eye star.' Thus, in that manner 'telescope star' would be indirectly given a rule of interpretation.

Although we seem to have met Spector's criticisms thus far, let us consider a different criticism. Note in the above discussion the statement to the effect that telescope stars are observed by means of telescopes. But is not this statement a contradiction according to Carnap's criterion? Also, could we not say that the ammeter permits us to observe both the electric current and the amount of electric current? The ammeter enables us to measure the electric current. It would also seem correct to generalize and claim that most instruments the scientist uses are designed to make some kind of measurements.

Now measurements are made or inferred (mathematically, at least) on the basis of what we observe; we do not observe measurements, we make them. Furthermore, the pre-testing of an instrument has to decide whether the instrument can be used (and how accurately) to make such measurements. Although the measurement is an unobservable, it does not follow that what is measured is also an unobservable unless the distinction between 'measurement of x' and 'x' is not correct. The measurement made by the ammeter is 10 amperes. Now is that which was measured, the electric current, something distinct from 10 amperes?

By analogy, perhaps another example will help clarify the issue. We observed the top of the table, we measure it to find its area. We do not observe the table top's area (else there would be no need of an instrument such as a ruler), but we do observe the table top. However, there is an ambiguity here. We say both that we measure the table top
and we measure its area. 'Measuring the table top' is more ambiguous than 'measuring the table top's area' since one can measure the table top's weight or volume and be measuring the table top; whereas measuring its area is measuring it in a specific manner. Is the table top something distinct from its area, weight, or volume? Not if its area, weight, and volume are identifying characteristics of it. Is its area, volume, and weight something distinct from, say, 100 square inches, 1,000 cubic inches, and 50 pounds? Again, if these values are identifying characteristics of it, then the table top is not distinct from these values. Hence if these values are unobservables, then the table top, insofar as it has area, volume, and weight is unobservable. The consequence of our discussion is this: whatever is measurable in a certain respect is unobservable in that respect. Thus if electric currents cannot be identified except by ampere values, they are unobservables.

Nevertheless, eyeglasses, telescopes, and microscopes, insofar as laymen use them, are not instruments in the sense that they measure anything. Thus they are used only to determine qualitatively the characteristics of phenomena. Furthermore, these items (as well as, e.g., hearing aids) seem to be merely extensions of our sense organs: a way of enabling our senses to distinguish better qualitatively. Yet the above instruments can be used to make measurements, such as a telescope in a spectrometer. This latter discussion, however, does not enable us to blur the distinction between 'naked-eye star' and 'telescope star.'

It seems, then, that we can distinguish at least two kinds of
instruments, viz., those that detect phenomena and those which measure phenomena. Telescopes, eye glasses, microscopes, and electroscopes are that kind of instruments which enable us to detect phenomena, e.g., telescope stars and microanimals. On the other hand spectrometers, ammeters, micrometers are that kind of instruments which enable us to measure what we detect, i.e., to infer unobservable properties from phenomena.

In defending Carnap's analysis thus far we have come to the following conclusion: a property which can only be ascertained through measurement is an unobservable property. One can determine whether a property is observable by noting whether he can determine the property without an instrument that measures it.

In the light of this analysis we can evaluate other criticisms which Spector presents against Carnap. First of all, Spector presents the example of a physicist who places his arm near a body and observes his arm hairs stand on end. Spector claims that in this manner the physicist is observing an electric field without any instrument and, furthermore, (apparently by how fast his arm hairs rise) that the physicist can tell roughly how strong the field is. Thus with practice, he could predict approximately what a galvanometer would read if placed in the field. In defending Carnap we have to note that it is only by means of an instrument that the physicist can correlate his hair raising with an electric field; and it is only with an instrument that he can correlate the degree of hair raising with the strength of the electric field. So whatever he is measuring is measured ultimately in terms of an instrument.
A second criticism is based on observing the wind blowing. We can determine by sound and tree movement how strong the wind is blowing, and although an instrument is required to determine its exact velocity, "the wind would not be classified as unobservable for this reason."14 Yet on Carnap's analysis it is not the wind that is an unobservable, it is its velocity as measured by an instrument. Winds were observed long before there were any instruments.

In a later article Carnap argues that the language of science has two parts: the observation language and the theoretical language.

The observation language uses terms designating observable properties and relations for the description of observable things or events. The theoretical language . . . contains terms which may refer to unobservable events, unobservable aspects or features of events. . . .15 To his list of unobservables, Carnap adds psychological drives and potentials and what he calls 'observable relations' between observable properties of events or things, such as 'x is warmer than y' and 'x is contiguous to y.'16 Carnap notes that physicists call quantitative magnitudes such as temperature, velocity, and force observables whereas the philosopher (such as Carnap) would hold them to be theoreticals.17 Again the reason he gives for the physicist calling such properties 'observables' is that their values can be fairly simply and

14 Ibid., p. 16.


16 Ibid., pp. 38-41.

17 Ibid., p. 49; Carnap, Philosophical Foundations of Physics, pp. 225-226.
On the other hand, he notes that the physicist would not claim that the mass of either an electron or any molecule is observable because of the complicated measuring techniques involved in determining their mass.

The problem that Carnap attempts to solve, at this point, is the one of determining a criterion of significance for theoretical terms. As noted before, a theory must have empirical or observational import to be a scientific theory. It is obvious that the theory's observational terms and observational statements have empirical import. But how do theoretical terms and theoretical statements acquire empirical meaning? The answer, according to the traditional view, is that theoretical terms and theoretical statements acquire such meaning partially by their connection (via correspondence rules) to observational terms and observational statements. Carnap here attempts to explicate and formalize the thesis of partial interpretation.

Let us call the total language of science \( L \), and its two parts \( L_o \), the observational language, and \( L_t \), the theoretical language. Carnap characterizes \( L_o \) in the following manner: (1) all its primitive terms are observationally interpreted, (2) the values of its variables are observables, (3) the non-primitive terms are explicitly definable (ultimately in terms of the primitives), (4) it is finite, (5) all of its variables have a designating expression, and (6) all of its logical connectives are truth-functional.\(^1^9\) \( L_t \) on the other hand, without its

\(^{1^8}\)Carnap, Philosophical Foundations of Physics, pp. 226.

C-rules (the correspondence rules) is empirically uninterpreted. Thus before the C-rules are added to the theoretical statements the first five characteristics above do not apply to it. (Carnap accepts truth-functional logic for all of L.) As for the C-rules, Carnap holds that they can be considered either as rules of inference or as postulates of the theory. Since 'to accept a theory' means for Carnap 'to use that theory in prediction from what has been observed,' C-rules must be of a proper form to facilitate predictions of individual events. Let 'C' be the conjunction of the C-rules of a theory, 'T' be the conjunction of all the theoretical statements of a theory, and 'O' be the conjunction of all the observational statements of a theory. The theory would be the conjunction of 'C' with 'T' and 'O.'

On the basis of the above concepts, Carnap explicates a criterion of significance for theoretical terms. Let 't' be a theoretical term and 'S_t' a statement about the theoretical property or entity t; let 'o' be an observational term and 'S_o' a statement about the observable property or entity o. In order for 't' to be empirically meaningful 'S_t · T · C' should either entail or inductively support 'S_o.' However, in order for 't' to have empirical meaning, 'S_t' must be essential for the derivation of 'S_o' from the theory. To insure empirical significance for 't' Carnap holds that 't' must be the only descriptive term in 'S_t.'

Furthermore, if a set of theoretical statements of this form are needed in the derivation of 'S_o,' then they must have been shown to have empirical meaning individually in serial order; i.e., the first

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\(^{20}\)Ibid., pp. 49-52.
term given empirical meaning must be directly connected with a \(C\)-rule, the next term given empirical meaning can either be connected indirectly with the first term already established or directly with another \(C\)-rule, and so on until we get to 't.' Thus in deriving 'S_o' we will not be confirming or giving empirical meaning to the conjunction of such statements, but only to 't' via 'S_t.' On the basis of this explication Carnap presents a criterion of significance for theoretical statements. According to Carnap, a theoretical statement is empirically meaningful if it satisfies the rules of formation for \(L\) and all its descriptive terms are empirically meaningful in the above sense.\(^{21}\) These two criteria together are supposed to be adequate for determining the empirical significance of theoretical terms and statements.

Is this claim accurate? It is, of course, understood that all of the statements in a theory are logically consistent. First of all, let us consider the requirement for 'S_t.' Carnap says that 't' should be the only descriptive term in 'S_t.' Now the only statements having such a form of which I know are either existential claims, such as 'There are electrons,' or singular statements, such as 'This is a chair.' Since 'S_t' is theoretical, it will assert that either a theoretical entity or property exists or that some specific entity is an entity of a certain kind or has a property of a certain kind. It seems that 'S_t' will have to be a statement of a theoretical initial condition. (It is possible that 'S_t' be of the form '(x)Fx' or '(x)-Fx' but such statements, if true, seem to be too metaphysical, e.g., 'everything is material' or 'nothing is material': the theses of

\(^{21}\)Ibid., p. 60.
materialism and immaterialism, respectively.)

Now Carnap claims that 'S_t' is significant because 'S_o' can be predicted on the basis of 'S_t · T · C.' However, as we discovered in the section on the prediction of an individual event, a necessary condition for such a prediction is that our premises contain at least one statement of observed initial conditions. But 'S_t · T · C' lacks such a statement, since 'S_t' is a statement about a theoretical initial condition, 'T' is a statement or set of statements of the theoretical part of the theory, such as the theoretical laws, and 'C' is the correspondence rule connecting 'S_t' to 'S_o.' It seems that we need another $C$-rule such as 'C_1' which connects 'S_t,' or some other theoretical statement 'S_k,' which in turn is connected to 'S_t,' to the statement of observed initial condition 'S_e.' (The significance of 'S_k' will have to be determined independently of 'S_t' so that the prediction of 'S_o' will indicate the significance of 'S_t.') Thus as it stands, Carnap's explanation is inadequate. To make it adequate for meeting the above objection, we will have to add the qualification that there is another $C$-rule, 'C_1' such that it connects either 'S_t,' or another statement 'S_k' whose significance has already been established, with a statement of observed initial conditions 'S_e.'

An interesting criticism about the form of $C$-rules has been given by John A. Winnie. Winnie argues that if the $C$-rule is of the form '(x)(T(x) ⇔ O(x)),' where 'T' is a theoretical property and 'O' an observational property, then we have violated the theoretical-observational distinction. A statement of this form states that being a T is also being an O. But if T is a sub-set of O, then T must be an
Q, i.e., the theoretical property must be an observational property (which is contradictory to the distinction). Thus rules of that form cannot be used for partial interpretation of theoretical terms.

Although Winnie presents his argument in the context of Carnap's explication and his argument seems to be sound, it is not clear whether it has any bearing on Carnap's explication of the concept of partial interpretation. Even though Carnap talks at times as if the Q- rules have the form Winnie criticizes, e.g., "if we have Q- rules for certain terms ...," he also states that "these rules must be such that they connect sentences of L_o with certain sentences of L_t ... ." This last quotation indicates that Carnap conceives of Q- rules as linguistic links between statements, and only indirectly between terms, due to their being in such statements. In accordance with his formalization of his criterion, Carnap considers Q- rules to connect statements rather than terms, and thus to be statements of the propositional calculus's form, not of quantification. (My previous discussion of Q- rules also construed them as connecting statements rather than terms.)

Winnie offers a Q- rule of the form '(x)(TxO (3y)0x)' as not being subject to his criticism. Yet rules such as these (which have been discussed in the section on prediction of individual events and explanation of laws) will not enable us to predict any individual events.

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24 Ibid., p. 47.

25 Winnie, op. cit., p. 228.
Thus 'S_o' will not be derivable. The consequence of this analysis is that C-rules cannot be of the form '(x)(Tx O x)' and '(x)Tx (S y) O x),' since those of the former violate the theoretical-observational distinction and those of the latter premit singly neither the prediction nor the explanation of an individual event. Having discussed Carnap's attempt to distinguish the theoretical from the observable, we shall now consider Putnam's critique of Carnap.

Section II

Putnam's Critique of Carnap's Explication

Hilary Putnam offers a detailed criticism of Carnap's defense of the theoretical-observational distinction and of his account as to how theoretical terms acquire empirical meaning. Putnam notes that some of the terms Carnap lists as observational, such as 'red' and 'x is contiguous to y,' have been used to refer to unobservables, e.g., Newton's theory that red light consisted of red corpuscles, and saying that one molecule is contiguous to another. Putnam decides that if observational terms are to refer only to observables, then there are no observational terms. One possible way of defending Carnap's view is to hold that, when such terms are used, they are used in the model of the theory. Thus we can construe Newton's claim by saying that he

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27. Ibid.
picted red light as if it were composed of red billiard balls moving rapidly and in fairly close contact. (The latter part of the model could be used in explaining contiguity of molecules.)

Putnam also does not like the identification of 'unobservable' with 'theoretical.' For one reason, he believes that 'angry' and 'love' refer to unobservables, but it is too much an extension of common usage to say that they are theoretical entities. By 'theoretical term' Putnam means a term peculiar to a scientific theory, a technical term apparently. Thus, 'satellite' would be a theoretical term and yet it does refer to observable entities. Carnap, as Putnam notes, could reply to this example by saying that one cannot without instruments determine whether or not an object is a satellite. Perhaps that claim is correct.

Putnam's second major criticism concerns the concept of partial interpretation. Putnam examines three possible meanings of that notion. (Due to the fact that the second meaning has a bearing on descriptivism I shall present it when I consider descriptivism.) The first sense of 'to partially interpret a theory':

is to specify a non-empty class of intended models. If the specified class has one member, the interpretation is complete; if more than one, properly partial. Putnam believes that this notion cannot help Carnap because one must know the meanings of the theoretical terms in the theory before one can establish any kind of model for the theory. Putnam has in mind model. But to this criticism Braithwaite, for instance, could reply: if one

\[\text{28}^\text{Ibid., p. 243.}\]

\[\text{29}^\text{Ibid., p. 245.}\]
did know the meanings of the theoretical terms, then a model would be superfluous. A model, for the traditionalist, is designed to clarify a theory. Furthermore, by appeal to familiar objects, their properties, and relationships, a model is formulated to explain the phenomenon under investigation. But in being a model it is assumed to be an approximation to the situation. On the basis of the model, a theory is formulated by analogy; but, since a theory is supposed to accurately represent the situation governing the phenomenon, it will be completely interpreted only to the extent that the model has been directly confirmed. The lesser or indirectly confirmed parts of the model will not carry over completely into the theory due to possible modifications later in the model. Thus we need not accept this part of Putnam's criticism of the first sense of 'partial interpretation.'

The second part of Putnam's criticism of the first sense "is that theories with false observational consequences have no interpretation." Apparently Putnam believes that, according to this sense of 'partial interpretation,' a theory is interpreted only if it has true observational consequences. However, the traditionalist only need hold that a theory have observational consequences regardless of their being true or false. Even if its model specifies a non-empty class of observable entities, it does not follow that the observable entities used to interpret the theory will be found to have all the properties the theory predicts for them. Thus if a theory implies a statement of the form \( Fa \supset Ga \), then the theory is partially interpreted, in this first sense, if only there are observable \( F \)'s, not if there are \( F \)'s

\[ ^{30} \text{Ibid., p. 247.} \]
which are G's.

According to sense three:

to partially interpret a formal language is to interpret part of the language (e.g., to provide translations into common language for some terms and leave the others mere dummy symbols).  

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Putnam's criticism of this notion is that it implies that theoretical terms are only linguistic inferring devices and have no factual content. This view is, of course, instrumentalism and Putnam, without argument in this essay, says that it is unacceptable. Whether Putnam is correct in this claim remains to be seen.

Putnam's third and final criticism is directed against Carnap's account of introducing theoretical terms into the scientific language. Just what is the problem, Putnam asks, that the thesis of partial interpretation is supposed to solve?  

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As we noted with Carnap's thesis, observational terms are ostensively and explicitly defined in terms of empirical phenomena, but theoretical terms are not. Since the theoretical statements of a theory in conjunction with observational statements of initial conditions imply other observational statements of predicted conditions, theoretical statements are more general than observational statements and, consequently, cannot be explicitly defined by observational statements (on the assumption that a theoretical statement is not merely a finite conjunction of observational statements). According to Putnam, if the problem is to give the meanings of theoretical terms using only observational ones (or more specifically, using only Carnap's

31Ibid., p. 246.

32Ibid., p. 248.
observational terms), then the task cannot be done.\textsuperscript{33}

The preceding problem is, however, not the problem of Carnap, as explained above. According to Carnap \( \mathcal{C} \)-rules are essential in connecting theoretical statements to observational statements. In examining another possibility, Putnam says that perhaps the problem is how theoretical terms are learned. Putnam believes that they are learned in the same way that other terms are learned: from their uses.\textsuperscript{34}

Although Putnam seems correct here, Carnap's problem is not how one learns such words but how they enable us to predict and explain phenomena.

Finally, Putnam argues that some theoretical terms can be explicitly defined in terms of observational terms. According to Putnam, 'elementary particle' is definitely a theoretical term, but it can be explicitly defined as follows: "X is an elementary particle if X cannot be decomposed into parts Y and Z which are not contiguous. . . ."\textsuperscript{35}

Putnam believes that '. . . is a part of ___' and '. . . is contiguous to ___' are observable predicates, according to Carnap. Yet if we hold that only those descriptive terms in science which refer to measurements or measurable magnitudes are theoretical predicates, then Putnam's criticism does not apply.

In my examination of Carnap's explication, I decided on his grounds that measurement predicates (of science) are theoretical or unobservable terms. Whether other terms, such as 'electron' and 'atom'
are observable or unobservable has not yet been decided. We would have to examine the theory which contains such terms and determine the relationships statements containing them have to statements predicted by means of the theory. Thus, in Carnap's explication, two possible ways arise of determining whether a term is observable or theoretical: (1) if it refers to a measurement or magnitude, it is theoretical and (2) its place and function in the theory; if it never occurs in a prediction of any established theory, the term is not an observational term. (Trivial inference in the deviation of the prediction, such as addition or its equivalent, are of course ruled out.)

Carnap's explication does not seem to have been severely damaged by the criticisms just discussed. With regard to the second possibility above, we only have inductive evidence that a term is theoretical, since it may later occur, with the development of the theory, in a prediction. But insofar as a theory has been formulated, we can decide by means of logic which of its terms are at this time definitely theoretical and which are definitely observational.

To clarify the last point, consider a theory having statements 'T_1', 'T_2', 'O_1', 'O_2', and \( \mathfrak{C} \)-rules 'C_1' and 'C_2'. From 'O_1' and 'C_1' we can derive 'T_1'. From 'T_1', 'T_2' is derived; and from 'T_2' and 'C_2', 'O_2' is derived as a prediction. Thus all the non-logical terms contained in 'O_2' will be observational terms. Furthermore, assuming that it is possible to independently verify 'O_1' and 'O_2' from '-O_2' and 'C_2', we can derive '-T_2'; and from '-T_2', we derive '-T_1'; and from '-T_1' and 'C_1', we derive '-O_1' as another prediction. Thus all the non-logical terms contained in 'O_1' will also be observational. Thus
'O₁' and 'O₂' will be observational statements. If any of the other statements contain some of these observational terms (the $\Xi^*$ rules definitely will since they contain either 'O₁' or 'O₂') but other non-logical terms not contained in them, then those other terms will be theoretical terms if 'O₁,' 'O₂,' and their contradictories exhaust the predictions possible from the theory. With Carnap's explication still in mind, let us turn now to Hempel's.

Section III

Hempel's Presentation of the Distinction

As with Carnap, Hempel divides the non-logical vocabulary of science into two parts: (1) the observation(al) or experimental terms and (2) the technical or theoretical terms. According to Hempel, observational terms and statements describe data which are "obtainable by direct experience and which serve to test scientific theories or hypotheses." The data in question should be publicly observable to insure the objectivity of scientific knowledge. What especially should be inter-subjective is the testing of theory. According to Popper, with whom Hempel agrees on this point, to insure inter-subjectivity of testings, the testings should be as simple as possible.

The other terms in scientific theories are called theoretical constructs. Dispositional and metrical terms (i.e., measurement terms) are different kinds of theoretical constructs. With Carnap, Hempel

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37 Popper, The Logic of Scientific Discovery, p. 104.
holds that such terms cannot be explicitly defined by means of what we observe (Hempel adds 'directly observe'). In another essay, Hempel claims that observational terms refer explicitly "to things and events which are ascertainable by direct observation" and that theoretical terms refer to such entities which are not directly observed. Again, in a third essay, he says that an observational term is any term that: (1) refers to an observable property (such as Carnap's examples above), or (2) "names some physical object of macroscopic size." In another book he distinguishes the theoretical from the observable on the basis of the fact that the entities and properties of the former cannot be directly observed or measured as can those of the latter.

Here, however, Hempel adds that the non-logical terms of the statements used in testing a theory (i.e., of its predictions) have to signify:

things and occurrences with which we are antecedently acquainted, which we already know how to observe, to measure, and to describe.

From this quotation one might infer that the non-logical terms of the testing statements are observational terms and that observational terms are those terms of a theory which are known and used prior to the theory and independently of it. Yet Hempel holds that some of these terms can

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40Hempel, *Philosophy of Natural Science*, p. 73.

41Ibid., p. 74.
be dispositional or metrical provided that they are well understood and agreed upon by the scientists. It should, however, be added that terms such as these which appear in the prediction will have been already interpreted in other theories by means of test statements that are observational in Hempel's and Carnap's sense. Hempel, otherwise, seems to agree with Carnap that a theory is an elaborate system of statements, and of terms some of which are explicitly definable by means of observation terms, whereas others are only partially interpreted because of their relationships in the theory with those terms that are explicitly definable.\(^2\) (Braithwaite also, as noted previously, accepts this thesis.)

In comparing Hempel's presentation of the distinction with Carnap's, it should be noted that there seems to be little if any difference. By 'observing something directly,' apparently Hempel means observing it without the aid of an instrument. Thus something which needs an instrument for its detection or discovery would be a theoretical entity or property. However, there are some problems that arise with this concept of Hempel's. May it not be the case, as Achinstein suggests, that whether one needs an instrument to observe or to detect something depends on the experimental situation? Achinstein argues that anything that is held to be unobservable under one condition can be shown to be observable under another condition; thus there is no general criterion that is applicable to all terms which enable us to decide whether they are observable or unobservable.\(^3\)


Is it possible, then, to observe or detect something without an instrument that would require an instrument in another situation? It might seem to be the case. If we were on a mountain top, then a lake which is a hundred miles away could only be detected by means of a telescope. Yet if we were closer to the lake, we could detect it without any instruments whatsoever. Of course we can save the theoretical-observational distinction (on this basis) by relativizing it to the experimental situation. Almost every term referring to entities or properties would then become both theoretical and observational, though of course under different situations. But what if an instrument permits us to detect or observe something we cannot observe under any known conditions without the instrument? Could we not then claim that we have found a counter-example to Achinstein's criticism?

As an example consider the nuclei of micro-organisms. It is only by means of a microscope that we can observe such entities. Could they not, therefore, be theoretical entities in a non-relativistic and non-contextualistic sense (i.e., under every context in which we detected their existence we have to use instruments)? Of course one might attempt to answer that certain observable macroscopic conditions in an animal, for example, are signs that the animal has present within its system certain micro-organisms. (This example is analogous to Spector's physicist who detects electric fields with his arm hairs.) To this answer it can be replied that initially an instrument was essential in establishing that certain macroscopic conditions are signs of microscopic entities. Thus, in this sense, micro-entities cannot be observed or detected without instruments. Furthermore, if instruments
are essential for the observation and detection of micro-entities, then it seems to hold also for those entities which are submicroscopic—e.g., the atom and its constituents.

One may wonder if there is any difference in meaning between 'observing $x$' and 'detecting $x$.' There does seem to be a difference. 'Detecting $x$' seems to imply that one is observing a sign or symptom of $x$, such as something that has been observed in conjunction with $x$ (e.g., since smoke has been conjointly observed with fire, it can be used to detect the existence of fire.) But are we justified in claiming that we have detected $x$ because we have observed $y$ and yet we have never observed their conjunction since $x$ is a theoretical entity? Perhaps we are if $x$ is described in a well confirmed theory that predicts a statement describing $y$.

Achinstein presents a number of examples to show that under different conditions the same thing can be considered as either observed or unobserved. One such example involves the context of a cloud chamber experiment. According to Achinstein charged particles, such as electrons, leave visible tracks in cloud chambers whereas neutral particles, such as neutrons, do not. Now the question is: When we observed a track in a cloud chamber, have we observed an electron or have we only detected one in the confirmed-theory sense above? We have observed a track. Are electrons certain kinds of cloud chamber tracks? Surely electrons, if they exist, have existed prior to cloud chambers and, therefore, prior to cloud chamber tracks. But could it not be the case that under certain conditions, such as a cloud chamber experiment,

\[\text{Ibid.}, \ p. \ 195.\]
electrons are cloud chamber tracks? Or is it the case that under those conditions electrons appear as cloud chamber tracks? The difference between these two alternatives is that in the latter the electron is the cause (at least a necessary part of it) of the track, but is not identical to it as in the former. Thus the track could be used as a detector of an electron, but not as an observation of it. Since, however, both alternatives are limited to an experimental context, the difference between the alternatives seems to be one of preference.

It is not clear as to what the atomic theory predicts for such an experimental context: Does it predict (1) that one will observe an electron as a track, or (2) that one will observe an electron that is a track. Though the difference seems minimal between them we have indirect inductive evidence for the mode of speech in which (1) is expressed. According to the conjunction of the atomic theory and micro-biological theory, we can infer that electrons are much smaller than micro-organisms. Furthermore, micro-organisms can only be observed via instruments of magnification. Thus, if \( x \) is smaller than \( y \) with respect to observer \( P \), and \( y \) needs to be magnified in order to be observed by \( P \), then \( x \) will need to be magnified even more so that \( P \) can observe it. Yet cloud chambers are not instruments of magnification. Hence subatomic particles are not observed by means of cloud chambers; they are, at most, only detected by them. Consequently, contrary to Achinstein's contextualistic claim, it seems that we have a scientific justification for (1) as against (2). They are not equally justified modes of speech.

Another macroscopic example Achinstein uses is the following. He
is asked what he is observing on the dirt road ahead. He claims that he might, in one and the same situation, claim to be observing a car, a trail produced by a car, or just a cloud of dust.45

Achinstein holds that the alternative he decides to use depends on the extent of his knowledge about such situations and the type of answer he believes will interest his questioner.46 For example, if he knows that there are very few horse-drawn carriages in the area, he will not answer that he sees such a carriage. Due to its size (i.e., the cloud of dust), he will say he is observing either a motorcycle or a car. Achinstein also claims that whatever answer he gives (such as any of the three above in his quote) none of them are necessarily "imprecise, inaccurate, ambiguous, or . . . untoward."47 If by 'necessarily' he means 'analytically,' then Achinstein of course is correct. But even in the context, is it not the case that one of the alternatives is inductively preferable to the others?

Observing a car is not the same as observing a trail produced by a car, and neither of these in turn is the same as observing a cloud of dust under any observational contexts. They are logical distinct activities. I believe that Achinstein is correct in saying that whatever answer is given will depend upon the respondent's knowledge; however, I deny that they are equally justified. If in his example, a car is not observed by the respondent but a cloud of dust is, then the third alternative is the only justified one. If he observes both a car and

46Ibid., p. 194.
47Ibid.
the cloud of dust in spatial contiguity, then the second alternative is justified. And finally, if he is only observing the car (or he is observing both the car and cloud of dust but they are not spatially contiguous), then the first alternative is justified. In short, what one may answer depends on what one observes.

If, however, it is claimed in defense of Achinstein's argument that we are considering how to interpret what we are observing, one can reply by saying that if all three alternatives are equally sound interpretations, the situation is hopelessly ambiguous. But Achinstein denies that the situation is ambiguous. Therefore, either not all three alternatives are equally sound (contrary to Achinstein) or we are not considering how to interpret what we are observing. This latter alternative seems to be acceptable to Achinstein because he holds that 'observing' does not imply 'recognizing.'

According to Achinstein 'observing x' implies 'attending to various properties or relations of x.' For instance, staring at a wall is not observing it unless I am aware of its cracks, color, and relations to other objects. Achinstein holds that an "airplane watcher may observe jets flying overhead, though all that is visible are white trails." But if the watcher does not see the jet, hear it, touch it, smell it, or taste it, is he attending to it? He is if the white trail is a feature of it. But if the trail is distinct from the jet can it still be a feature of it?

49 Ibid., p. 160.
50 Ibid., p. 161.
Still another example he uses is observing corn in the field even though all that is seen are husks. But there seems to be a significant difference between these examples. Husks are parts of the corn plant, but are trails any part of a jet? They do not seem to be. Trails are not like the color, cracks, and texture of a wall (which are examples Achinstein gives for aspects or features of a wall). Since only the trail is visible we do not observe any significant relations of the jet to anything else. It would thus seem that the watcher observes the trails and uses them as indicators of the jets (he detects the jets).

An analogous criticism can be used when Achinstein claims that he "can observe the temperature of a substance by looking at the thermometer in contact with it." What he observes is the thermometer which enables him to detect the magnitude of the substance's temperature.

It appears, therefore, that there is some plausibility in distinguishing the theoretical from the observable by insisting that the former are detected only by means of instruments (at least initially). Furthermore, it does not seem to be the case that the distinction is so contextualistic as Achinstein claims. In making his claim Achinstein does not distinguish between what we in fact do say we observe and what in fact we do observe. The criterion for making the distinction enables us to determine whether we are justified in claiming that we have observed something. Thus it appears that under no known context (experimental situation) are we ever justified in claiming that we have observed microscopic or submicroscopic entities or properties (if our criterion is based on using of instruments--man-made devices). At most

\[51^{\text{bid.}}, \text{p. 163.}\]
we observe entities or properties which are signs of the microscopic and submicroscopic (via theory and analogy of macroscopic entities and their observable relationships).

I am now ready to criticize Hempel's criterion. It seems to be contrary to common usage to say that we do not observe things with telescopes and microscopes (let alone eye-glasses). One could argue that we only observe the images of things with such instruments, not physical objects. It is due to the visual similarities between what we observe with the naked-eye and what we observe with the telescopes that we can identify telescope imagery. The problem with accepting this argument is that one could continue using it to argue that the only things we observe with our sense organs are images. This line of thought thus approaches representative and/or critical realism, i.e., the view that we never observe physical objects, only sensations or images that they produce in us as they affect our senses. It seems, therefore, that if we hold that whatever is known only by means of an instrument is a theoretical entity, everything known becomes a theoretical entity. One way of stopping such an argument is distinguishing, as I did in Carnap's discussion, between instruments that extend our sense capabilities and those which we use for measuring. Only the latter detect theoretical entities.

Section IV

Nagel's Presentation of the Distinction

According to Nagel laws express an order or connection between observable events, and the final or ultimate test for these laws is
Nagel broadly divides laws into two kinds: (1) experimental laws and (2) theoretical laws.

Experimental laws

formulate relations between things or features of things that are commonly said to be themselves observable, whether with the unaided senses or with the help of special instruments of observation.53

On the other hand theoretical laws formulate relations between things or their features that are not so observable. Apparently Nagel disagrees with Hempel by holding that observables have macroscopic size.

After this brief introduction, Nagel considers some problems with the distinction and attempts to justify it. First of all, as we have already noted, observable and theoretical are rather unclear concepts. Nagel claims that the experimental laws are not statements about connections between sense data; since they are universal they go beyond anything given in experience. Thus experimental laws are not about the observable in the sense that they are about sense data. To put Nagel's point in another way, laws do not have the form of singular statements like observational statements, but they do express some sort of invariance between events whether those events are being observed or not. Furthermore, as we have discovered in the discussion on direct and indirect confirmation, most non-logical terms of a law can also be found in other laws. (How else could a set of statements be interconnected through indirect confirmations?) Also in testing a law, we assume, for instance, that our instruments are reliable and tacitly

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52 Nagel, op. cit., p. 79.

53 Ibid.
assume that the laws describing their behavior are correct. Furthermore, in deriving a quantitative property (measurement) we will have to average the measurements derived from a series of similar experiments to determine its value approximately. Such averaging assumes the law of experimental error in experimental testings to account for the differences between each experiment of the series.

Perhaps the most forceful criticism of the distinction is that what are held to be experimental laws "include as part of their meaning assumptions that are admittedly theoretical." Yet one might be inclined to react against this criticism by saying that the supposed experimental law is actually a theoretical law. In any case Nagel believes that items such as the above have raised doubts as to the justification of the distinction. He himself holds that there is no precise criterion available that permits of a precise distinction between the theoretical and the observable. Nevertheless Nagel believes that lack of precision is not sufficient to dismiss the distinction—in fact, he believes that there are certain characteristics that enable us at times to make the distinction.

First of all Nagel claims that each non-logical or descriptive term of an experimental law

is associated with at least one overt procedure for predicing the term of some observationally identifiable trait when certain specified circumstances are realized.

Such a procedure usually only partially determines the meaning of the observational term, because most terms have several different experimental procedures for their predication. For example, 'strength of an

\[54\text{Ibid.}, \ p. 83.\]

\[55\text{Ibid.}, \ p. 83\]
electric current' can be given observational meaning by the reading of an ammeter, by measuring the amount of silver deposited from an electrolytic solution for a period of time, and by noting the temperature rise through temporal intervals for a particular substance. Nagel believes that in situations like these one of the procedures is taken as the standard defining the observational term.

In such procedures, one should employ them without assuming the law that is being tested. Thus, if $P$ is a term in $L$, then the observational procedure for determining the meaning of $P$ should be made independently of $L$. This condition is essential for $P$ having genuine empirical content. Such a procedure also permits having direct confirmation for experimental laws. Yet at this point we should remember that laws are not isolated statements. Thus $P$, for instance, will probably occur in other laws as well.

Let us now ask: When we determine a procedure for $P$, should we assume neither $L$ nor any of those other laws? In answering this question let us consider as $P$, 'the strength of an electric current' and its laws, as 'the ammeter law' ('A-L'), 'the deposit per time law' ('D-T'), and 'the temperature per time law' ('T-T'). In order for 'the strength of the electric current' to have empirical import in 'A-L' it must be determined independently of its meaning and occurrence in 'A-L.' But this procedure does not rule out assuming 'D-T' or 'T-T' in establishing empirical import for 'the strength of an electric current.' But what if we want to determine its meaning independently of all three laws? In that event, we would have to establish a procedure that does not assume any of them. Yet if all the laws are interconnected, is
such an independent procedure possible, i.e., can we establish a procedure for \( P \) which is independent of all of them, or do we have to establish one procedure as the defining standard? Nagel does not consider this question. Perhaps the best thing to do is to set down all the assumptions one has in establishing a procedure for \( P \) and see if any of these assumptions are the laws. It may be the case that the assumptions are more general than the laws and the laws are derived from them. If such is the case, then are we assuming the laws when we establish a procedure for \( P \) on these assumptions? We would be if the assumptions (or a part of them) entailed the law or laws. However, if the assumptions do not entail them, then even if one or more of the assumptions are used in deriving the laws, \( P \) would have empirical content with respect to the laws in question.

There may, nevertheless, still be a problem from another point of view. Let us assume that the laws are of the form \( (x)(Fx\supset Gx) \). What will be the form of the statement describing the observational procedure? If it is 'Fa' or 'Ga,' then, since statements of the latter form are instances of the statements of the former, how can it be correct that the procedure has a meaning independently of the law? The way of avoiding this criticism is to deny that in determining the meaning of an instance of a law one has to initially assume the law. It seems logically possible to determine whether something is a man, even though one does not know what a mammal is. Thus in such a procedure one would not be assuming 'All men are mammals.' In fact, this possibility must be actual if the law is to be non-analytic.

From Nagel's discussion it follows that the descriptive terms of
theoretical laws are those for which no experimental procedures can be given for applying them. Consequently, theoretical laws cannot be directly confirmed. A corollary of the preceding discussion, according to Nagel, is that whereas experimental laws can be derived by means of inductive generalization from observed data, their relations, and testing procedures for their terms, theoretical statements cannot be so generalized.\textsuperscript{56} Examples which Nagel uses for experimental laws are the gas law and Galileo's law for freely falling bodies. Even though some experimental laws are derived from indirectly confirmed theoretical laws, Nagel holds that an experimental law (unlike a theoretical law) is not considered established until it has been directly confirmed.

Even though theoretical laws are used to explain and sometimes derive experimental laws, the meaning of experimental laws can be formulated independently of their theories and can even survive after their theories' refutation. As an example Nagel cites Balmer's law which was initially explained by the Bohr theory and later by the quantum theory\textsuperscript{57} Nagel argues that if an experimental law and its terms did not have any meaning independent of their explaining theoretical laws, then the theoretical laws would actually have nothing to explain. Nagel admits that the meaning of the experimental laws and their terms may be dependent on some theoretical laws, but the former cannot be used to explain the latter. In contrast, theoretical terms and laws cannot be understood apart from the theory in which they are contained, since they acquire their empirical meanings through the interconnections with the other

\textsuperscript{56}Ibid., p. 85.
\textsuperscript{57}Ibid., p. 87.
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terms and statements of the theory. If these interconnections are altered, then so is the theory but not the experimental laws and terms.

Let us now summarize Nagel's presentation. With regard to terms, an observational term is any descriptive term whose meaning is wholly or partially determined by an observational procedure for using it. Theoretical terms are those descriptive terms of a theory whose meanings are determined only partially and by means of their position in the theory—by their interconnections in statements or rules with other statements of the theory. Their empirical meaning is established by means of their connections with the observational terms. With regard to experimental laws (general observational statements), only these statements of a theory may be inferred by means of inductive generalization from statements about phenomena. Theoretical laws (general theoretical statements) are only indirectly confirmed; yet they explain and predict new experimental laws. Furthermore, if the set of theoretical statements \( T_1 \) and \( T_2 \) explain an experimental law \( O_1 \), the meaning of \( O_1 \) can be established independently of \( T_1 \) and \( T_2 \), but the empirical meaning of \( T_1 \) and \( T_2 \) is dependent on \( O_1 \). The best way of determining whether a term is observational or theoretical, or whether a statement is observational or theoretical, is by considering its place in a theory and how it acquires its meaning in relation to the other terms and statements of a theory. In this sense, meaning is theory-dependent for the traditionalists.

To understand this view of theory-dependence of the meaning of theoretical terms, perhaps an analysis similar to Gilbert Ryle's analysis will help. According to Ryle 'straight flush' is a 'Poker-
laden' term; i.e., it acquires its meaning in accordance with the rules or theory of the card game Poker. On the other hand, 'Queen of Hearts' is not theory-laden with respect to Poker, but to the card game theory in general. Thus in explicating 'straight flush' one can use some of the playing cards whose meaning is established by means of the rules of interpretation (the card game theory, in general, which explicitly names each card). As to the values and meanings of the card relationships, one must appeal to the rules of the specific card game Poker. Thus, 'Queen of Hearts' is not Poker-laden and not Bridge-laden (i.e., one can use 'Queen of Hearts' without knowing how to play either Poker or Bridge). Similarly, 'red,' though appearing in light wave theory, qualitative chemical analysis, and genetic theory, acquired its meaning independently of any of those mentioned theories. Since 'playing card' can be used apart from 'Queen of Hearts' which can be used apart from 'straight flush,' there seems to be a hierarchical series of meanings in card games just as Carnap, Hempel, and Nagel have advocated for terms occurring in scientific theories. Just as one must know how to identify the individual cards to play any card game, so too one must know how to apply observational terms to experimental techniques before one can use the theories for explanation and prediction.

As with Carnap's and Hempel's explications, criticisms have been directed against Nagel's presentation. First of all, Leo Simons mentions (as we noted earlier in Chapter I, Section III) that concepts

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such as 'instantaneous velocity' and 'ideal gas' are not observable according to Nagel. Yet in giving examples of experimental laws, Nagel presents us with Galileo's law for freely falling bodies which uses 'instantaneous velocity' and the gas law which applies only to ideal gases. Both of these concepts as well as others involve what Simons calls an extrapolation to a limit of what is observed. It appears that Simons is correct. The question is whether these concepts are to be considered as observational or theoretical. Both of these laws are quantitative rather than qualitative. If Nagel had given 'copper contracts when heated' as an experimental law, Simons' criticism would not apply because it is a qualitative law. In the light of this problem as well as of the comments on Carnap's and Hempel's explication, it would probably be best to consider all quantitative laws as theoretical. Of course one can give rules for applying these concepts to explanatory models, as is frequently done, but they cannot be applied to entities or properties in an actual experiment without assuming the model. For example, with the model in mind we can determine the instantaneous velocity of a rock we intend to drop; and we can determine to what extent the gas in the laboratory is an ideal gas. To repeat a point I made earlier: we derive, infer, or make measurements on the basis of what we do observe. 'Instantaneous velocity,' 'ideal gas,' 'area,' 'volume' are not the sorts of things we perceive or attend to.

The second criticism of Simons is again based on 'instantaneous velocity.' If this term is a theoretical term and since it occurs in

Newtonian mechanics, it should not be defined independently of that theory. But, Simons notes, it is explicitly defined in terms of distance and time. Although 'distance' and 'time' have meanings apart from Newtonian mechanics, the way velocity is defined in terms of them is not independent of Newtonian mechanics—the definition is one of its postulates. ('Distance' and 'time' in Newtonian mechanics proper are given specialized interpretations by means of Newton's doctrine of space and time.) Apparently Simons believes that on Nagel's account theoretical terms cannot be given explicit definitions. But this belief is incorrect. What Nagel holds is that theoretical terms cannot be given complete empirical meaning by means of observational procedure. Thus, Simon's second criticism is unsound.

A third criticism Simons presents is that theoretical statements, because they are connected by C-rules to observational statements, should also be directly confirmed as observational statements are. Yet such is not the case. As we have seen, the observational statement or law must be of the form '(x)(Fx\[\supset]Gx)' to be directly confirmed. Since only statements of the form 'Fa\[\supset]Ga' confirm the experimental law, the theoretical law would have to be identical to it to be also directly confirmed. Since such identity eliminates the need for the C-rule, Simons' third argument is also unsound.

Other criticisms of Nagel's presentation have been presented by Hesse. Contrary to Nagel, Hesse believes that theoretical statements

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60 Ibid., p. 164.
61 Ibid., p. 165.
62 Ibid., p. 167.
have observable instances. As an example she gives the cloud-chamber-example. To this example we can reply in defending Nagel (as we did earlier in defending Hempel) that the track enables us to detect the electron, not observe it. Hesse then asks: How is the cloud-chamber experiment relevant to testing the atomic theory if the theoretical statements have no observable instances? But one can reply by showing her how the theoretical statements become indirectly confirmed due to the confirmation of the observational statements.

One interesting criticism of Hesse's is what she believes to be an inconsistency in Nagel's thought. He makes the claim that today most educated people accept the heliocentric theory and, even though they use terms such as 'sunrise' and 'sunset,' they use them with meanings different from the meanings for those words used by the geocentric advocates. If 'sunrise' and 'sunset' represent observational terms, then it appears that a change in theory has produced a change in some of its observational terms. In defending Nagel, one can say that the phenomena designated by 'sunrise' and 'sunset' are undoubtedly the same for both the heliocentric advocate and the geocentric advocate. Thus the empirical procedures used by both in applying the words are also the same. Yet what is different is their explanations of the agreed upon phenomena. If they did not agree upon the phenomena, then these theories could not be in such well-known disagreement.


64 Ibid.

65 Nagel, op. cit., p. 604.
I have now portrayed the traditionalist's presentation and defense of the theoretical-observational distinction. We have found that the distinction can best be defended in the context of a theory rather than by appealing to isolated terms or isolated statements. Of the descriptive terms found to be theoretical, only the metric ones seem to be definitely theoretical (dispositional terms will be discussed later). Secondly, if a property of an entity can only be decided on the basis of elaborate experimentation and instrumentation, then that property may be considered as theoretical. Yet it seems to be the case that most properties that require such elaborations are also metrical.

It appears that the two best criteria for making the distinction are the following. First, an observational term or statement is that term or statement of a theory whose meaning can be understood independently of that theory. And, secondly, the only descriptive terms occurring in the prediction of an individual event in accordance with a theory are observational, and the prediction is itself an observational statement. Such statements are of the form 'Fa' or 'Fa•Ga.' If one formalizes a theory, most of the statements of the preceding form will either be predictions or explananda. To insure that the statement in question is an observational statement, one must appeal to the first criterion. Thus it seems that, ultimately, the first criterion is the most important one in establishing the theoretical-observational distinction. The distinction can only be made within the context of a theory.

For example, if the game of chess were our theory, i.e., if we formalized all of the rules and moves, then, after identifying the
chessmen, our rules of interpretation, a class of appropriate moves can be predicted and explained in accordance with the chess theory and statements of initial board conditions. These predictions and explanations would be "intelligible" to someone who did not know how to play chess; however, not knowing the theory, he would neither be able to explain nor predict the moves. And yet it seems possible that, after observing some chess games, the layman could begin to formulate some of the chess theory himself. Such a formulation will help him explain the chess moves made so far and, hopefully, will enable him to predict further chess moves. If the new chess moves agree with the predictions, the formulations are kept. If they do not, then the formulated rules will have to be modified in accordance with the data and then retested. And yet in both cases, whichever theory one would be considering, the statements describing the phenomena may be understood apart from them, but not their explanatory arguments nor their predictive arguments.

The scientist is, perhaps, more similar to the layman than to the chess expert, in that he does not know completely all the laws and "rules" of phenomena. Thus, after investigating some regularities, the scientist begins to formulate a theory which would explain these regularities and enable him to predict new ones. If the new regularities agree with the predictions, the theory is kept. But if they do not agree, then the theory has to be modified in the light of the data and retested again.
Section V

A General Critique of
the Distinction: Theoreticism

There is a general critique of the traditionalist's distinction and also of the traditionalist's view of theory. The basic points of this critique can be found in the thoughts of Popper, and the critique itself has been presented most strongly by Feyerabend. In what follows, I shall first present Feyerabend's argument. But this requires us to reconsider the traditionalist's general view of a theory's structure.

A theory is an intricate linguistic network. In such a network, even the meanings of the terms are interconnected. Let us assume that we have a theory T'. Now what will happen to this theory if we add to it additional terms, statements, and $\xi$-rules such that we now have theory T? Will the meanings of T contain intact all the meanings of T' but, in addition, other meanings? That is, will T merely be an extension of T'? One way of deciding this question, perhaps the best way, is to determine whether all the inferences that one could make from T' can also be made from T. If we can infer from T everything we could infer from T', then, apparently, T is an extension of T'.

Feyerabend, however, does not believe that such a change in extension, and extension only, usually occurs when one replaces one theory with another. According to Feyerabend:

what happens is, rather, a complete replacement of the ontology (and perhaps even of the formalism) of T' by the ontology (and the formalism) of T and a corresponding change of the meanings of the descriptive elements of ... T' ... . This replacement affects not only
the theoretical terms of \( T' \) but also at least some of the observational terms which occurred in its test statements.\(^66\)

Feyerabend also holds (1) that scientific theories are our linguistic inventions for unifying, explaining, and predicting our experiences; and (2) that theories are testable and should be abandoned if their predictions do not turn out to be true. Feyerabend believes that this latter condition helps insure scientific progress.

Such views as (1) and (2) are not peculiar to Feyerabend; they are also found in Popper's works. Popper argues that the rational procedure is that of conjecture and refutation, i.e., inventing theories and then trying to refute them.\(^67\) The best theories are those which say that certain things do not occur; "the more a theory forbids, the better it is."\(^68\) As to the question which came first—a particular theory, conjecture, hypothesis or the observations it was established to explain—Popper says that, of course, it was the observations; however, the observations occurred in the framework of prior theories and were such that these theories could not explain them (without adding \textit{ad hoc} auxiliary assumptions). A new theory was invented in the light of the old, which attempts to explain these observations.\(^69\) Popper thus believes that all reasoning is theoretical, conjectural, or hypothetical in the sense that it is always done within the framework of


\(^{67}\text{Popper, "Science: Conjectures and Refutations," p. 51.}\)

\(^{68}\text{Ibid., p. 36.}\)

\(^{69}\text{Ibid., p. 47.}\)
testable theories or assumptions.

The traditionalist would definitely hold that scientific theories are testable. He could also hold with Feyerabend that a change in the theoretical part of a theory will usually result in meaning changes throughout the theory. Can, however, the traditionalist hold that changes in a theory also affect some of the theory's observational terms? It depends, of course, on what Feyerabend means by 'affects.' Feyerabend claims that the observational statements of T mean something different from the comparable ones of T'. The traditionalist could say that the observational statements have had their meaning extended due to additional correspondence rules or have had their meaning limited due to removing correspondence rules. Furthermore, if T replaces a C-rule of T' with a different C-rule, then (in this sense) the observational statements affected by the rule of T will differ from their counterparts in T'.

If the preceding is all that Feyerabend means, then he offers nothing disturbing to the traditionalist. Yet the traditionalist holds that the meanings of the observational terms of T are decided by experimental procedures that do not presuppose T'. Now, as with Popper, Feyerabend holds that experimental evidence (and, therefore, any statement of experimental procedures) "does not consist of facts pure and simple, but of facts analyzed, modeled, and manufactured according to some theory."70 The traditionalist can agree with Feyerabend's remark provided that the theory which the evidence and testing procedures

70Feyerabend, "Explanation, Reduction, and Empiricism," p. 50-51.
presuppose is other than T. Feyerabend is claiming that there is no such thing as an isolated statement describing experimental evidence or experimental procedures. If he is only claiming that in searching for evidence and following procedures we also hold certain assumptions which guide us, then he is undoubtedly correct, but his point seems to be trivial. Furthermore, even in the testing of a theory it seems that we have to assume it as a hypothetical guide, i.e., the theory tells us what sorts of things are to be observed and what sort of things are not to be observed. Thus, in testing the theory, we use the theory. But the theory as a theory does not tell us completely how to test for it. The rules for testing procedures may be a part of another theory, viz., the meta-theory of all scientific theories of a certain domain or subject matter, e.g., mechanics. The meta-theory would apply to all of the scientific theories of that domain so that objectivity is insured.

Yet Feyerabend seems to hold that the above appeal to a theory-neutral meta-theory governing the experimental procedures for all the theories of a certain domain is not sound. Feyerabend argues that the observational techniques used and how they are corrected also depend on the specific theories T and T'. Feyerabend concludes that the facts counting as evidence for T will differ from those counting as evidence for T'. The immediate question which now arises is this: In what sense can the theories be said to have the same domain if they have different experimental procedures and evidence? Scheffler continues this line of argumentation by noting that if Feyerabend is correct, then there cannot be any opposing theories, since there cannot be any

71 Ibid.
evidence common to them for explanatory disagreement. This argument can be easily extended to produce the paradox of common language:

The isolation of each scientist within the world of meanings created by his own beliefs, so that every attempt to raise his voice to be heard beyond that world must fail, and every attempt to justify these very beliefs must itself presuppose them. Justification of theory in fact disappears, giving way to proclamation of theory.72

Scheffler admires the traditionalist's view because it offers a theory-neutral observational language. However, one could modify Feyerabend's view such that we have a neutral meta-theory which determines the experimental procedures and evidence for a particular domain and by means of which we evaluate competing theories. This neutral meta-theory would be the foundation for objectivity and agreement between scientists and their theories.

I do not believe that Scheffler is correct when he says that, according to Feyerabend's view, theory-justification disappears. Even if experimental evidence is not given independently of theory, it must be given independently of the theory being tested. But this is not to say that the meanings of the evidence is independent of the theory tested, only that the truth of the evidence is determined independently of the tested theory. If its truth could not be so determined, then every theory would guarantee its own truth and testings would be meaningless or, at most, superfluous.

I do not believe that there is much difference between the formulation of the experimental procedures for observational terms and the

function of the meta-theory mentioned above. If I am correct, then the traditionalist's view and Feyerabend's view differ little on this point. In justification of that preceding claim, Feyerabend holds that crucial experiments are possible between two or more theories provided there are background theories which provide a common interpretation for their observational statements. The traditionalist can here agree completely with Feyerabend. The possibility of such agreement tends to indicate that Feyerabend is not guilty of the paradox of common language. I shall thus call Feyerabend's view **theoreticism**.

Feyerabend next considers the testing of these background theories (being a realist he assumes that these theories have factual import and are not merely meta-theories whose content is purely linguistic as an instrumentalist would hold). Here different procedures are possible. One could, if ingenious enough, construct an even more general background theory for testing conflicting though less general background theories (remember, for Feyerabend, a good theory must be testable). And secondly, one could examine the internal structure of the theories to decide which is more closely connected to observation. (This seems to be an appeal to simplicity).

Each of these solutions deserves some comment. The first one seems to be in the spirit of Popper's thought. The second one is

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rather weak because of problems in explicating 'simplicity.' Whichever explication is accepted, such simplicity—or whatever it presupposes—can be incorporated into the more general background theory. It would seem, then, that whatever other alternative procedure is given, it can be interpreted as a special case of the first alternative. It should be noted that if the background theory is taken as asserting factual statements, then as our testing proceeds from a less abstract background theory to a more abstract theory, the more difficult it becomes to test the theory. Thus it seems that perhaps the difference between the scientific and the metaphysical is also one of degree (the more metaphysical being the more abstractive).

The other critique I wish to consider should not actually be considered as a critique of the traditional view. This is the critique of inductive reasoning in science, especially as a source for theory construction and theory testing. Of the traditionalists only Hempel and Carnap fully emphasize the need for inductive reasoning in science. We have discussed Hempel's use of the I-S model in explanation and prediction, but if our analysis is correct, then the I-S model can easily be replaced by the D-S model. Thus there would be no need for induction in either explanatory or predictive arguments. Yet along the way, another model presented itself as seemingly adequate in these two major scientific functions, the I-A model. Now the above argument of Popper and Feyerabend, i.e., theoreticism, prima facie can be used against the I-A model (and possibly instrumentalism), since, according to theoreticism, all prediction and testing assume some general statements of law. The I-A model was established as an attempt to have
adequate explanations and predictions of individual events without assuming laws. If Popper and Feyerabend are correct, then the I-A program is impossible. It should be remembered that the I-A advocate could hold laws as summary statements of observed data. But laws proper are not so temporally restricted.

In evaluating this criticism against the I-A model, we shall consider a simple example taken from Pap. Suppose we want to test the law of the lever of the form:

'If bodies of equal weights are suspended at equal distances from the fulcrum of a perfect lever, then the lever is in equilibrium.'

We have two bodies A and B which we are going to use as the test bodies of equal weight, but we have to know whether they have equal weights. Pap believes that 'A and B have the same weight' is not an observational statement (which is in agreement with our previous analysis), because we can only establish it by presupposing some law or universal hypothesis. On the assumption of Hooke's law ("the amount of distortion produced in a body when a force acts upon it is directly proportional to the magnitude of the force"), by means of a spring scales we measure their weights (the acting forces on the spring) to be the same. Then we place each body on one of the ends of the lever and observe if the lever achieves equilibrium. If it does achieve equilibrium, then the law of the lever has been confirmed—but only on the assumption of Hooke's law. Is there any way in which the I-A advocate

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74Pap, op. cit., p. 215.
could test the lever law without assuming Hooke's law?

First of all, instead of testing the lever law the I-A advocate would be testing the following: 'If \( A \) and \( B \) have the same weight then, if each one is placed on one of the opposite ends of the lever, the lever will achieve equilibrium.' The latter is an instance of the lever law and the advocate holds that at most we can test such statements as these. Now, then, how can the I-A advocate determine the weights of \( A \) and \( B \) without assuming a universal hypothesis? Instead of taking Hooke's law as a universal statement of fact, could it not be considered as a contextual definition of 'having the same weight'? Thus:

For any two bodies \( x \) and \( y \), '\( x \) and \( y \) have the same weight' means '\( x \) and \( y \) stretch the spring scale the same amount.'

By using the above definition instead of Hooke's law, the I-A advocate could test his statement. The problem that now arises is why accept the above definition? Definitions, especially technical ones, are mere stipulations. Why not simply proclaim that \( A \) and \( B \) have the same weight and then proceed to test the statement? At this point the I-A advocate could ask the theoreticist why he assumes Hooke's law and not some other law. The theoreticist may reply that he can justify Hooke's law by appealing to some other law just as he justified the lever law by appealing to Hooke's law. But how does one justify definitions?

It appears that the I-A advocate will have to take another line of defense. He notes that in the past it has been observed that, within certain limits, whenever two weights stretched the spring scales equally, they both balanced each other on the lever. Thus, from such past data and the present data that \( A \) and \( B \) equally stretched the spring
scales, he infers by analogy that they will balance each other on the lever. Thus the inference was made neither by assuming Hooke's law nor by stipulating arbitrarily a definition.

Let us now generalize the I-A advocate's methodology. In any situation in which one assumes a law to determine a property of an object (e.g., weight) under certain observed situations (e.g., stretching of the spring scales), and in which one used the determined property, or a statement of it, to make a prediction (e.g., the lever will balance), one can make that same prediction by means of analogy on the basis of statements of certain observed situations (e.g., stretching of the spring scales) and their being correlated with statements similar to the predictions (e.g., the lever has balanced), provided that one has just observed one of those situations. It should be noted that the I-A advocate can make his prediction without using 'weight.' Thus it is eliminable from his scientific vocabulary.

If it is replied that the I-A advocate must assume some theory which enables him to determine the stretching of a spring scale, he can make the same reply *mutatis mutandis* as the one he made above. What permits such a reply is that one does not rely merely on a law or a theory in determining a property of an observable: one also relies on another statement of initial conditions. Several statements of such initial conditions will enable the I-A advocate to draw the same inference as the law holder with this exception: the I-A advocate will be inferring by analogy, not by deducing from an assumed law and statements of initial conditions. Of course the criticism that can always arise is that these statements of initial conditions presuppose some
theory, but the advocate need only point out that the presupposed theory is not enough; other statements of prior initial conditions are needed to explain statements of posterior initial conditions. Those statements of prior initial conditions explain by analogy the statement of posterior initial conditions.

If, nevertheless, the theoreticist denies that every explanation of a statement of initial condition requires another statement of different initial condition, i.e., that only a universal statement is required, then the I-A advocate can reply that under those conditions the universal law functions as a definition. If a universal statement does not link two logically distinct statements (i.e., distinct in the sense that an analysis of one does not yield the other), then it is a definition. Thus if Hooke's law were the only statement telling us how to determine the weight of an object, it would be merely a contextual definition. But it does link two logically distinct statements, viz., 'x and y have the same weight' and 'x and y stretch the spring scale the same amount,' since each can be determined independently of the other.

In conclusion, we observe that the I-A advocate has defended his model even against what appeared to be a severe criticism. In defending the I-A model, one interesting logical move of the advocate enabled him to eliminate 'weight.' Such a move tends to indicate that the word merely signifies the correlation of 'stretching a spring scale equally' with 'balancing a lever'; it does not refer to anything else. Since this view is indicative of descriptivism. I shall now evaluate the thesis of descriptivism.
CHAPTER V
DESCRIPTIVISM

Section I
An Initial Criticism
from Hempel and Carnap

When presenting the introductory discussion of the three main views, I mentioned that Nagel holds that there are two kinds of descriptivism: (1) phenomenalism, and (2) physicalism. According to phenomenalism, theories are to be translated into statements about sense data—the immediate data of sensation whose descriptive statements are either undubitable or incorrigible. Such statements, however, are only known to be privately true. If the language of science is public and the language of phenomenalism is private, then it seems that phenomenalism is not an adequate view of scientific theories. But perhaps the public language can be constructed from the private language. What would 'being public' mean from the standpoint of a private language? Since other people would be constructions of private sensory data, the public would be that upon which most of the other people and the phenomenalist agree, and that which confirms or verifies his expectations and the verbal and bodily expectations of the other people. One could also identify 'the public' with 'the real.' Thus that which was not agreed upon or failed to verify an expectation (a false prediction) would be unreal.
At this point it might be asked, How does phenomenalism differ from physicalism? It would seem that phenomenalism has, in its linguistic construction of 'public,' also constructed physicalism, since physicalism holds that scientific knowledge is publicly observable. However, these two views can differ. Physicalism can hold that we only have knowledge with respect to what is publicly observable; there is no such thing as private knowledge. In any case, it does not seem to be too important, for the issue in question, which view is accepted, since the language and knowledge of science is public. Descriptivism proper, then, is the view that the language of science must be defined, reduced or translated into the language of public observation. Phenomenalism will thus carry the defining, reducing, or translating further to the private level. Thus in order for both phenomenalism and physicalism to be true, descriptivism proper must be true on the assumption that there is scientific knowledge. If descriptivism is true, then it would seem that the theoretical-observational distinction is ultimately unsound. But whether the distinction is sound or not depends on how one explicates 'defines,' 'reduces,' and 'translates.'

Let us consider the first procedure, i.e., definition. Is it possible, first of all, to explicitly define a theoretical term by means of observational terms? If by a theoretical term one merely means a technical scientific term, then such a definition seems to be quite possible. Yet Hempel attempts to show that not every term in science can be explicitly defined. As an example, let us say that we want to define \( x \) is magnetic at \( t \) (where \( x \) is an object and \( t \) is a particular time). A simplified definition is
(1) 'x is magnetic at t' means 'if, at t, a small iron object is close to x, then it moves toward x.'

The word 'magnetic' is considered by Hempel to be a dispositional predicate; i.e., a predicate which refers to the manner in which a thing reacts under certain conditions. The general form of presenting linguistically a dispositional property is conditional; thus, 'x has D' means (where 'D' is dispositional) 'if x occurs under conditions C, then x has property E.' Hempel believes that dispositional properties are not directly observable characteristics of things; they are dispositions of things. Since 'small object,' 'close to,' and 'moves toward' are apparently all observable properties, then it would appear that we have defined the dispositional 'x is magnetic at t' in terms of observables.

There are two questions which arise at this point: (1) Are dispositional properties unobservable properties? and (2) Is the above definition adequate? First, if such properties were unobservable, then to define them in terms of observables would be self-contradictory. Consequently, any descriptivist who holds the thesis of explicit definability of all scientific concepts cannot hold the theoretical-observational distinction. Thus, if the thesis cannot be maintained, we have some evidence in favor of the traditional view. Hempel argues that the above definition is inadequate. If we assume that the logical connectives of terms and statements be the truth-functional connectives


2Ibid., p. 24.
of the propositional calculus, then the definiens of the above defini- 
tions will have to be construed as a material implication. Hence the 
only time an object will not be magnetic is when a small iron object 
is close to it, but does not move toward it. On the other hand an 
object will be magnetic (such as a rubber band) when it is not close to 
a small iron object, i.e., whenever the antecedent of the definiens is 
false. For any object failing to satisfy the antecedent, that object 
is magnetic. Since being close to a small iron object is the test con-
dition for \( x \) being magnetic at \( t \), then we will have to say that any-
thing failing the test condition is definitely magnetic. Hempel 
believes that this situation is objectionable. Apparently it is objec-
tionable because he believes that most people would not want to say 
that something is magnetic if it failed the test condition. But what 
if a descriptivist did not find (1), under that situation, objection-
able? After all, is not the issue here merely one of preference?

To show that Hempel's objection can be defended, let us consider 
another definition, viz.: 

(2) \('x \text{ is magnetic at } t' \text{ means } '\text{if, at } t, \text{ a small } 
\text{iron object is close to } x, \text{ then it does not move toward } x \text{ at } t.'\) 

Now everything which fails to meet the test condition for (1) also fails 
to meet the test condition for (2). These entities would both be mag-
netic\(_1\) (being true of (1)) and magnetic\(_2\) (being true of (2)). On the 
other hand, everything else which meets the test condition will either 
be magnetic\(_1\) or magnetic\(_2\), but not both. Since meeting the test con-
dition is more selective than meeting either definition, the definitions 
are not adequate with regard to selectivity. But what if the
descriptivist is not concerned with selectivity of definitions? After all, to use Smart's phrase mentioned earlier, even if we accept these definitions we cannot infer anything false from them.

Hempel might reply by saying that it is false that a rubber band which did not meet the test condition is magnetic. What would be Hempel's evidence for such a claim? It can only be that he has observed other rubber bands meeting the test condition and failing the response, viz., the iron did not move toward the rubber band. And, by analogy, he infers that \( x \), a rubber band, would have reacted similarly. Since the preceding inference involves the subjunctive conditional, Hempel believes that some other method of analysis besides (1) is needed. Because of problems with the subjunctive conditional, he rejects using it as an explication of dispositional properties. He believes that (1) does not explicate the pre-analytic meaning of being magnetic and that the subjunctive conditional analysis is still inadequate. Thus we are to remain within the truth-functional logic yet (1) will have to be replaced by another formulation.

Hempel, following Carnap, considers the use of reduction sentences for introducing disposition predicates into languages of science. It appears to be a method of reductionism because of the name of the sentences used. Statements of the form \( 0_1 \supset (0_2 \supset 0_3) \) are called 'reduction sentences'; and, statements of the form \( 0_1 \supset (0_2 \neq 0_3) \)

\[ \text{Ibid., p. 25.} \]

\[ \text{Carnap, "Testibility and Meaning," p. 53-62.} \]

\[ \text{Hempel, Fundamentals of Concept Formation in Empirical Science, p. 25-29.} \]
are called 'bilateral reduction sentences.' Thus we can convert (1) into the bilateral reduction sentence:

\[(3) \text{"If a small iron object is close to } x \text{ at } t, \]
\[\text{then } x \text{ is magnetic at } t \text{ if and only if that object moves toward } x \text{ at } t."\]

Although in (3) the material implication is still present, it does not follow that \(x\) is magnetic when it is not close to a small iron object.

However, (3) is not a definition of 'magnetic'; it only enables us to apply 'magnetic' to those objects which are close to small iron objects that move toward them. A definition presents a necessary and sufficient condition for the application of a term, but a reduction sentence provides only a sufficient condition for the application of a term to those objects that have been tested in accordance with it. Thus the term is not applicable to untested objects. Since the reduction sentences do not tell us whether the term in question is applicable to untested objects, Hempel says that such sentences only partially define the term. Hence for objects which are not close to small iron objects, we do not know if they are magnetic.

Although this analysis may seem to be a restriction, it actually is not. We do not know empirically whether an object has a particular property unless we test for it. If it meets the test at time \(t\), then we can say that at \(t\) it had that property. But for that object at any other time and for any untested object, we have to infer beyond the empirical in determining whether or not the term applies to them. Such

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6 Carnap, "Testibility and Meaning," p. 54.

an inference, of course, is required for predictions. Thus reduction sentences are not by themselves sufficient for making predictions nor adequate for explanations (assuming the identity thesis).

Yet if (3) is the only statement in which 'magnetic' appears, one could say that (3) defines the operational procedure that is necessary and sufficient for determining whether something is magnetic; in other words (3) would be an operational definition. It would be a necessary condition for applying 'magnetic' if (3) is the only statement telling us how to empirically apply 'magnetic.' Nevertheless, most significant terms occur in more than one reduction sentence. Thus, 'magnetic' also occurs in:

(4) "If $x$ moves through a closed wire loop at $t$, then $x$ is magnetic at $t$ if and only if an electric current flows in the loop at $t."^8\)

From (3) and (4) we can infer that:

(5) if a small iron object is close to $x$ at $t$ and that object moves toward $x$ at $t$, then if $x$ moves through a closed wire loop at $t$ an electric current flows in the loop at $t$.

Statement (5) apparently is empirical since 'magnetic' was supposed to be the only theoretical term in (3) and (4). But it is eliminated through deductive inference from (3) and (4) to (5). (5) will be an experimental law. If 'electric current' is considered to be a theoretical term also, then we could set up another reduction sentence for determining whether one has an electric current flowing through a wire, e.g.:

(6) If the closed wire loop is connected to the terminal of a galvanometer at $t$, then an electric current

\(^8\)Ibid., p. 27.
flows in the loop at \( t \) if and only if the needle of
the galvanometer fluctuates at \( t \).

From (5) and (6) we may infer deductively:

(7) if a small iron object is close to \( x \) at \( t \) and
that object moves toward \( x \) at \( t \), and if \( x \) moves
through a closed wire loop at \( t \), then if the
closed wire loop is connected to the terminals
of a galvanometer at \( t \) the needle of the galva-
nometer fluctuates at \( t \).

Thus 'electric current' is eliminated. However, we now have 'galva-
nometer.' Is 'galvanometer' an observational term? It seems to be
meaningful only in accordance with electric and magnetic theory. Con-
versely, 'being magnetic' seems to be not so theory-dependent (e.g.,
the lodestone was known by the ancients as attracting iron even though
electric and magnetic theories had not yet been formulated). In
defense of the theory-dependency of 'galvanometer' consider its defi-
nition: 'an instrument which detects electric current by its needle's
fluctuations.' The descriptivist cannot use this definition for remov-
ing 'galvanometer,' since he would end up with (6) again.

It is, of course, possible to give reduction sentences for 'galva-
nometer,' but there seems to be no end in sight where we have only
observational terms in our sentences. The decision procedure for deter-
mining the application of 'galvanometer' will involve other instruments
which, in turn, will need reduction sentences. Since this procedure
cannot go on indefinitely, it can be stopped by positing an assumption,
e.g., 'this is a properly working galvanometer.' (Another problem,
perhaps even more crucial, is that a galvanometer works only because
it is magnetic. Thus, it would seem that in presenting reduction sen-
tences for 'galvanometer,' 'magnetic' will reappear. If this situation
happens, the entire procedure may become circular. To avoid at least
the threat of circularity, one will have to detect an electric current
by some other means, e.g., an electric light bulb or silver deposition
on electrodes. But these latter instruments would require reduction
sentences in their turn and the circularity problem might arise again.)
However, positing such an assumption, if it is considered to be factual,
cannot be accepted by the descriptivist since it would be at least
partially a theoretical assertion. The descriptivist might have to hold
that it is a definition. But if it is held to be a definition, then
are we not back at (1)?

The descriptivist could hold that reduction sentences describe
Nagel's experimental procedures used in applying predicates to what we
observe. As we noted earlier, Nagel claimed that such procedures only
partially determine the meaning of the predicates, since other distinct
procedures are also used in applying them. Both Nagel and Hempel agree
on this point. The descriptivist could then define 'galvanometer' as
'an instrument that detects electric current by the fluctuating of its
needle.' Hence we are not back at (1) because the preceding definition
is not conditional.

The next question, nevertheless, is how to determine whether an
instrument is a galvanometer. One must have an independent procedure
for detecting electric currents, and then apply the supposed galva-
nometer to the situation. But is an independent procedure possible
without the initial use of some other instrument? It does not seem to
be the case. Thus it appears (so the theoreticist would argue) that,
since we must start some place, we have to assume not merely a
definition but a factual claim, e.g., 'This instrument is a properly functioning light bulb.' By means of this factual assumption and the definition of both 'light bulb' and 'galvanometer,' we can determine whether the instrument before us is a properly functioning galvanometer; and thus we can proceed with the test described by (7).

But there is a problem with this view of the theoreticist. For if somewhere in our procedure we have to assume a factual statement, then why not assume that \( x \) is magnetic, and be done with these experimental and logical exercises? In fact such a procedure probably agrees with actual scientific testings. The scientist does not determine whether he has a magnetic object before him except by means of (3). But how does the scientist know that the small object before him is iron? He either assumes that it is iron or tests it by means of a reduction sentence. Yet that sentence will also have other technical terms which likewise need reduction sentences. Thus he will have to assume some factual statements somewhere in the procedure.

At this point someone might be tempted to say that the scientist uses iron to determine whether he has a magnet and the magnet to determine whether he has iron. Thus along with (3) he uses reduction sentence

\[
(\text{8}) \text{ if } x \text{ is magnetic at } t, \text{ then a small iron object is close to } x \text{ at } t \text{ if and only if that object moves towards } x \text{ at } t. 
\]

However, (3) and (8) entail:

\[
(\text{9}) \text{ if a small object moves toward } x \text{ at } t, \text{ then } x \text{ is magnetic if and only if that object is iron.}
\]

If anything is an observational predicate 'a small object moves towards \( x \) at \( t \)' is, since it seems to be easily ostensively defined. Thus we
would have an observational predicate being a sufficient condition for the biconditional of the theoreticals 'being magnetic' and 'being iron.' But has this move helped us? We still do not know whether each object is magnetic or iron. All we know is that if one is magnetic the other is iron, and conversely.

Section II

The Theoreticist's Critique

Theoreticism is the view that there are no observational statements isolated from theory or assumptions. It seems to be a sound view. Its method is called 'the hypothetical-deductive method.' Since it claims that we always make a factual assumption in any testing procedure, it is a type of realism. Thus we have Feigl's hypothetico-deductive realism. According to this view, we deductively explain and predict statements describing phenomena from a theory and statements of initial conditions. (This is in agreement with the traditional view). We do not start merely with observational statements and reduction sentences connecting them. A view opposing hypothetico-deductive realism is probabilistic realism. Apparently the probabilistic realist would agree so far with the I-A advocate. However, the I-A advocate might also be able to help the descriptivist. Let us now decide whose view is strongest.

Let us say that 'O' is a statement describing the event to be explained or predicted. 'T' is the explaining theory and is composed of 'A' the statement or conjunction of statements of initial conditions and 'L' the conjunction of its laws. In order for 'T' and 'L' to have
factual import, 'A' and 'O' must be tested independently of 'L.' But if the theoreticist is correct, 'A' must be known in accordance with another 'T', i.e., 'A' is deducible from 'L' and another statement of initial condition 'A'. But, as with reduction sentences, this procedure too cannot extend indefinitely. Thus we must stop at some theory and assume its laws and statements of initial condition. But the I-A advocate can take as his assumptions merely the statements of initial condition and infer, by analogy, 'from 'A' to 'O.' Thus both descriptivism and probabilistic realism have escaped the hypothetico-deductive realist's (theoreticist's) critique.

Which view is preferable? If they all adequately explain and predict the same phenomena, then the preferable view will be the one assuming the less. Now all the views must take into account statements of initial conditions, but only hypothetico-deductive realism has to assume laws. So prima facie it seems to turn out that hypothetico-deductive realism is least preferable. This issue shall be explored in the next chapter.

Section III
Translatibility and Reducibility

I mentioned earlier that reduction sentences constitute a type of reduction; and in Chapter I I mentioned that 'reduction' has been distinguished from 'translation.' Statement 'S' is said to be translatable into statement 'S' provided 'S' and 'S' are logically equivalent. 'Reduction,' on the other hand, has several different uses.

Let us look at four of them. First, (R), 'S' is said to be
reducible to statement 'S₂' if 'S₂' entails 'S₁'. Secondly, (R₂), one term 'T₁' is said to be reducible to another 'T₂' if they both have the same extension, i.e., if 'T₁' and 'T₂' are factually equivalent terms. An analogous reduction is possible also with statements, viz., one statement 'S₁' is said to be reducible to statement 'S₂' if they both are factually equivalent. Thirdly, (R₃), a term 'T₁' is said to be reducible to 'T₂' and 'T₃' provided they appear in a reduction sentence of the form 'T₂ ≡ (T₃ ≡ T₁)' or in the bilateral reduction sentence 'T₂ ≡ (T₁ ≡ T₃)'. And finally (R₄) one statement 'S₁' is said to be reducible to statement 'S₂' provided there is a true C-rule, 'C₁,' such that 'S₂ C₁' only together entail 'S₁.' One way of determining the truth of a C-rule could be by reduction since, whenever 'S₂' is true, 'S₁' must also be true. Reduction may also be applied to theories. For example, when we have two theories 'T₁' and 'T₂' such that whatever 'T₁' explains is also factually equivalent to some of the phenomena that 'T₂' explains, but 'T₂' explains much more phenomena than 'T₁,' 'T₁' may be incorporated into 'T₂' through the addition of the appropriate C-rules. Some of these C-rules will have to be tested indirectly via new predictions. These, then, constitute the important uses of reducibility which distinguish it from 'translatability.' Let us now determine the success of descriptivism using these techniques.

If a species of descriptivism requires the method of construction (like phenomenalism constructing a public language from a private one), then the constructed language is not logically equivalent to the source language. Hence phenomenalism is a type of reductionism (it could use
all three forms). Both physicalism and phenomenalism, as regards the language of science, held that scientific theories must be reducible to statements about what is or has been observed, or what is observable. Consider universal temporally unrestricted experimental laws. Such laws are not logically equivalent to any set of statements of observed phenomena. They are logically equivalent to at least a set of statements of observed and unobserved phenomena, i.e., observable phenomena. A descriptivist who holds that scientific theories are reducible to statements about what is or has been observed, will have to deny such laws as comprising theories. Such a descriptivist will probably adhere to the I-A model. On the other hand, a descriptivist who holds that scientific theories are reducible to statements about what is observable can admit experimental laws. However, he cannot admit theoretical laws.

In considering the distinction between theoretical statements and statements of observable phenomena, all descriptivists will have to hold that no term or statement is meaningful only in the context of a particular theory. This view is analogous to saying that 'straight flush' has meaning apart from poker, or 'castling,' apart from chess. To say that a term is meaningful only in the context of a theory or game does not entail supposing descriptive laws; at most, descriptions or rules concerning particulars or their characteristics. It does seem to be the case that 'electroscope' and 'galvanometer' are meaningful only in context of electromagnetic theory because of their explicit definitions. Thus in the terms of the theoretical-observational distinction defended earlier, descriptivism seems to be false. Some terms
and statements are meaningful only in the context of other terms and statements.

Perhaps descriptivism can be defended by the use of reduction sentences. Perhaps so-called theory-dependent terms can be introduced into the descriptivist's accepted language by means of reduction sentences in which the other terms of the sentence are observational. Such sentences would have the form '0₁(0₂>T₁)' or '0₁(Τ₁≡ο₂)' where '0' represents any observational term and 'Τ' any theoretical term. Putnam (as previously mentioned) criticizes the use of reduction sentences as the second notion of 'partial interpretation.' In particular he criticizes the indeterminacy part of the use of reduction sentences. If an object has not been tested with regard to having a certain property, e.g., being soluble, then the meaning of 'it is soluble' has been left undetermined or unspecified. For example, to say of an untested cube of sugar that it is soluble is to say something that is empirically undefined. Putnam finds this result objectionable; he believes that inductively the property can be applied to the sugar in the same sense that it was applied to tested sugar cubes. In defense of the reduction sentence technique, it should be noted that the reason Putnam applies 'soluble' to tested sugar cubes is not the same reason for his applying it to untested sugar cubes. It is by means of an operational procedure (which in fact is described by the reduction sentences) that he applies 'soluble' to tested sugar cubes, and it is by means of an inductive inference that he applies 'soluble'

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9 Hempel, "The Theoretician's Dilemma," p. 188.

to the untested cube. It seems, then, that Putnam has criticized the reduction sentence technique for not enabling him to make inductive inferences. Actually the purpose of such sentences is merely to describe experimental procedures for applying predicates to individuals meeting the test condition. If one wants to extend the application of the predicates to untested individuals, then one will have to use a different technique, perhaps induction.

A more rigorous criticism of the reduction sentence technique has been given by Scheffler. Scheffler criticizes the view that terms introduced by reduction sentences cannot be explicitly defined truth-functionally. Instead of introducing 'x is magnetic' by (3), Scheffler suggests introducing it explicitly;\(^{11}\) thus

\[ (10) \text{ 'x is magnetic at } t \text{'} \text{ means 'a small iron object is close to } x \text{ at } t \text{ and that object moves toward } x \text{ at } t. \]

If one replies that given (1), 'x is magnetic' as introduced in (4) means something differently than 'x is magnetic' in (10), Scheffler would agree. Thus we would have

\[ (11) \text{ 'x is magnetic at } t \text{'} \text{ means 'x moves through a closed wire loop at } t \text{ and an electric current flows in the loop at } t. \]

Since (10) and (11) are logically distinct, let us label the definendum of (10) \( M_1 \) and the definendum of (11) \( M_2 \). Since \( M_1 \) and \( M_2 \) are differently defined, they are different concepts and one cannot now infer (5) from (3) and (4). (See pp. 271-272 above.)

Perhaps the main reason why Hempel denies explicitly defining predicates by means of their operational procedures is that one would

\[^{11}\text{Scheffler, The Anatomy of Inquiry, pp. 174-176.}\]
thereby prohibit inferences such as (5) which enables us to extend the
interconnectness of our concepts so that we can predict and explain new
phenomena.\(^{12}\) But now that the linking predicate of (3) and (4) has
been given distinct definitions, (5) can no longer be inferred. It
should be noted that Percy Bridgman holds that if a term is intro-
duced by means of a specific operational procedure, then any term
introduced by means of some other operational procedure has a differ-
ent meaning from the former even if the terms, as linguistic entities,
are the same.\(^{13}\) Furthermore, Bridgman holds that until the concepts
of the operations have been shown to be equivalent by experiment, it is
best to consider them as distinct concepts.\(^{14}\) Carnap also agrees with
Bridgman on this point. Carnap holds that if we have three different
test procedures for 'electric current,' we have "given operational
definitions for three different concepts; they should be designated by
three different terms, which are not logically equivalent."\(^{15}\) Carnap,
consequently, prefers only one reduction sentence per introduced term.
But is it true that, if we were to adopt Scheffler's or Bridgman's or
Carnap's procedure, science would not be able to achieve one of its
principal goals which (according to Hempel) is "the attainment of a

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\(^{13}\) Percy Bridgman, *The Logic of Modern Physics* (New York: The

\(^{14}\) Bridgman, "Operational Analysis,"

\(^{15}\) Bridgman, "Operational Analysis," *Philosophy of Science*,

\(^{15}\) Carnap, "The Methodological Character of Theoretical Concepts," p. 64.
simple, systematic unified account of empirical phenomena"? Who is correct?

Scheffler, in his critique of Hempel's claim, formulates another definition which (in continuing our example) would be

\((12) \ 'x \text{ is magnetic}' \text{ means 'M}_1 \text{ or } M_2.\)"

From \((12)\) and the new reduction sentences containing \(M_1\) or \(M_2\) rather than 'x is magnetic at t' which are \((3a)\) and \((4a)\), respectively, \((5)\) is again deducible. Thus \((12)\) still permits the scientific unification of phenomena. If someone asks Scheffler why he accepts a definition such as \((12)\), he would reply that the same question could be asked for the reduction sentences of 'x is magnetic.' One can also extend Scheffler's criticism. Consider any two bilateral reduction sentences of the form 'A \(\supset\) (B \& C)' and 'D \(\supset\) (E \& F)' where 'B' and 'E' represent the introduced terms. We can posit 'G' and its definition 'B or E.' From the two reduction sentences and the definition we can infer the experimental law of the form '((A \& C) \(\supset\) (D \& F)).' Hence it appears that one can unify any two scientific procedures regardless of their subject matter. Of course, the experimental law should itself be tested. It should also be noted that such unification procedures, as the above, will occur if and only if reduction sentences are of the bilateral form.

In the preceding discussion we have been considering the introduction of dispositional terms by means of observational terms and reduction sentences. Carnap believes that theoretical terms cannot be introduced into a language by means of reduction sentences, because no observational predicates, in whatever relationship, are sufficient for

\[\text{\textsuperscript{17}}\text{Scheffler, The Anatomy of Inquiry, p. 176.}\]
the application of any theoretical term. Given the traditionalist structure of a theory and the partial interpretation thesis, Carnap's reasoning is sound. For example, placing a substance in ether and noting that it dissolves is not sufficient evidence for applying 'phosphorus' to the substance. A series of different procedures with the substance will have to take place and it seems that even the conjunction of these procedures, if in agreement with the definition of phosphorus, will still constitute only inductive evidence for the substance's being phosphorus. The consequence of this analysis is that theoretical terms cannot be reduced into observational terms.

Could the descriptivist hold that all C-rules are of the form 

\[(x)(Tx \supset Ox),\]

where 'T' is a theoretical term and 'O' an observational term (i.e., reduction)? Given such a C-rule, the theoretical terms become observational terms and the statement becomes an observational statement. (This point was discussed earlier when we took up Carnap's view on the theoretical-observational distinction.) Consider the statement 'Electrons are not things which rot.' One can give a reduction sentence for 'rot' in terms of observational predicates which are not defined by some principle of the atomic theory and need not be learned by learning that theory. But are all statements about electrons observational in that sense? It seems that 'Bohr's electron' cannot be understood apart from the Bohr atomic theory. Consider the statement 'Electrons have electric charges.' If by 'electric charge' is meant the non-magnetic attraction physical objects at times have for each

other, then the statement too seems to be observational. It can be understood apart from the atomic theory (even the ancients knew of this property), and one can present a reduction sentence for 'non-magnetic attraction.' It seems, then, that the descriptivist can accept quite a few statements about so-called theoretical entities (so-called in Carnap's list) on the basis of C-rules of the specified form. Thus far the descriptivist knows observationally that electrons belong to the classes of non-rotters and carriers of non-magnetic charges, i.e., to classes whose members have at least some observable characteristics.

Generalizing from the above examples, it seems that any physical property ascribed to an electron can be understood apart from the atomic theory. Now if electrons are identical with all their properties and their properties are observable, then are not electrons observable? It may be true that 'electric charge' refers to something that is observable at times, but it does not follow that an electron's electric charge is ever observable. For example, the electric charge of a hard rubber rod can be observed, but is the electric charge of an electron observed in the same way? We can formulate a reduction sentence for \( x \) has an electric charge if \( x \) is a hard rubber rod. But the same reduction sentence is useless if \( x \) is an electron. Has the descriptivist been misled by an equivocation into believing that 'electric charge' as applied to electrons is logically equivalent to 'electric charge' as applied to hard rubber rods? (A similar question would apply to 'non-rotter. ')

If the application of the two observational terms is made on the basis of reduction sentences about macroscopic entities, then they are
not applicable, by that fact alone, to electrons. In order to estab-
lish some sort of similarity in meaning between 'electric charge of an
electron' and 'electric charge of a hard rubber rod,' either they must
be derivable by means of a common principle which would be a law of the
atomic theory, or the former is derived from the latter by analogy in
accordance with a theoretical model of the electron. The first pro-
cedure of establishing similarity of meaning could be that of
hypothetico-deductive realism; the second, that of the I-A model. The
similarity between the rod and the electron is their non-magnetic
attraction. The attraction of an electron is similar to that of a
hard rubber rod that has been rubbed with cloth. Thus analogical
inference and models seem to be essential for the descriptivist in
maintaining that all non-analytic statements about electrons, for
example, are observational. If the descriptivist is correct, the only
theory-laden statements would seem to be technical definitions.

Is this type of analogical reasoning permitted for the descript-
ivist? Statements of the entities discussed in scientific theories
are derived by analogy from those entities and properties discussed in
the common language (or at least in the language of another theory).
At this point it seems that the distinction between the I-A model and
descriptivism is minute indeed. On the other hand, the descriptivist
who holds that the similarity of 'electric charge' is derived from a
common principle will become a traditionalist. Since we have con-
sidered both the traditionalist's model of explanation-prediction and
the I-A model of explanation-prediction, we now have the answer to
Brody, Capaldi, and Hesse's question: Can descriptivism give an ade-
quate account of explaining and predicting phenomena? So far both
Section IV

Descriptivism and the I-A Model

At this point I shall summarize my findings concerning descriptivism. If one wishes to hold Feigl's fourth view, naive conventionalistic positivism (NCP) or phenomenalism, then one will have to hold that all scientific statements except predictions are statements of observed phenomena. Such a proponent will have to accept the I-A model for explanation and prediction. On the other hand, critical phenomenalism, operationalism, or positivism (Feigl's fifth view) hold that all scientific statements are statements of observable phenomena. (Perhaps it should be added that we are not considering definitions as statements. By a statement in this context is meant a contingent assertion.) The critical phenomenalist can accept the traditionalist's view of theory except at the point of theoretical laws. The critical phenomenalist will argue that in so far as such statements enable us to predict and explain phenomena, they are either logically equivalent to or reducible to experimental laws. Or to put this point another way, theoretical laws have no surplus meaning. Their factual import is entirely contained in the experimental laws derived from them.

Feigl's sixth view, formalistic phenomenalism or syntactical positivism, holds that theories have the same structure as that advocated by the traditional view—with the terms in postulates or theoretical statements being connected to (the terms in) observational statements by correspondence rules. The syntactical positivist holds that the
theory's postulates have no surplus factual meaning. Their factual import is equivalent to that of the observational statements. But if this view is correct, then all the postulates and C-rules do is connect the different observational statements together into a linguistic network. Such connections can be expressed as formal and material rules of inference.

Feigl's last view is descriptivistic or contextualistic phenomenalism. This view merely stresses the claim that a theory is such an interconnection of observational statements that it is impossible to test one in isolation from the others. What one does is to test one observational statement by assuming the others as the criteria or standard. These assumed statements can be reinterpreted as definitions or material rules of inference, thus leaving only the observational statement by itself to be tested in accordance with the formal and material rules of inference.

The above four types of descriptivism are reducible to the first if it is construed in accordance with the I-A model. I believe that it is easily seen how critical phenomenalism reduces to NCP. Take all evidential statements for the laws (statements of observed phenomena), add the statement of observed initial conditions, and explain or predict by analogy. The same process may be used in translating syntactical and contextualistic positivism to NCP. As for the so-called postulates, they are to be construed as definitions or material rules of inference. (Compare Scheffler's way of connecting different experimental laws or reduction sentences by means of a definition mentioned above, page 28ff.)

Two major criticisms have been presented against this view: (1)
one cannot predict new phenomena by means of such a theory, and (2) these views do assume factual statements which have surplus factual meaning not reducible to that of the evidential basis, i.e., the observational statements. I shall now evaluate these criticisms.

Braithwaite presents the first in a formal, yet simple manner.¹⁹

Let us say that our theory contains five observational predicates: A, B, C, D, and E. From experimentation we have directly confirmed the following experimental laws:

- (E1) For any x, if x is an A and a B, then x is a C.
- (E2) For any x, if x is a B and a C, then x is an A.
- (E3) For any x, if x is an A and a D, then x is an E.
- (E4) For any x, if x is an E and an A, then x is a D.

The theory also contains four theoretical predicates: L, M, N, and R. The theory's postulates and C-rules are summarized together as:

- (T1) For any x, x is an A if and only if x is an L and an M.
- (T2) For any x, x is a B if and only if x is an M and an N.
- (T3) For any x, x is a C if and only if x is an N and an L.
- (T4) For any x, x is a D if and only if x is an L and an R.
- (T5) For any x, x is an E if and only if x is an M and an R.

From (T1), (T2), and (T3), (E1) and (E2) deductively follow. The theory also predicts the new experimental laws:

- (E5) For any x, if x is a C and an A, then x is a B.
- (E6) For any x, if x is a D and an E, then x is an A.
- (E7) For any x, x is a B and a D if and only if x is a C and an E.

Yet the question is whether the same laws or their observational instances can be inferred by the I-A model using only as premises observational statements of evidence and (possibly) definitions. Let us also assume that the observational properties are observed independently of each other. Let 'a,' 'b,' 'c,' etc., be the names of

¹⁹Braithwaite, Scientific Explanation, pp. 50-76.
individuals tested. Thus 'Fa' is to be read as 'a has F.' Since observational statements of evidence are of the form 'Fa,' a statement of evidence for (E1) would be 'Aa • Ba • Ca.' But note that that statement is also evidence for (E2) and the predicted law (E5). Further, a statement of initial conditions 'Ab • Db • Eb' not only confirms (E3) and (E4), but it also confirms the predicted law (E6). Thus the I-A advocate can use these statements of evidence together with a statement of initial condition 'Ac • Cc' or 'Dd • Ed' to explain or predict by analogy any phenomena which (E5) and (E6) explains with those same statements of evidence. However, neither of the above statements of evidence not even conjointly confirm (E7). Thus (E7) appears to be a genuine new law.

Is there any way that the I-A advocate could have anticipated (E7) or its evidential statements? In considering the above statements of evidence from 'Aa • Ba • Ca' and 'Ab • Db • Eb' one can predict by analogy 'Ab • Bb • Cb' and 'Aa • Da • Ea' because 'A' appears in both premises. ('Aa • Ba • Ca' and 'Ab' analogically imply 'Ab • Bb • Cb'; 'Ab • Db • Eb' and 'Aa' analogically imply 'Aa • Da • Ea.' Together, the statements of evidence and predictions, 'Aa • Ba • Ca,' 'Ab • Db • Eb,' 'Aa • Da • Ea,' and 'Ab • Bb • Cb' constitute evidence for (E7). Due to the persistence of 'A' in our evidence for (E7) it may be the case that whatever satisfies (E7) is also an A. (This method is predicting by analogy another law that Braithwaite's theory predicts. As it turns out (E8), i.e.,

\[ (x) \left\{ (Bx \land Dx) \Rightarrow (Cx \land Ex) \Rightarrow Ax \right\} \]

It would seem, then, that descriptivism can be adequately defended by the I-A model including NCP without even appealing to any definitions. What, then, is the role of the theoretical terms except to provide
analogical connections between statements of evidence and predictions?

Braithwaite further notes that in examining the theoretical statements he finds the possibility of another such statement's being added to the theory, namely

(T6) For any x, x is an F if and only if x is an N and an R.

Braithwaite claims that the theory can "suggest the possibility of there being a new observable property related to the known ones."20 But that new property cannot be predicted by the theory as it stands. The question, however, is how new can that property be? Perhaps the best answer is that it is new to the theory. In other words, an inspection of the theory indicates that there is or may be an observable property that we did not incorporate into the theory. Noting that 'N' and 'R' appear in (T6), we then examine the other statements that contain either 'N' or 'R.' These statements are (T2), (T3), (T4), and (T5). By examining how we determine 'B,' 'C,' 'D,' and 'E,' we may discover 'F.'

Let us say that we indeed discover 'F' by this method. But what kind of a method did we use in discovering 'F'? The method seems to be basically analogical. Our initial examination of the theory's postulates can be formalized as the following:

(i) there is an observable property related to L and M; and, L and M is a possible combination.
(ii) there is a second observable property related to M and N; and, M and N is a possible combination.
(iii) there is a third observable property related to N and L; and, N and L is a possible combination.
(iv) there is a fourth observable property related to L and R; and, L and R is a possible combination.
(v) there is a fifth observable property related to M and R; and, M and R is a possible combination.

20 Ibid., p. 69.
(vi) N and R is a possible combination.
(vii) there is an observable property related to N and R.

We then reason by analogy to those statements in which either N or R are related to an observable. This discussion in turn suggests that the theoretical predicates are merely linguistic connections between observational predicates that permit analogical explanations and prediction of phenomena.

Thus, in defending descriptivism, it appears that we eventually end up with instrumentalism. Therefore, there seems to be no difference between descriptivism, as we have been defending it by means of the I-A model, and instrumentalism. It is now time to consider the dispute between realism and instrumentalism.
CHAPTER VI

INSTRUMENTALISM AND REALISM

Section I

Surplus Meaning and the Kinetic-Molecular Theory of Gases

In his presentation of the different views on the cognitive status of theories, Feigl distinguishes (as noted in Chapter I, Section II) realism from phenomenalism—his term for both descriptivism and instrumentalism. (To distinguish it from phenomenalism proper, let us hereafter call it 'phenomenalism₂.') His distinction hinges on his claim that realism holds scientific theories to possess surplus meaning. But, from the previous discussion of Braithwaite's example, one may well wonder just what is this surplus meaning the realist is advocating.

In their criticisms of Feigl's paper, Philipp Frank, Hempel, and Nagel argue that there is no significant difference between semantic realism and syntactical positivism. If semantic realism is to avoid metaphysics, Frank argues, then all of its significant statements should also be formulatable in the language of syntactical positivism.¹ Hempel argues that the surplus meaning of a theory is the logical relationships of all the statements, postulates, and definitions of the

theory. On the other hand, he argues that the empirical meaning of a theory is the totality of its statements of evidence. Yet Hempel's notion of surplus meaning seems to be a logical kind of meaning rather than a factual one, and in the I-A model this surplus meaning is contained in the analogical rule of inference.

Nagel argues that it is one thing to hold that a theory is logically equivalent to its statements of evidence, and that it is another thing to hold that a theory's function "can be adequately understood and described in terms of the observable connections [its] use establishes between classes of observation statements." Nagel believes it is this instrumental role of theories which descriptivism emphasizes, not whether some of its non-observational terms have factual reference. A theory is not merely a summation of observational statements; it is a linguistic instrument enabling us to organize observational statements in an effective way for purposes of explanation and prediction. Thus Nagel seems to be agreeing with Hempel in holding that the surplus meaning of a theory comprises no factual reference beyond the observational statements. (This view of Nagel has already been given, in Chapter I, Section III, under his discussion of instrumentalism, especially with regard to his hammer analogy.)

In reply to these criticisms Feigl seems to be claiming that only semantic realism can justify why certain predictions from a theory are

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more justified than others.\textsuperscript{4} Feigl maintains that the phenomenalist in his reduction has to

locate and date the events described in the antecedents and the consequents of the factual or counter-factual conditionals which form the evidential basis.\textsuperscript{5}

Feigl believes that such locating and dating requires a realist background. Another important distinction Feigl constantly defends throughout his essays is the distinction between the evidence for a statement and the statement's reference. According to Feigl, the phenomenalist has failed to make this distinction: the phenomenalist translates or reduces the factual reference of a statement to its statements of evidence. In justifying this distinction Feigl appeals to historical assertions, i.e., to statements asserting that something occurred. (All statements of initial conditions are such historical assertions.) Feigl believes that such statements can only be indirectly tested or confirmed; and such testing presupposes realism.

The type of statements Feigl is considering he calls 'existential hypotheses.' Existential hypotheses are singular statements of now unobserved phenomena. It may be the case that the phenomenalist can reduce universal hypotheses to their evidential basis, but existential hypotheses cannot be so reduced. Is Feigl's argument sound? Evidence for singular statements can be of two kinds: (1) universal statements and other singular statements, and (2) other singular statements. If we accept those of kind (1), we may have deductive evidence for the singular statement in question. However, such evidence uses premises


\textsuperscript{5}Feigl, "Existential Hypotheses," p. 55.
which will also need to be justified, unless we merely posit them as conjectures. Yet one may posit anything which will ultimately entail the singular statement. Thus we must have independent evidence for the premises of a justification using statements of kind (1), and, since all evidence is expressed in singular statements, we are reduced to statements of kind (2) as our only justification of any kind of factual statement.

Our evidence for a past historic event is, moreover, based on the reliability of our source of evidence. If our source states something which we can test and the assertion is determined to be true, then by analogy we infer that the singular statement in question is true. If the tested assertion of our source turns out false, then we infer by analogy that the singular statement in question is false. If, however, half of the tested statements turn out false and half turn out true, then we will have to go to another source. In either case the statement of evidence has a factual reference distinct from the statement being justified. This type of justification appears to be ultimately the only type for singular statements. On the other hand, the justification of a universal statement does consist in using at least some of its factual reference. For example, 'Socrates is a man and is bald' is both evidence for 'All men are bald' and entails an instance of it (part of its factual reference): 'If Socrates is a man, then Socrates is bald; and, Socrates is a man.' Because phenomenalism has been defended by advocates of the I-A model, and the I-A model has been used to defend and explain the difference between evidence for a singular statement and the reference of a singular statement, it does not seem
to be the case that the phenomenalist has failed to make the distinc-
tion. The question now, however, is whether the making of such a dis-
tinction presupposes realism.

It is not clear why locating and dating an event presupposes real-
ism. In reasoning from analogy we must be able to distinguish the two
events and also compare them. Let us say that we are making our infer-
ence from a and b and some of their properties to b's having another
property we observe a to have. If they are co-temporal, the need for
dating does not seem to arise; yet, we perhaps need location to dis-
tinguish them. On the other hand, if they had all spatial properties
in common, the need for dating arises. In any case, it does not appear
that realism has been presupposed. If by realism Feigl means that we
must distinguish between the evidence for a statement and its reference
(and this seems to be just what he means by semantic realism), then
semantic realism has been justified. Anyone attempting to identify
them is in error. If the phenomenалиsts have held such an identifi-
cation, then their thesis is incorrect. But realism as understood
here seems to be a too trivially true doctrine.

There must be a more substantial way of distinguishing realism
from its competitors. Perhaps the following is the way. The realist
is an individual who not only has evidence for a certain singular
statement but he believes that the singular statement is true. Thus
the scientific realist would hold that we have good evidence for
'Electrons exist' and he believes that electrons exist. It seems pos-
sible to have evidence for a statement and yet not believe that it is
true; and, conversely, it is also possible to believe something true
and yet have evidence against it.

The scientific instrumentalist, on the other hand, can hold at least one of the following: (1) we do not have good evidence for 'Electrons exist'; (2) 'Electrons exist' is false; (3) 'Electrons exist' is meaningless; and (4) to say 'Electrons exist' is merely another way of saying that the atomic theory has enabled us to make successful predictions--i.e., 'Electrons exist' merely connects different singular statements about observables in such a way that one statement may be inferred from some of the others. The fourth claim implies that 'Electrons exist' is not a genuine singular statement; at least, it is not so genuine as 'This is a track in a cloud chamber.' The fourth claim could likewise be construed as saying: "The predicate 'electron' is analogous to a dispositional predicate which connects several distinct operational procedures, such that from a statement about one procedure one may infer a statement about another." In considering Nagel's discussion of instrumentalism, the fourth claim likewise seems to be what he understands by instrumentalism.

In evaluating instrumentalism and realism let us consider a scientific theory, viz., the kinetic-molecular theory of gases. I choose this theory, because it deals with molecules (submicroscopic entities), is fairly easy to understand, is very successful as a theory, and because it enabled scientists to develop the atomic theory. In this theory, as with most scientific theories in natural science, we have present quantitative laws. Such laws were lacking in Braithwaite's example: all of his laws were qualitative.

Quantitative laws are those laws which express qualities that are
mathematically related. Their values are derived from the operational procedures of measurement. Because these values are inferred from what is observed, I am hesitant to call them observables. But they may be merely a mathematical way of expressing the observable relationship between the measuring device and the measured entity. Thus in the special case in which the beam is a straight rod, the force lines are parallel, and the weights are equidistant from the fulcrum of the beam balance, we observe that two weights on a beam balance. From this observation and the lever law, we infer that their weights are equal; and, if we know the weight of one (the standard) we can infer, by the lever law, the weight of the other. However, such laws are about unobservables only to the extent that the mathematics used in the measurements is about unobservables; the laws are about theory-dependent concepts.

The purpose of the kinetic-molecular theory of gases was to explain all the known laws of gases and to predict new laws by bringing the behavior of gases within the reach of classical mechanics. In establishing such a theory we may start with the known laws for gases. All of the laws may be summarized in the quantitative law 'PV/T=k' (where 'P' is the gas' pressure, 'V' its volume, 'T' its temperature, and 'k' the gas constant). According to the model adopted, gases are ensembles of submicroscopic particles called 'molecules.' These particles are perfectly elastic spheres which have mass, size, and velocity. Their total mass is the mass of the gas, and their total volume is the volume of the gas. By applying Newtonian laws of mechanics to these molecules, one may derive deductively the formula 'PV=2/3 total kinetic
energy of the molecules.' Assuming that 'Tk=2/3 total kinetic energy of the molecules,' the gas law is derived deductively.6

It might be asked what justification we have for assuming that 'Tk=2/3 total kinetic energy of the molecules.' It would be justified if we could successfully predict phenomena from it alone. Consider two gases. If they have the same temperature, then they have the same kinetic energy. From their having the same kinetic energy, it may be derived that the ratio of the average velocity of their molecules is equal to the square root of the inverse of the ratio of the masses of their molecules. (It is assumed that the gases are pure and that the molecules of each gas are identical in size and mass.) Can that derived law be confirmed? We take two gases having different masses and being placed in two independent containers of the same volume. Each container is pierced with a small hole and the gases escape from the container into a vacuum. The gases' pressures, temperatures, and velocities are measured. It is predicted that the lighter gas, the one having less mass, will escape faster from its container than the heavier gas. The prediction is verified and the law confirmed. Thus we are empirically justified in making the above identity, viz., 'Tk=2/3 total kinetic energy of the molecules.'

The experiment just discussed is quite important. It is a direct confirmation of the predicted law (L1), viz., 'The ratio of the velocity of two gases having the same pressure, temperature, and volume, is

equal to the square root of the inverse of the ratio of their masses.' Since (L1) is derived from (L2), viz., 'The ratio of the average velocity of the molecules of two gases having the same kinetic energy is equal to the square root of the inverse of the ratio of the molecules' masses,' (L1) indirectly-confirms (L2). And since (L2) is derived from the assumption that "Tk=2/3 total kinetic energy of the molecules," (L2) indirectly-confirms that assumption. The important question is this: From all the observational data prior to the formulation of the model and to the discovery of how the temperature of a gas is related to the kinetic energy of the gas, could we have inferred (L1)?

In answering this question we have to know what our observational data was. The data was summarized in the gas law 'PV=TK.' Deductively, from that law and the fact that two gas samples have the same pressure, volume and temperature, we cannot validly infer (L1). From analogical data such as "gas a is such that at time t and place p 'PV=TK' is true of it," "gas b is such that at time t-1 and place p' 'PV=TK' is true of it," we cannot analogically infer (L1) will hold for a and b. However, if we add to our statements of evidence the Newtonian laws of mechanics which were in fact used in the formulation of the model (or their instances with respect to a and b which would be used as evidential statements in the I-A model), (L1) would follow analogically from our evidence. (It simply follows from the law of kinetic energy that two gases whose masses and velocities are so related have the same kinetic energy.) Furthermore, by experimentation it will be found that all gases which have the same kinetic energy will
also have the same temperature, pressure, and volume. If the experimenta-
tion is carefully done, it will be found that \( PV = Tk = \frac{2}{3} \) total
kinetic energy of the gas.\(^1\) We have arrived at the same conclusion
with the kinetic-molecular theory of gases that we did with Braith-
waite's more abstract theory, namely, we can derive the same conclu-
sions that are derived from a theory merely by using the evidence
utilized in formulating the theory. One of Smart's criticisms against
instrumentalism is to the effect that one can present an instrumental-
ist formulation of a theory only after it has been given a realist's
interpretation.\(^7\) Yet the analysis of both Braithwaite's theory and the
kinetic-molecular theory of gases implies that whatever can be empiri-
cally established by these theories can also be predicted prior to
their formulation by the I-A model. Thus Smart's claim seems to be
false. It seems, moreover, that anyone who claims a theory can predict
phenomena which the evidence used in formulating the theory cannot pre-
dict is mistaken. A theory, being a deductive system, neither contains
nor entails anything more than was used in formulating it.

Section II

Popper's Critique of Instrumentalism

Although the above conclusion seems decisive enough, there may be
a way of circumventing it. Consider Popper's view that theories are
conjectures. Theories, he holds, are not formulated from what we have
observed; they are pure mental linguistic inventions. Yet in making a

\(^7\)Smart, Between Science and Philosophy, p. 150.
prediction, we need a statement of initial conditions. As a simplified example, let us say that we conjecture that all bears are mammals. We have individual tests for the predicates 'being a bear' and 'being a mammal.' Furthermore, we have only observed one bear, Smokey, and we are going to test whether he is a mammal. Thus merely from the statement of initial conditions, 'Smokey is a bear' and 'All bears are mammals' (a conjecture or guess) we predict that Smokey is a mammal.

The important point here is that our universal conjecture was not formulated specifically from observational statements. Hence we presumably have no analogical evidence for predicting that Smokey is a bear. Such situations of lack of evidence seem to be rare. But when we deal with an anomaly, an unexpected event, we may in fact postulate a conjecture rather than search for other similar anomalies. Yet after the first test of our conjecture, our reasoning may then continue analogically. Thus if our prediction 'Smokey is a mammal' turns out true, then we predict by analogy that the next bear we observe will also be a mammal.

Such a deductive procedure will, claims Popper, always proceed from a guess. If, however, the question comes up as to why the theory in question was conjectured and not another, then an analogical formulation of the theory may easily appear, e.g., all the large observed animals in the woods were found to be mammals and Smokey is a large observed animal in the woods. The question now is: Can we justify our theory without supposing evidence that can be used in conjunction with the statement of initial conditions to predict analogically what the theory and the same statement of initial conditions predicts deductively?
Popper believes that the only justification for a theory is testing it by means of predictions in the attempt to falsify it. He holds that evidence only counts for a theory when it is the result of testing the theory. Although a theory may only be tested or justified by means of predictions deduced from it, is a theory conjectured independently of observation? Popper holds that the theory we conjecture "will have been preceded by observations -- the observations . . . which it is designed to explain."

In formulating three requirements for the growth of knowledge, Popper holds that, first, the theory should connect hitherto unconnected phenomena by means of a "simple, new and powerful, unifying idea." Such an idea, for example, is the idea of gravitational attraction. Second is the requirement of independent testability, i.e.:

apart from explaining all the explicanda which the new theory was designed to explain, it must have new and testable consequences (preferably consequences of a new kind); it must lead to the prediction of phenomena which have not so far been observed.

This second requirement is presented by Popper to avoid ad hoc theories. Any theory meeting the second requirement will be able to explain everything that the theory it replaces explained as well as phenomena not explained by the old theory. The third requirement is "that the theory

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9 Ibid., p. 55.


11 Ibid.
should pass some new, and severe, tests." Popper claims that instrumentalism can only account for the prediction of events of the kind already known, whereas realism can account also for the prediction of new events.  

Reverting to the third requirement, in what sense are these "new events" new? They could be new in the sense that they were not predictable from the theories which have been replaced by the new theory. However, this sense of 'new' does not eliminate inductive formulation of a theory because one can derive inductively a theory from the evidence which is both explained by the old theory and which constitutes its refutation. Yet such a procedure, as noted above by Popper, is an ad hoc technique. Thus the new theory, besides explaining what the older theories adequately explain, must also adequately explain their refutations, and must also permit us to predict events never before observed. Such a theory in fact would be a guess, a testable conjecture. As an example, assume that Galileo's parallelogram law was derived by inductive generalization from observations of rectilinear motions only. Pap maintains that from a statement of the initial conditions that a body moving in uniform rectilinear motion is suddenly acted on by a central force, and from the rectilinear law, we may deductively predict that it will move in a circle around the central force. Such circular motion is predicted though it was never before observed. This example, as well as the one about the bear as a

12 Ibid.
14 Pap, op. cit., p. 369.
mammal, tend to show that the I-A model is not logically necessary for predicting phenomena and, in fact, that there are some cases of prediction in which it is not at all applicable. Such predictions are derived deductively from conjectures that could not be inductively derived from prior observations.

Popper has presented a serious criticism of the I-A model as model adequate for all scientific explanation and prediction. Since I defended descriptivism by means of the model, this criticism also affects that defense. Although the gas law was probably derived by inductive generalization, one wonders whether Newton derived his laws of motion and, especially, of gravitation in that manner. Even though it seems that some laws and theories began as conjectures or guesses, it should also be noted that most of these theories could have been derived inductively either analogically (as summary statements) or as inductive generalizations.

Is there any way in which the I-A advocate could answer Popper's criticisms? The I-A advocate could claim that a newly presented theory has two parts: (1) that part which predicts all the phenomena the replaced theory predicts as well as the anomaly the replaced theory failed to predict; and (2) that part which predicts a new unobserved phenomenon. The I-A advocate could argue that the only justified part of the theory is the first part since the second part, being merely a guess, has no foundation whatsoever. That it has no foundation can easily be seen from the following. The second part definitely cannot be deduced from the first part if it is to predict new phenomena unpredictable from the first part. The second part cannot be established
inductively from the first or else the new unobserved phenomena could be predicted analogically from the evidence. Thus the second part is unfounded: we do not even have indirect evidence for it.

If, however, the second part of the theory is tested and is successful, the theory is no longer unfounded and the evidence obtained will now enable us to explain and predict by the I-A model everything the newly tested theory explains and predicts. Hence it seems that one can only reduce a theory to the I-A model after it has been tested. This last point would substantiate Smart's comment, above, about instrumentalist interpretations of theories—that they can only be formulated after realist interpretations. But the interesting point is that one apparently has a realist interpretation of a theory only if the theory is partially unfounded. Although some realists may not favor such a description of realism, it seems that they would have to hold it to avoid being reduced to instrumentalism. Popper's comments on theories as guesses or conjectures indicate that he has accepted that point. Yet it seems that the second, or conjectural, part of a theory is totally unrelated to the other part. To be sure both parts must be consistent, but what significant relationship is there between them if it is neither deductive nor inductive?

Perhaps the best way of evaluating this point is to consider a simple, formal example. Let us say that we have a theory, (TH1) which explains 'Aa • Ba' because of its law '(x)(Ax ⊃ Bx).' However, from further experimentation one has discovered that 'Ab • ¬Bb' is also true. Thus, (TH1) is false. Our new theory, (TH2) must explain both 'Aa • Ba' and 'Ab • ¬Bb,' and predict a new phenomenon. Theory (TH2) postulates
that C is causally connected to B, and that -C is causally connected to -B. However, C has not been observed. Thus (TH2) contains the two following laws: (L1), "'(x) \([Ax \cdot Bx) \supset Cx]\)' and (L2), "'(x) \([Ax \cdot Bx) \supset -Cx]\)." (Together (L1) and (L2) are equivalent to (L3), viz., "'(x)(Ax \supset (Cx \equiv Bx))'.") On the basis of 'Ca' and (L2), (TH2) explains 'Aa \cdot Ba.' On the basis of '-Cb' and (L1), (TH2) explains 'Ab \cdot -Bb.' However, (TH2) also predicts that whenever 'C' is observable, so is A and B; and whenever -C is observable, so is A and -B. If further testings invalidate these predictions, then new observables, not yet observed, are postulated in the hopes of developing a more adequate theory.

Although (TH1) does not entail (TH2), it does entail (L2), i.e., ''(x)(Ax \supset Bx)'' entails ''(x) \([Cx \supset (Ax \supset Bx)]\)'' which is logically equivalent to (L2). (L2) is indirectly-confirmed\textsubscript{\textsuperscript{1}} by (TH1). (Since (TH1) is false, it also indirectly-disconfirms\textsubscript{\textsuperscript{1}} (L2).) However (L2) is just one of many laws derivable from (TH1). (TH1) offers no greater evidence for (L2) than any of the other laws. It seems to follow, then, that prior to testing (TH2) we are not justified in holding it any more than any other theory. Perhaps it will be replied that we are definitely more justified in holding (TH2) than (TH1), because (TH1) is known to be false, everything that (TH1) explains and is true is also explained by (TH2), everything that refutes (TH1) is also explained by (TH2) and, finally, because (TH2) predicts that 'C' accompanies 'B.' (This latter point is comparable to the conjecture 'being a mammal' accompanies 'being a bear.') Such would be Popper's reply.

Yet it may be asked, how do we know that 'C' designates an
observable or testable property? That 'C' is an observable and that we know how to determine it are essential for testing (TH2). It also seems to be the case, though trivially so, that observable properties are only known through observation. Thus if we use 'C' in a theory, apparently C has been observed by us. Yet such a requirement does not mean that we have observed C in conjunction with (especially) B. What the new event (TH2) predicts is just that, viz., the conjunction 'B • C' and the conjunction '-B • -C.' (Compare Pap's example above. In predicting 'circular motion' we must have been acquainted with circular curve and moving things. The theory predicts a new event having both properties, i.e., moving in a circular curve.)

The preceding discussion, nevertheless, is sufficient for adopting the I-A model. Let us say that, by means of some observational techniques, we have determined as true 'Aa • Da.' By other experiments we determine as true 'Cb • Db.' Since 'Aa • Da' entails 'Da,' from 'Da' in conjunction with 'Cb • Db' we can infer 'Ca' by analogy. Also, since 'Cb • Db' entails 'Db,' from 'Db' in conjunction with 'Aa • Da,' we can infer 'Ab' by analogy. Thus we may predict 'Aa • Ca • Da' and 'Ab • Cb • Db' from statements of evidence, analogically derived from 'Aa • Da' and 'Cb • Db.' Although such procedure may be trivial (e.g., 'D' could merely be 'physical object'), the predictions do proceed by analogy and are testable. The event described by 'Ab • Cb • Db' is different from the event described by 'Cb • Db.'

It would seem, then, that even in conjecturing hypotheses we may use analogical reasoning. In light of prior knowledge, for example, the kinetic-molecular theory was postulated. It was known that the
behavior of planets, cannon balls, and pebbles could be predicted accurately enough by using Newton's laws. Due to the fact that natural inanimate objects tend to assume a spherical shape and that objects which seem to be solid from one point of view appear to be porous from another, the model of ensembles of molecules for gases is formulated analogically. Since planets, balls, and pebbles vary in size while mechanical laws apply equally to all of them, one also infers by analogy that, if there were molecules, mechanical laws would also apply to them. Such analogical reasoning enables us to decide before testing which model or theory is most credible, i.e., which has the better prior probability. Without a comparable procedure of justification, one is apparently in the position of deciding between guesses having the same prior probability.

Although Popper's justification of why this conjecture or theory rather than another seems to be adequate, what should be noted is how we ascertain the different aspects of that justification. For example, how do we know that the theory predicts an observable property? Apparently, we must have observed it. But admitting that point is sufficient for admitting the I-A model of justification. Yet such an admission seems to be essential in justifying why our theory is better than the preceding one. The I-A model can presumably be used even when we are required to predict new phenomena. Thus descriptivism and instrumentalism are still defensible.

Popper's main criticisms of instrumentalism are that it cannot explain (1) how theories are able to predict new unobserved phenomena
and (2) why "theories are tested by attempts to refute them. . . "15
According to the instrumentalist, scientific theories are linguistic
devices that enable us both to organize statements about phenomena and
to make predictions and inferences from one set of statements (the
statements of initial conditions) to another (the explanandum or
statement of the predicted event). Popper compares such devices to the
computation rules of the applied sciences.16 He attempts to show that
there are significant differences between trying out computation rules
and testing theories. Popper believes that one selects as a test of a
theory a case in which we "expect the theory to fail if it is not
true."17 But by analogy, Popper argues that it makes no sense to say
that we subject an instrument to those tests in which we expect it to
fail if it is not adequate. Instruments are not tested to determine
whether they are to be rejected; they are tested to determine their
range of applicability.

Let us now evaluate Popper's argument.18 First of all, the
instrumentalist claims that theories and laws are linguistic instru-
ments. Thus any analogy with a physical instrument, such as Nagel's
hammer, may break down. Secondly, the instrumentalist holds that these
linguistic instruments function as rules of inference for making pre-
dictions. Although it would be unusual to test a computation rule, it

15Popper, "Three Views Concerning Human Knowledge," p. 112.
16Ibid., p. 111.
17Ibid., p. 112.
18Ibid., pp. 111-119.
does not seem to be unusual to test rules of procedure. A rule, as the instrumentalist considers theories, entitles one to perform an inference from certain given-as-true statements of initial conditions. Yet rules of procedure are at times rejected. They are rejected for various reasons. Perhaps the most usual reason is that following them did not yield the expected result. In following a theory or a law as a rule of inference we hope to make successful predictions. But if it fails at times to enable us to make successful predictions, then it is modified or replaced by another rule which we hope to be more successful. Although it may be the case that rules cannot be refuted because they are neither true nor false, like statements, they may be rejected for others. Popper argues that through the refutation of a theory we obtain new information, viz., that the theory is false. But to this comment, the instrumentalists can reply by saying that through the rejection of a rule we also obtain new information, viz., that the rule is not so useful as we hoped it would be.

An instrumentalist can likewise account for the severe testing of a theory. Instead of trying to falsify his theory, he will be trying to find an exception to his rule, i.e., he will try to discover truth-conditions such that his rule of inference enables him to derive a false statement from a true premise. It should be noted that not all the rules of inference the instrumentalists use are deductive. Hence there is a genuine problem as to whether there are certain truth-conditions such that, upon applying his rule-of-inference instrument to them, he infers a false from a true statement.

Another criticism from Popper is that instrumentalism cannot
account for scientific progress. Popper argues that only through attempting to refute theories and in replacing them by other theories (in the manner discussed previously) can we have scientific progress. Yet two options seem to be open to the instrumentalist. If his rule of inference does not meet with his specifications, he may keep modifying it until it does, or he may replace it by another rule of inference which: (1) enables him to make all the successful predictions the former rule did from the same premises or statements of initial conditions; (2) enables him to make true predictions from the same premises that yielded false predictions in accordance with the old rule; and (3) enables him to make predictions of a kind different from the old, rejected rule. Thus the instrumentalist seems to account for accepting and rejecting rules of inference in a manner analogous to Popper's. Instrumentalism can account for progress as much as realism. After all, it may just as easily be the case that the reason why a scientist keeps amending his theory is his intense belief that his theory is true. (Although the theories of Velikovsky, Reich, and Hubbard, for example, have been defended via ad hoc amendment techniques, they all believe their theories to be more than mere rules of inference.\(^\text{19}\))

As noted in Chapter I, Section III, Popper is a realist who believes that scientific theories are statements about the world. He likewise believes that the distinction between theoretical statements and observational statements is untenable because he finds that

\(^{19}\text{Cf. Martin Gardner, }\textit{Fads and Fallacies in the Name of Science} (\text{New York: Dover Publications, Inc., 1957}).\)
theoreticism is sound. (We have seen, however, that theoreticism offers no significant threat to descriptivism, instrumentalism, and the I-A model.) Popper also believes that all properties are dispositional. He holds that physical objects, for example, possess certain properties even when those properties are not observable. This view is definitely realistic.

Popper holds, moreover, that a statement like 'Here is a glass of water' cannot be observationally verified. The reason is that both 'glass' and 'water' are universals or dispositionals and cannot be reduced to any set of observations.20

By the word 'glass' . . . we denote physical bodies which exhibit a certain law-like behavior, and the same holds for the word 'water.'21

On the basis of this point, Popper defends his theoreticism; every descriptive statement is a theory or a hypothesis, because it uses universal or dispositional predicates which are not reducible to any set of experiences. What Popper is saying is that not even singular statements are verifiable.22 But if they are not verifiable, then how is it possible to test laws? Popper holds that singular statements describing empirical situations are testable, but not verifiable. In other words, such statements are completely falsifiable, but not completely verifiable, because complete verification "entails an infinite number of test statements."23


21 Ibid.

22 Ibid., p. 424.

23 Ibid.
But if a test can completely falsify one singular statement, then would it not be the case that the contradictory of the falsified singular statement is verified? Popper does not believe that such is the case. Any statement that is tested is tested by means of a test-statement (statement of initial conditions) which is assumed to be true. Because we test the singular statement on that assumption, Popper believes that we can only determine the tested statement's falsity, not its truth.

Yet Popper seems to be incorrect here. The test-statement itself is a singular statement. Thus if it has any bearing on the testing of the other statement, then either both statements are materially equivalent (that is, either both true or both false) or logically contradictory. Popper has confused the testing of a law with the testing of a singular statement. Both are tested by means of an assumed test-statement which itself is singular. Due to the fact that the test-statement itself is singular, it will have a logical relationship to the tested law that is different from the logical relationship it has to the tested singular statement. And, since Popper denies inductive relationships, the only possible relationships between the two singular statements are the above mentioned ones. (If both statements were merely logically consistent with each other, the one would not be the test-statement of the other.) Apparently Popper's asymmetry between verifiability and falsifiability breaks down when singular statements are tested. (If one can only falsify one statement by assuming another, then why can one not verify a statement by assuming another?) Consequently, his claim that every descriptive statement is non-verifiable
because it contains a dispositional predicate also breaks down since each test-statement also contains a dispositional predicate.

Nevertheless, Popper's main claim still stands—viz., that all descriptive statements transcend experience. How can the instrumentalist explain this situation? The instrumentalist would hold that such statements indeed transcend experience, because he uses the descriptive words in them to designate many experiences. Such designation is made on the basis of similarities between one experience and another. If, moreover, it is replied that similarity and repetition "always presuppose the adoption of a point of view," the instrumentalist will agree. However, his point of view need not be a statement of assumed fact about the world's structure or nature. His point of view is, so to speak, to determine which repetitions or similarities enable him by analogy to make the most successful predictions. If it is then replied that he will never know which regularities are reliable, the instrumentalist can counter by saying that the realist likewise cannot determine which laws are true.

If asked to justify 'Here is a glass of water,' the instrumentalist replies that the object, as he is observing it, has properties similar to things he has designated as being glass and water. As to his justification of 'this sugar cube is soluble in water at time t,' two possibilities are open to him. First, the instrumentalist could say that all the information, conveyed by 'this sugar cube is soluble at time t' is conveyed by the following material rule of inference: "'x is

\[ \text{"'x is} \]

Ibid., pp. 421-422.
dissolving at $t'$ is derived from 'x is a sugar cube at $t'$ and 'x is in water at $t$." This line of analysis is comparable to Carnap's reduction-sentence technique. Second, the instrumentalist could say that the statement means that there are true statements about pieces of sugar dissolving in water such that from the statement of initial conditions, 'this is sugar and is placed in water at $t-1$, we predict by analogy that 'this will dissolve at $t'$ a time similar to that time required for the other sugar to dissolve.

Popper's and Feigl's criticism of both possibilities would be that the instrumentalist has confused the evidence for 'this sugar cube is soluble' with its meaning. But the instrumentalist can reply that the meaning of 'this sugar cube is soluble' is determined by its use; and, its use is to show how certain singular statements are connected either by a material rule of inference or by analogy. Apparently Popper's criticisms can be adequately refuted by the instrumentalist.

Section III

Feyerabend's Critique of Instrumentalism

Feyerabend disagrees with Nagel and Pap, and holds that the issue between realism and instrumentalism is not merely verbal. He believes that "realism is preferable to instrumentalism."$^{25}$ Basing his analysis on Popper's theoreticism, Feyerabend divides the empirical content of a theory into three groups:

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the events which support the theory, the events which disconfirm it, and the events which prima facie disconfirm the theory but whose "real nature" is left undecided pending further investigations.²⁶

If we have to choose between two theories T and T' such that their second class seems to be empty (i.e., there are no known events which disconfirm either of them), then we choose that theory having the fewest prima facie falsifiers. If our two theories were the Ptolemaic and the Copernican theories then, given Aristotelian physics as the background theory, the Ptolemaic would be preferable to the Copernican. Although the curious characteristic of the Copernican theory was its mathematical simplicity, this characteristic can be explained either factually or formally; i.e., the simplicity reflects the structure of the solar system or it arises from the formalism of the theory itself.

Feyerabend admits that the Copernican theory was entertained or introduced merely as a more effective calculating device, not as revealing the structure or nature of reality.²⁷ Later, however, a new background physics was formulated which had better predictive and explanatory import than the Aristotelian, and which supported the Copernican rather than the Ptolemaic theory. By means of this example, Feyerabend tries to show that eventually realistic interpretations of theories triumph over their instrumentalistic interpretations, especially by the result of detailed research. Commenting on this historical example, Feyerabend queries:

Is this not a splendid argument for realism? Does it not show that the realistic position encourages research

²⁶Ibid., p. 283.
²⁷Ibid., pp. 288-289.
and stimulates progress, whereas instrumentalism is more conservative and therefore liable to lead to dogmatic petrification.\textsuperscript{28} Feyerabend likewise believes that eventually the same situation will happen in quantum mechanics.

Is Feyerabend's argument sound? I believe that it is not. His argument establishes neither realism nor instrumentalism since the historic issues are compatible with each view.

The primary reason why the Copernican theory was not given a realistic interpretation was that the Ptolemaic theory had already been given such an interpretation. It was influential realists such as Osiander and Bellarmine who unwittingly forced the instrumentalistic interpretation on the Copernican theory. A scientific instrumentalist is, as Smart says, an instrumentalist with regard to all scientific theories.\textsuperscript{29} And since the instrumentalist is looking for the best predictive rules of inference, more than likely he would have chosen the Copernican over the Ptolemaic. It thus seems that this historical example, on the contrary, is a splendid argument for instrumentalism.

Frequently the cause of conservativism is the realistic interpretation of theories, especially the background theories. The instrumentalist argues that if these theories are reformulated into definitions, material rules of inference, or (at most) hypothetical models, then the air will be rid of dogmatic petrifaction. The instrumentalist searches for the best linguistic system that enables him to organize and predict phenomena. Now, of course, there are lazy instrumentalists

\textsuperscript{28}Ibid., p. 302.

\textsuperscript{29}Smart, op. cit., p. 151.
who patch up their rules if they are not too reliable, but then there are some lazy realists who amend in ad hoc fashion their theories. Yet surely there are the searching and researching realists and instrumentalists. Thus Feyerabend's argument is unsound. What encourages research, it seems to me, is the attempt to make new theories and/or rules of inference for explaining and predicting old and new phenomena; and this is compatible with both views.

Section IV

Fictionalistic Agnosticism and Models

Instrumentalism might also be labeled fictionalistic agnosticism. The latter is Feigl's term for the view that theoretical concepts are merely useful fictions having no factual reference. Such examples as ideal gas, perfect elasticity, and instantaneous velocity are held to be convenient fictions by most scientists. Smart, a realist, even holds that line of electric force is also a useful fiction. The reason why he believes that it is a fiction is that \(2\pi\) lines of force are supposed to leave a unit charge.\(^3\) There does not exist \(2\pi\) of anything, according to Smart. The instrumentalist, however, goes further and says that electrons and genes, for example, are either fictions or may be considered as fictions. Since the second alternative is the weaker and must be true in order for the other alternative to be true, I shall concentrate my analysis on it.

If these concepts have no factual reference or we may use them in

\(^3\)J. J. C. Smart, Philosophy and Scientific Realism (New York: Humanities Press, 1963), p. 34.
such a way that the question of their factual reference never arises, then what function do they have? For the instrumentalist, their function is to enable us to make a variety of predictions. Thus the predicate 'having the same weight' enables us to infer 'objects A and B will balance each other on the beam scales' from 'A and B stretched the spring scales equally.' The same analysis, as we have seen, can be applied to all dispositional concepts. Such concepts seem to function as the instrumentalist claims—as connectors enabling us to infer one statement from another. However, as we noted in the preceding chapter, theoretical terms do not link statements together as dispositional terms do. (For the technique of eliminating dispositional predicates, see Section III of Chapter V.)

Let us now consider the function of theoretical terms and statements according to the traditionalist's view.31 We have, for example, two theoretical statements 'T_1' and 'T_2' and two observational statements 'O_1' and 'O_2.' Now according to the theory, 'O_1' is given as inductive evidence for 'T_1,' 'T_1' entails 'T_2,' and 'T_2' entails 'O_2.' However, the observational statements 'O_1' and 'O_2' permit the same inference as was made with the theoretical statements of the theory. A comparable situation happens when one uses the I-A model of explanation. It appears as if the theory merely connects statements of initial conditions to statements of explananda or predictions. Of course, the theory connects other statements besides 'O_1' and 'O_2,' but that is all it does. If the above theory only enabled one to infer 'O_2' from 'O_1' then, given 'O_1,' it is logically equivalent to the

statements '0₁⇒₀₂' and '₀₁.' Furthermore, '₀₁⇒₀₂' is convertible into the material rule of inference "'₀₂' is derivable from '₀₁.'" Thus our theory, which originally had four statements, is reduced to having only two statements and a material rule of inference. This technique may be applied to any scientific theory since the traditionalist's conception of the structure of scientific theories seems basically correct.

Smart criticizes the above analysis by saying that if 'T₁' and 'T₂' were not true, then '₀₁⇒₀₂' being true is inexplicable. Smart argues that such a view implies a kind of cosmic coincidence—that galvanometers and scales behave as they do even though there are no such things as electrons and gravity. On the other hand, if one adopts a realist position, such behavior is explicable. A similar argument is advanced by Toulmin. According to Toulmin if a predictive argument is successful, then its success needs to be explained. To put (especially) Smart's point another way, the realist interpretation of a theory can explain the instrumental value of theories, while such an explanation is not possible if one adopts the instrumentalist's position. Also, Smart finds

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32 Smart, Between Science and Philosophy, p. 150-152.
33 Toulmin, op. cit., p. 36.
34 Ibid., pp. 151-152.
Thus if Smart or Toulmin were given an I-A model explanation, I likewise do not believe they would be satisfied. Why are the temperature, pressure, and volume of this gas related in the manner expressed by the gas law? Because another gas's temperature, pressure, and volume are also related in the same manner, or because the kinetic-molecular theory is true? Smart chooses the latter alternative "because it shows an analogy between a gas and a swarm of particles." Yet both types of explanations are analogical. The I-A model explains one singular statement in terms of other analogous singular statements. The kinetic-molecular theory explains one law by analogy, but in terms of other laws. The gas law, for example, is shown to be in some sense analogous to the laws of mechanics.

The I-A advocate and the fictionalistic agnostic may also explain and predict by using models, especially theoretical models. Yet they deny that the theory is distinct from the model. The fictionalistic agnostic, moreover, will hold that the model is either false or cannot be inductively established as true; we are always as-if thinking when we use such models. Curiously enough, Popper is close to the fictionalistic agnostic. He also holds that theories cannot be inductively established and that most theories will be falsified. In fact, the kinetic-molecular theory is not strictly true; however, it is more convenient than the other models and, except in highly precise experimentation, the other models are seldom needed. The agnostic could hold that we have evidence that the rigid-sphere model is more warranted than the elastic-sphere model because we get more accurate predictions

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from the former model. Yet inductive evidence that something is more warranted is not evidence that it is true. Furthermore, the major internal difference between the two models is their degree of analogy to what we actually observe. The rigid-sphere model is the better analogy. It appears, then, that 'molecule' actually does function as a dispositional predicate. In the models it connects mechanical laws to the gas laws.

From the models, it appears that the connection between observational predicates and theoretical predicates is stronger than that presented by the traditional theory. Statements of initial conditions are not merely inductive evidence for theoretical statements. The logical connective of correspondence rules seem to be material equivalence—e.g., statements using 'temperature' are extensionally equivalent to statements using 'mean kinetic energy of a molecular ensemble.' (Compare the analysis of the biconditional in correspondence rules in Chapter I, Section IV.) We have also seen that the traditionalist constantly draws a distinction between models and theories. At most models and analogies serve in the context of formulating a theory or extending it into new territory. But once formulated or extended, the model or analogy may be dismissed.

Smart, however, is of a different opinion (from our discussions of the views of Hesse, Spector, and Achinstein on models, it appears that they would agree with him). Smart argues that to formalize a theory, to make precise all its terms, and to dismiss all analogical predications for univocal ones is to destroy the utility of a theory.36

The fertility of a theory consists in its inexact analogues which enable us to infer connections and to test analogical inferences that would never be possible in a formalized system. Smart seems to be agreeing with the traditionalist in holding that theoretical terms are only partially defined. Nevertheless, he means that theoretical terms are names of analogues drawn from the public domain, and that it is because they are analogues that they cannot be exactly defined by observation terms. Smart, moreover, advocates extending analogies as far as they will go in the hope of determining more accurate laws. I think that Smart's presentation is in better accord with the procedures of the scientist than some presentations because analogical inference has especially contributed to the formulation of theories.

One of two important questions, at this point, is whether theory-thinking is as-if thinking. We have seen that the traditionalist looks upon model thinking as as-if thinking. One reason for this is that models are considered to be didactic devices explaining the unfamiliar by means of the familiar. The second important question is whether theories are distinct from models. In determining the answer to these questions, we will have to consider criteria for 'reality.' Nagel's criteria are: (1) public observability, (2) empirical confirmation, (3) occurrence in more than one empirical or causal law, and (4) designation of an invariant. The kinetic-molecular theory has empirical confirmation and 'molecules' occur in several causal laws, but not in any empirical laws. Criteria (1) and (4) do not seem to apply, except for the laws' confirmation.

Bergmann's criteria (see Chapter I, Section IV, Part II) are
concreteness (or localization in space and time), acquaintance, simplicity, significance, realism or coherence, process, and model. As to his first criterion, molecules were, classically, definitely held to be in space and time. But since we do not perceive molecules, we are not acquainted with them. Molecules are simples in the sense that, according to the model, a gas consists of molecules. One can define a gas by means of molecules. Molecules meet the fourth criterion if they are described by statements of lawfulness believed to be true. Molecules seem to meet this criterion even if they do not exist, since laws can be expressed subjunctively and the laws holding for molecules explain the laws holding for observable physical (i.e., perceptual) objects. The realism or coherence pattern applies to the coherence of laws of perceptual objects. Since molecules are not perceptual objects, this criterion does not directly apply. However, it can apply if molecules, as physical objects, are reconstructed out of certain laws of perceptual objects such as flickers on screens, tracks in chambers, and ticks of counters. According to the process pattern molecules would exist since they are described by statements of initial conditions of gases from which the gases' earlier and future state descriptions may be deduced in accordance with laws. However, according to the model pattern molecules may not exist. It all depends on what one's basic language is. If it is a macro-language, then all micro-entities merely become certain ways of looking at macro-entities.

Of the above criteria, the ones most applicable for establishing the reality of theoretical entities are Nagel's second and third, and all of Bergmann's except the second. However, it would appear that
most of Bergmann's criteria become redundant when dealing with theoretical entities. The theory, by means of its laws, tells us that molecules are simples (3), that they occur in coherent laws (5) having empirical import (4). Thus, Bergmann's criteria amount to the following: theoretical entities are real if they are described in a well-confirmed theory. The theory will also tell us whether the entities are in space and time and, possibly, what independent status they possess. Thus Bergmann's criteria seem to reduce to Nagel's second and third criteria. If we add, as we should, that the empirical import is based on the publicly observable, then Nagel's first criterion may be added. In summary we have the following criterion for the reality of theoretical entities: they are real if their names occur essentially (not simply by addition, for instance) in laws or existential statements that are well-confirmed by public observation.

The realist would probably want to add that the laws and existential statements must be true. But how do we determine the latter? Hesse's criteria may help: (1) the theory should yield new successful predictions (also Popper's second requirement); (2) the individual theoretical entity should have, under certain conditions, observable effects (e.g., tracks of an electron in a cloud chamber); and (3) the terms of the theoretical entities should designate substantives. Hesse's weakest criterion seems to be the third simply because whether something designates an entity or a property or relation seems to depend on how one formalizes the theory. Her best criterion seems to be the second. Fictitious entities, such as perfect gas and 2π lines of force, have no observable effects.
The modified kinetic-molecular theory meets the summary criterion for reality. Thus molecules are real because they are described by a well-confirmed, non-ad hoc theory. But molecules could still be fictitious entities. Hence we must consider Hesse's criteria. 'Molecule' is a substantive term in the theory. But do we ever observe the individual effects of a molecule? The closest we have come to such a situation are photographs of supposed molecular structures and Brownian movement. A sceptic, at this point, might note that it is only by means of the theory that we can identify the photographs and movement as the effects of individual molecules. We need, he suggests, some means independent of the theory to identify these phenomena as effects of individual molecules. We conjecture that Smokey is a brown mammal, because he is a bear. We make the prediction 'Smokey is a brown mammal,' knowing that he is a bear; and, we can check out the prediction independently of the conjecture. But we can never perform an analogous procedure with regard to Brownian movement. All theoretical explanation, concludes the sceptic, is theory-biased and, therefore, inconclusive.

Such a criticism shows quite strongly the difference between an observational statement and a theoretical statement. The conjecture, 'If Smokey is a bear, then he is a brown mammal,' is observational because one can determine 'being a bear' and 'being a brown mammal' independently of the conjecture. But one cannot determine 'being an effect of a theoretical entity' independently of the entity's conjecture, i.e., its theory. The most a theoretician can do is use the theory to predict phenomena not yet observed (e.g., photographs of
such-and-such structures and certain types of movements). It is quite true that the success of such predictions does not conclusively establish the theory. But are we, nevertheless, justified in accepting the theory?—accepting it as true or as a good *as-if* model?

We are now in a position to evaluate Smart's criticism of fictionalistic agnosticism and the I-A model. Why is it the case that a predictive argument is successful? The I-A advocate answers because the statements of initial conditions and evidence were true and the prediction was true. No further explanation is necessary. Why is it the case that a model or a theory is a good instrument of prediction? The fictionalistic agnostic can answer that it has enabled us to infer true predictions from true statements of initial conditions and evidence. Smart's reply would probably be that he already knows that the model has enabled us to infer true predictions from true statements of initial conditions and evidence, since he knows that it is a good instrument of prediction. The agnostic can then reply that Smart already has the answer to his question; nothing more is required. To explain the instrumental value of a theory one only needs to show how it meets criteria for being instrumentally valuable. It would seem that Smart's argument fails to show that theories and models have to be considered true just because they have instrumental value.

Smart, in his argument, tends to suggest that one can only hold instrumentalism or fictionalism if one accepts or presupposes realism. However, it is false that there is any logical relationship of importance between realism and fictionalism. The realist advocates 'A is a B'; the fictionalistic instrumentalist, 'A appears as if it is a B.'
Is there any deductive relationship between these two statements? If A is a B, does it deductively follow that 'A appears as if it is a B'? Although 'This is a house' may be true, it does not follow that it has to appear as a house. In the night it may appear as a large white rock. Furthermore, if A appears as if it is a house, then it does not follow that it is a house. A stage prop may look like a house, but it is not a house. If it is argued that 'A is a B' is, in some sense, simpler to and, therefore, preferable to 'A appears as if it is a B' because in order to understand the latter one must understand the former, one may reply that we cannot understand any particular one without the other. As for simplicity, the former holds that there are both A's and B's; the latter only asserts that there are A's.

Extending this discussion to the kinetic-molecular theory, there are situations in which a gas does not appear to be composed of small elastic spheres. For instance, (1) a gas appears homogeneous, not granular, and (2) even ideal gas laws suggest that a gas may be a continuous compressible fluid. Under high pressure the molecules become closer together and their mutual attractions increase. As Smart notes, however, even on the analogy of the model and our knowledge of gravitational attraction, such behavior of the gas can easily be expected.37 In short, the kinetic-molecular model would merely be incomplete.

Are there any good arguments, instrumentally speaking, between accepting models as true and accepting models as mere analogies? As we have seen, Smart and Workman hold that an adequate explanans must be considered true. This same point is advocated by Hempel, who likewise

37Ibid., p. 461.
argues that analogies or models are useful in the formulation and extension of theories, but they explain nothing.\textsuperscript{38} Upon the formulation of the theory, the analogies and models may be discarded.\textsuperscript{39} Thus they play no role in explanation and prediction for Hempel. There seems to be disagreement over what is essential for an adequate explanation. Apparently Hempel believes that analogies are not adequate for such roles. Hempel requires some law for both explanation and prediction; the I-A advocate does not. Yet as we have seen, laws do not seem to be essential for predicting or explaining at least individual events.

In the preceding discussions, I have developed and argued that the I-A model of explanation and prediction is as sound as any other model. Furthermore, I have shown how extensive its role can be in both the formulation and the testing of scientific theories. Even theoreticism does not seem to escape its method. It would seem that the widely held view that a scientific theory has predictive consequences which its evidence lacks is incorrect if analogical inferences are permitted. There does seem to be a virtue in holding that scientific theories are analogies. Such a view supports Popper's claim that theories are always to be considered as tentative: they are always subject to refutation, they are never established completely. Thus, holding that gases should be considered as appearing as if they were composed of molecules, supports Popper's claim, whereas holding that gases are composed of molecules does not. Furthermore, as R. A. R. Tricker notes:

\textsuperscript{38}Hempel, "Aspects of Scientific Explanation," p. 441.

\textsuperscript{39}Ibid., p. 439.
If scientific theories are no more than analogies, then clearly we are able to employ the most convenient one for the purpose at hand.\textsuperscript{40}

Thus if we consider scientific theories as analogies, science will not present us with the ultimate nature or structure of reality. Due to the fact that the formulations and justifications of theories occur only in the phenomenal realm, there is no inductive warrant that they are indicative of any non-phenomenal realm. Also due to the fact of constant revision in theories, it seems best to say, at any moment in the life of a theory, that it tells us that matter behaves as if it were this way, not that matter is this way. The kinetic-molecular theory shows how the gas law can be analogically related to the laws of mechanics. It seems, therefore, that theoretical entities may be considered as fictions.

Section V

Eliminative Fictionalism and Rules of Inference

Eliminative Fictionalism is an extension of fictionalistic agnosticism. It holds that theories, or at least theoretical terms and statements, are eliminable and replaceable by statements of evidence and initial conditions and by rules of inference. Thus, if theoretical terms are dispensible in explaining and predicting phenomena, then that to which they presumably refer may as well be fictitious. The I-A model is a type of eliminational technique. It reduces all

theories and laws to singular statements connected together by deductive rules of inference and the analogical rule. Its virtue is that it does not need material rules of inference. However, as Nagel presents instrumentalism, it is the view that all laws and theoretical statements can be reduced to material rules of inference connecting only observational singular statements. We have already seen that this technique has some limited success. Only laws of the form '(x)(Fx ⊃ Gx)' may be converted into material rules of inference.

As was also noted, moreover, laws of more complex form have no direct role in the explanation and prediction of statements of individual events. Instead of discussing the truth or falsity of laws, as the realist might, the instrumentalist discusses the usefulness of rules of inference. But the difference between the realist and the instrumentalist seems to be exactly what Nagel claims: a difference in preferred modes of speech. Suppes argues that such a difference between these views is trivial and there is no good reason "to displace the classical semantical notions of truth and validity." Yet one may also wonder whether there is any good reason for not displacing the above classical notions; i.e., is it not the case that the difference is simply one of preference?

As we have noted earlier, Hempel shows that a problem arises if we want our factual language to consist of only singular assertions. Laws of the form '(x)(Fx ⊃ (3y) Rxy))' may be converted into material rules of inference of the form "A statement of the form '(3y) Rxy' is derivable from a statement of the form 'Fx.'" On the other hand, as I have

41 Suppes, "What is a Scientific Theory," p. 64.
also noted, such laws are not used to explain or predict phenomena.

Yet there is a way of avoiding this difficulty. Let us say that we have observed \( a \) to have property \( F \); also, we have observed \( a \) having relation \( R \) to \( b \). We also notice \( c \) to have property \( F \); but \( c \) does not have \( R \) to \( b \), \( c \) has \( R \) to \( d \). On the basis of these two sets of experiments, we may infer that all \( F \)'s have \( R \) to either \( b \) or \( d \). Thus we may formulate the rule of inference: "From a statement of the form '\( x \) is an \( F \) ' one may derive a statement of the form '\( x \) has \( R \) to \( b \) or to \( d \).'"

The instrumentalist, therefore, holds that '(\( \exists y \) \( R_{xy} \))' is equivalent in the context of a theory to a disjunction of singular statements. On the basis of this interpretation, one may predict and explain individual events by means of such disjunctions.

The problem with such an interpretation is that the rule becomes more complex as it is found that more and different objects have \( R \) to \( F \)'s. But one could avoid such complexity by establishing either the above equivalency or, preferably, another not having any quantifiers which is equivalent to 'having \( R \) to \( b \) or \( d \).' Thus, if "'\( Sx \)' \( \Leftrightarrow \)' '\( x \) has \( R \) to \( b \) or \( d \),'" then the law becomes '('\( x \)\( (F_{x\rightarrow Sx}) \)' which is not symbolically complex at all. The material rule of inference will be "A statement of the form '\( x \) is an \( S \)' is derivable from a statement of the form '\( x \) is an \( F \).'" One can use this same technique to convert laws of the form '(\( \exists y \)\( (F_{y \cdot (x \rightarrow G_{x \rightarrow R_{xy}}}) \)).' Thus Hempel's objections is overcome.

Another more popular objection to construing laws and theories as material rules of inference is that, as Hempel notes, scientists use laws as premises and believe their theories are true or false. 42

Popper has a similar argument for realism.\textsuperscript{43} It comes as no surprise that many philosophers of science are realists. What is more important is whether their mode of speech is any more logically justified than the instrumentalist's. There does not seem to be any such justification. Moreover, there are some advantages which the instrumentalist's mode of speech, as presented above, has over the realist's. If our criticism of Popper is sound, then singular observational statements are completely verifiable and completely falsifiable (either in the context of a theory or by a set of rules). One of the problems with the initial formulations of the criteria of either complete verifiability or complete falsifiability is that they violated what Hempel calls a condition of adequacy for criteria of cognitive significance:

\textit{If under a given criterion of cognitive significance, a sentence }S\textit{ is non-significant, then so must be its negation, }\neg S.\textsuperscript{44}

Now according to instrumentalism, only singular observational statements are synthetic and significant. Furthermore, if a singular statement of the form 'Fa' is verified (falsified), then automatically we have falsified (verified) the statement of the form '¬Fa.' Thus, instrumentalism meets the above condition of adequacy, whereas the standard view does not. (According to the standard view, laws are universal; test-statements, singular. Thus if 'Fa' is true, we may infer that '(x) ¬Fx' is false. However, if 'Fa' is false, we may not infer that '(x) ¬Fx' is true. Thus, the condition of adequacy is violated.)

\textsuperscript{43} Popper, "Science: Conjectures and Refutations," p. 63.
\textsuperscript{44} Hempel, "Empiricist Criteria of Cognitive Significance: Problems and Changes," p. 102.
A second advantage is that the I-A model of explanation, or the instrumentalist's model which incorporated material rules of inference, easily meets Hempel and Oppenheim's third and fourth explanation-requirements, viz., the explanans has empirical import and it is true. There is no guarantee on the traditionalist's account for meeting requirements three and four. However, it might be objected that the instrumentalist's view violates requirements one and two. Nevertheless, given the explanans and the material rule of inference, the explanandum is a logical consequence of the explanans, as much a consequence as any of a deductive argument. (It should also be remembered that Hempel and Oppenheim do accept inductive explanatory arguments.) Although the instrumentalist's view has no place for laws (thus *prima facie* violating requirement two), for every conclusion that the traditionalist infers from his law(s) and statement(s) of initial conditions, the instrumentalist can infer the same conclusion from the same statement of initial conditions and a material rule or rules of inference. Thus it is not clear whether requirement two is essential. From the same statement of initial conditions and at least one statement of evidence that the traditionalist would use to confirm the law, the I-A advocate can make the same inferences by analogy as the traditionalist does by his D-N or I-S models and as the instrumentalist does with this material rule of inference.

A third advantage of this material-rule-version of instrumentalism or of the I-A model version is that none of the paradoxes of confirmation arise simply because there are no universal unrestricted statements possible in either version. Thus these three important advantages may
well be sufficient methodological reasons for rejecting what Suppes calls the classical semantic views on truth and validity.

One might wonder just how material rules of inference compare with formal rules of inference. The main difference is that material rules may be rejected, whereas formal ones are not—not even the inductive formal rules such as analogy. Let us say that we have a rule saying the following: "From a statement of the form 'x is an A' a statement of the form 'x is a B' is derivable." We will have some other rule enabling us to decide when something is an A, and a third rule enabling us to decide when something is a B. By means of these latter two rules, we have a way of checking the reliability of our rules of inference. If we find that 'x is an A' is true, but 'x is a B' is false, then the rule of inference will be rejected provided we are satisfied experimentally with the truth of the premise and the falsity of the conclusion. This procedure is analogous to the traditionalist's rejection of a law. The law is held to be false; the rule, unreliable.

I grant that merely from this procedure the controversy between the realist and the instrumentalist appears to be trivial. Nevertheless, if there is no good reason for accepting instrumentalism, there also seems to be no good reason for rejecting it. Similarly, the same may be said of realism. But if we look closer at what the realist and the instrumentalist are saying, an interesting conflict between them may be seen. The conflict is not simply between laws and material rules of inference, but ultimately between deductivism and inductivism—between the views that all explanation and prediction is deductive and that all explanation and prediction is inductive. It is to this
conflict that I shall now turn.

It seems that the only way a realist can refute instrumentalism is to show that any other rules of inference besides deductive ones are unjustified. The descriptivist and the instrumentalist, as has been shown, can defend their views by using either the I-A model or the material-rule-of-inference procedure. They could even use both for simplifying their arguments. Although such a justification of deductive rules might be given, for example, by means of Venn diagrams or truth-tables, it seems to be indecisive. Brodbeck, for example, argues that valid deductive explanations and only valid deductive explanations justify an explanandum.\textsuperscript{45}

But is it not the case that any explanation justifies any explanandum if it is made in accordance with a rule of inference? Yet this line of reasoning seems to license any rule of inference whether it is valid or invalid. Thus it could be argued that any explanatory argument of the form Affirming the Consequent would be justified. An instrumentalist could reply to this criticism by saying that the rule of Affirming the Consequent has been found to be an inadequate rule of inference. So it, as well as all other deductive fallacious forms, is rejected as an inadequate tool of inference. But at this point a deductivist could reply that the analogical rule of inference is also an inadequate tool because it has licensed inferences from true premises to false conclusions. Here the deductivist criticizes the I-A model. An instrumentalist could accept this criticism by using only material

\textsuperscript{45}Brodbeck, "Explanation, Prediction, and 'Imperfect' Knowledge," p. 370.
rules of inference and rejecting the I-A model. The material rules of inference which he uses would be those which have heretofore yielded true conclusions from true premises.

Is there any way of saving the I-A model? The I-A advocate will have to say that in those cases where the analogical rule enables us to derive a false conclusion from true premises it is inapplicable. Such a situation might be compared to using a voltmeter in the place of an ammeter to determine the amount of current in a circuit. But since the analogical rule, unlike the material rule of inference, has a greater applicability potential, it is kept as a rule of inference and by means of it different comparisons are made for explanatory and predictive analogical arguments. The major problem with the I-A model and the material rule of inference technique is that their rules do not guarantee the truth of the conclusion if the premises are true. Only the deductive model gives us such a guarantee. And yet the deductive model is actually as risky as either the I-A model or the material rule technique.

What is risky in the deductive model is the law premise. The presumed law, like the rule of analogy and the material rules of inference, does not guarantee that from a true statement of initial conditions a true prediction will be derived. Furthermore, the interesting point is that whatever conclusion the deductivist infers, the inductivist (the I-A advocate or the material-rule advocate) can also infer, and conversely. This analysis seems to support only one conclusion, viz., that pragmatically the difference between these three procedures seems to be one of preference. There is, nevertheless, an important point to
bear in mind. Neither the I-A advocate nor the material-rule instrumentalist has advocated induction by enumeration as a means of validating laws or empirical generalizations. One simply cannot validate such statements; if anything, one can only confirm them. Nor have these instrumentalists advocated using the statistical syllogism (a form of the I-S model) which has problems of its own as discussed earlier. They have only advocated non-deductive rules of inference which enable one to conclude from statements of initial conditions what one can also conclude deductively via laws from those same statements. Thus it seems as though laws function in a manner similar to these non-deductive rules of inference; and, conversely, these non-deductive rules of inference function in a manner similar to laws. In comparison to the realist's explanation and prediction of laws, the I-A advocate can use summary statements and the material-rule advocate can use his technique as developed from Hempel's criticism.

Another criticism of using material rules of inference has been presented by R. M. Hare in his arguments against Toulmin who advocated such rules in both scientific and ethical reasoning. Hare claims that the rule of inference (say) "From a statement of the form 'x is an A' a statement of the form 'x is a B' is derivable" can only be justified if 'being an A' means, at least partially, 'being a B.' Of course, only in definitions does one find such a relationship. But the instrumentalist does not have to justify his material rules of inference in this manner. His justification is that they have been, or he hopes

they will be, effective rules for making predictions and explanations. The point is simply that material rules are not justified in the manner in which deductive rules of inference are justified.

Still such arguments may not satisfy the deductivist. Perhaps there is a distinction between 'explaining X' and 'giving evidence for X.' The I-A model gives evidence for a prediction, but it does not explain anything at all. But such an argument goes counter to the identity thesis which a deductivist, as well as the I-A advocate, would hold. Yet is there a difference between the above two concepts and, if so, is the difference crucial to the controversy between realism and instrumentalism? There does seem to be a difference between giving evidence for a law and explaining a law. One gives evidence for a law by finding confirmatory instances of it; one explains a law by showing how it is related to other more general laws. But since the instrumentalist does not consider laws as statements, this discussion does not affect instrumentalism. Can the distinction be defended with regard to singular statements? It would seem that when one explains a singular statement one already knows the statement to be true. On the other hand, one seems to be only concerned with evidence for a singular statement, if one does not yet know the truth of it. Thus in explaining a singular statement, one presents an explanatory argument in which the statement is the conclusion. In giving evidence for a singular statement, one presents a predictive argument ('predictive' in the broad sense discussed earlier) in which the statement is the conclusion. Thus this distinction does not in the least affect instrumentalism. I conclude that the deductive model, the I-A model, and the material rule
technique are equally justified for explanation and prediction.

Even if the above conclusion is unsound, let us determine whether instrumentalism can accept deductive explanatory and predictive arguments. It is obvious that instrumentalism as construed by the I-A model advocate or by the proponent of the material rule method cannot accept the realist's deductive model. But the fictionalistic agnostic need not be an inductivist. He can accept laws of the as-if form; i.e., universal statements of the form 'For any \( x \), if \( x \) appears as if it is an \( A \), then \( x \) is a \( B \).' The realist's comparable law would have the form 'For any \( x \), if \( x \) is an \( A \), then \( x \) is a \( B \).' For example, 'If two gases of different masses appear to be composed of molecules, then the ratio of their velocities is equal to the square root of the inverse ratio of their masses' would be the fictionalistic instrumentalist's law and 'If two gases of different masses are composed of molecules, then the ratio of their velocities is equal to the square root of the inverse ratio of their masses' would be the realist's law.

The difference between these two opponents is their attitude towards the model or theory. The realist believes that the model can explain and predict only if it is accepted as true; the instrumentalist believes that it explains and predicts only to the extent that it enables us to infer true explananda and predictions from true statements of initial conditions. In making an inference from his law the realist must assert that the two gases are composed of molecules; the instrumentalist need only assert that the two gases appear as if they are composed of molecules (which, unlike the realist's assertion, is true independently of the existence of molecules). The reason why the
instrumentalist can accept laws of the as-if form is simply that such statements characterize the model which is itself a mental-linguistic invention. It is we who determine what statements shall hold universally of the model. Even at this point we do not have factual laws; we only have laws of models—which is the reason for the 'as if' in the law. It indicates that it expresses an analogy.

In the context of a model an interesting thing happens. The I-A advocate and the material-rule instrumentalist can accept the as-if deductive model. In such an analysis as this, the laws of the model and the deductive rules of inference replace the analogical and material rules. This conversion will present us with the same statements of initial conditions found in the inductive models, universal laws of as-if form, and deductive inference rules. The explananda of the realist's deductive model, the instrumentalist's as-if deductive model, and the instrumentalist's inductive models are the same.

Section VI
Realism and Summary

The preceding arguments at least seem to vindicate instrumentalism. Are there any good reasons for being a realist? Let us first consider radical realism, viz., the view that theoretical entities are more real than observational entities. This view has been advocated in some form or other by Eddington, Sellars, and Bruce Aune. The best defense of this view is presented by Aune. Aune argues that when observational laws or generalizations break down, they are corrected or explained by
appealing to theoretical principles. For example, if we place a cube of sugar in tank of water and it fails to dissolve even after a week, then we attempt to determine why it did not dissolve. If we cannot determine the factor observationally then, on pain of rejecting the causal principle, we postulate that the factor is unobservable or theoretical. Aune's and Sellars' point can be put more generally: only theoretical entities and their principles or laws adequately explain phenomena. This point likewise is essentially Smart's which was presented against the as-if thinking of instrumentalism.

To Aune's argument it may be replied that we need only posit a model and note that the sugar appears as if some theoretical factor is involved without having to categorically assert the factor's existence. Secondly, in order to justify the existence of such an entity, Aune will have to tell us what new phenomena his theory describing the entity predicts. But in making a prediction of a new phenomenon, Aune will also need a different statement of initial conditions. And given that statement of initial conditions and a material rule of inference connecting statements of its kind to statements of the new-prediction kind, the same prediction can be inferred without the theory.

An argument similar to Aune's, though in a psychological context, is presented by I. E. Farber. Farber notes that at different times

48 Ibid., pp. 120-124; 169.
it has been observed that apparently similar environmental events produce different responses in the same individual. Assuming that the same cause produces the same effect, how do we account for the different responses of the individual in the same environment? Farber suggests postulating the existence of different unobserved stimuli in the individual under those same environmental conditions that produce the different response. Similarly, we can account for the similar response of an individual to different environmental stimuli by postulating the existence of different unobserved stimuli in the individual.

Although this procedure may seem reasonable enough, it violates the identity thesis. If there is no public way one could predict the individual's responses in those environments, not even with a statistical law, then we have no scientific explanation. Scientists using such a procedure will always be prone to ad hoc hypothesizing. The only way to forestall such criticism is to continue searching for an observable difference that correlates with the different responses. I believe that this example, as well as Aune's, shows how easily adoption of the realist view can slow scientific progress through conjecturing unobservables. On the other hand, instrumentalism would force one to re-examine in more detail the environments, the characteristics of the individual, and his responses to the environment in the hope of discovering an observable difference that will give predictive import. Of course, one could deny the identity thesis but, as noted earlier, that way leads to occultism and other shady countries.

Now it might be objected that since the instrumentalist must assume the principle of causation or the principle of the uniformity
of nature in his investigations, he must be a realist with regard to them, i.e., he must assume that they are true. But this objection is countered by noting that the instrumentalist holds these principles as general rules of his methodology which, like analogy, are not subject to rejection. If it is asked why these rules are not subject to rejection, it may be replied that they have been more reliable than most material rules of inference, that they enable us to formulate material rules of inference and discover statements of evidence, and that their application is far more general than any material rule.

It may be the case that theories enable us to predict phenomena more successfully than common-sense laws, such as sugar is soluble. But this point can also be stated in a less misleading fashion as the fact that scientists have found better ways of making successful predictions and explanations. The same comments are applicable to the moderate structural realism of Kneale which was also discussed in Chapter I, Section III. The problem with all realisms is that, in explaining individual events, theory does in fact act as a link between one observational statement (of initial conditions) and another (the explanandum). Yet this link can always be reduced to an equivalent rule of inference which is either material or analogical. It would thus seem that, in contradistinction to a central claim of the traditionalists, theories and models have the same role and upon analysis differ little.

Another important criticism of realism was mentioned by Brody and Capaldi, viz., that conflicting theories are used to explain the same phenomena. Their example is that liquids are sometimes treated as if
they were composed of separate particles and sometimes as if they were continuous media.\textsuperscript{50} What this example shows is that under certain conditions, the particle model is a better explanans than the media model and that, under different conditions, the reverse is true. Such a state of affairs gives evidence to the view that scientific theories are best considered as analogies. However, such a situation is likewise evidence for the realist who holds that, for given areas, the correct theory or model has not yet been formulated adequately, if at all. Consider, for example, Popper, the realist, who holds that scientific theories cannot by their universal unrestricted nature ever be firmly established; they remain forever tentative and corrigible.

Let us now re-examine Feigl's four realisms: (1) naive realism, (2) probabilistic realism, (3) hypothetico-deductive realism, and (4) semantic realism. The first two advocate induction. Yet (1) and (2) are inadequate if they, like Aune's and Farber's examples, advocate inductively inferring theoretical entities whose presence or existence cannot be determined independently of the phenomena from which we inferred them. To insure against non-\textit{ad-hoc} emendations, we have to turn to hypothetico-deductive realism and semantic realism (Popper's and Feigl's views). Semantic realism is hypothetico-deductive realism coupled with the doctrine that theories have a factual surplus meaning. Is semantic realism justified? Semantic realism holds the traditional view of theories and claims that theories have, besides observational factual reference, theoretical factual reference or surplus meaning.

\textsuperscript{50}Brody and Capaldi, \textit{op. cit.}, p. 47.
The deductive instrumentalistic view discussed in the previous section also holds the traditionalist view of theories but without the doctrine of surplus meaning. Hence the traditionalist’s view is neutral between the realism-instrumentalism issue. Since there is no logical connection between fictionalistic agnosticism and realism, one does not have to hold that theories must tell us how things appear to behave before we are justified in holding that theories tell us how things actually behave. Thus Wolfgang Yourgrau argues that, from photographs of supposed barium atoms, cloud chamber tracks, and the like,

it is not too gigantic an assumption that one discovers or finds real things, in our case particles; one does not discover thought models, hypothetical constructs, or the like.\(^5\)

It would seem that the intellectual move to realism or to instrumentalism is about equally justified. So I conclude with Nagel that both instrumentalism (as I have presented it) and realism (as presented by Popper and Feigl) satisfactorily meet central criticisms brought against them. But it does not follow that they are merely different ways of expressing the same thing. There are cognitive differences between saying that a gas is composed of molecules and saying that a gas appears as if it were composed of molecules. Consequently, I deny that realism and instrumentalism are two different ways of expressing the same thing.

I have shown in the above discussion the manner in which the major views of theories explain how theoretical concepts and theories enable

us to explain and predict (the methodological role). Also I have shown how these views examine and justify whether theoretical concepts have factual reference (the referential role). The factual reference of theories can be considered to be non-observable entities in the sense that only by means of the theory can they or their effects be identified. The factual reference of theories can also be considered as the logical relationship between singular observational statements. If my evaluation tends to establish instrumentalism rather than realism, such an evaluation may be explained in light of the fact that realism is the leading view today. For (as Popper has suggested) no view, no matter how popular or accepted, should escape constant submission to critical examination. I hope that I have critically examined descriptivism, instrumentalism, and realism.
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