Articles

On the Aim of Scientific Theories in Relating to the World: A Defence of the Semantic Account

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According to the received view of scientific theories, a scientific theory is an axiomatic-deductive linguistic structure which must include some set of guidelines ("correspondence rules") for interpreting its theoretical terms with reference to the world of observable phenomena. According to the semantic view, a scientific theory need not be formulated as an axiomatic-deductive structure with correspondence rules, but need only specify models which are said to be "isomorphic" with actual phenomenal systems. In this paper, I consider both the received and semantic views as they bear on the issue of how a theory relates to the world (Section 1). Then I offer a critique of some arguments frequently put forth in support of the semantic view (Section 2). Finally, I suggest a more convincing "meta-methodological" argument (based on the thought of Bernard Lonergan) in favour of the semantic view (Section 3).

1. Differences on Relating to the World

According to the "received view" of scientific theories—as it has been dubbed by Hilary Putnam[1]—a scientific theory consists of an axiomatic-deductive logical calculus which represents relations among theoretical terms, along with a set of semantic guidelines for (partially) interpreting such terms with reference to the world of observable phenomena.[2] These guidelines have been given various labels, including, for example, "correspondence rules,"[3] "bridge principles,"[4] "co-ordinating definitions" and "empirical interpretations."[5] Without such guidelines, it is argued, a theory would have no empirical content. And, since the aim of a scientific theory is to offer an explanation for empirical phenomena, a scientific theory without correspondence rules likewise "would have no explanatory power."[6]

Once correspondence rules are adequately formulated for a given theory, it becomes possible—at least in principle—to deduce from the laws and principles of the theory (correct statements about) the behaviour of observable phenomena. On the received view, scientific explanation and prediction (or retrodiction) consist essentially in this kind of deduction, and so are logically identical. If one correctly deduces (statements about) some observable phenomenon in the absence of its actual observation, then one is predicting (or retrodicting). Similarly, one is said to be capable of explaining some observed phenomenon if one could have deduced (correct statements about) it from the laws and correspondence rules of a theory. Finally, development of the explanatory capacity of the sciences as a whole consists essentially in the extension of the deductive scope of scientific theories to encompass a greater number of possible observables.

The primary role of correspondence rules, then, is to "connect certain theoretically assumed entities that cannot be directly observed or measured ... with more or less directly observable or measurable aspects of medium-sized physical systems."[7] Correspondence rules thus constitute something like a technical "dictionary,"[8] to be used for "translating" the more or less recondite theoretical language of scientific discourse into terms we apply to the observables "with which we are already antecedently acquainted."[9] Like any technical dictionary, correspondence rules must include terms from each of the two spheres to be bridged. Of course, this implies a more or less strict (and problematic)[10] bifurcation of the (non-logical) terminology of scientific theories into a theoretical vocabulary (applied to the non-observable entities of scientific discourse—"electrons," "quarks," etc.) and an observational vocabulary (applied to the observables of our more or less direct experience—"red," "hot," etc.). It is the job of the correspondence rules to define the former with reference to the latter, and thereby to give empirical meaning to the unobservable theoretical entities "assumed" in scientific discourse.

In the early development of the received view, it was thought that it would be possible to provide "*explicit* definitions" of theoretical terms by reference to observables. A term is said to be explicitly defined when it can be eliminated from the context in which it occurs and replaced by the defining expression without any alteration in the meaning of the context.[11] However, certain difficulties which were later discovered militated against this possibility. As early as the 1930s, for example, Carnap pointed out that any attempt to provide explicit definition in terms of observables would involve serious difficulties as soon as dispositional terms (e.g., "soluble," "malleable," etc.) had to be taken into account.[12]

It was resolved that such terms could be given only partial definitions by reference to observables, and according to the "final version" of the received view,[13] such partial definitions are to be provided by means of bilateral reduction sentences. To use Hempel's example, the partial definition for the (minimally) theoretical term "fragility" would read, in the form of a bilateral reduction sentence: $(x)\ (t)\ [Sxt \rightarrow (Fx \Leftrightarrow Bxt)]$, and would specify that "if x is sharply struck at any time t, then x is fragile if and only if x breaks at t."[14] Of course, it is presumed here that the notions "being struck," "sharply" and "breaking" can be correlated with (more or less direct) observables (such as overt physical contact, magnitudes as designated by the position of a needle on a meter, the number of cracks or chips in a jar, etc.).

In contradistinction to the received view of scientific theories, the semantic view involves the denial that a scientific theory must specify its own relation to the world of observable phenomena. On the semantic view, a theory is simply a specification of a model or kind of system; a theory "defines its own subject matter,"[15] and it can do so without any explicit reference to the world of possible observables.

But while the semantic approach denies that a scientific theory must provide a specification of its own relation to the world of observable phenomena (e.g., through correspondence rules), it does not follow that a scientific theory on this view can have no reference to the world of observable phenomena (and hence no explanatory power). On the semantic view, a scientific theory is something like a "definition" of a kind of system;[16] and just as a definition need not specify its own relation to what is objective in order to have objective reference, so the same for a scientific theory. In other words, it is perfectly coherent to argue, as does John Beatty, that "the semantic view is an empiricist philosophy of science. It's just that the empirical claims of science are not supposed to be components of theories. Rather, the empirical claims of science are made on behalf of theories" by the scientists using them.[17]

Thus on the semantic view, the questions whether and under what conditions the kind of system specified by a theory might maintain in the world of observable phenomena need not be resolved by the theory itself. As Frederick Suppe writes, a scientific theory does not tell us directly about the world of observable phenomena, but allows us to make approximate statements about it, by telling us "how the phenomena *would have* behaved *had* the idealized conditions been met."[18] The intermediary specifications which are required in order to make empirical claims on behalf of a theory (or model) are not included within the theory itself. Thus the laws specified by a theory "are not counterfactual, although their particular applications may be."[19]

One of the reasons why the application of models (specified by theories) to actual phenomenal systems remains so open-ended is that such models are only "highly abstract and idealized replicas of phenomena."[20] Thus what is required for an adequate explanation of phenomena is not so much the specification of possible deductive links between theoretical concepts and observable manifestations, but simply an abstract formal (i.e., structural) identity between model and phenomenal system. Abstract models can and "do provide *some* insight into the working of the real systems, even if they do not tell the whole story."[21] This formal identity of model and phenomenal system is what is meant by the notion of "isomorphism."[22]

Finally, it is important to note that the semantic view does not deny that—at least in principle—scientific theories *might* be construed in the kind of deductive structures required by the received view. What it does deny is that a theory *must* be so construed if it is to have any real explanatory power.

2. Some Common Arguments for the Semantic Account

It should be clear from the foregoing that an adequate criticism of the received view on behalf of the semantic account cannot appeal simply to the technical impossibility of specifying a completely unique correspondence between theoretical concepts and observable manifestations. For proponents of the received view have been capable of acknowledging the intrinsic difficulties bound up with precise specification, yet without jettisoning the deductive explanatory ideal suggested by the received view. It may be admitted that a unique correspondence may never be specifiable in actual practice. But proponents of the received view can still continue to maintain that such is at least the true ideal of scientific explanation, an ideal towards which scientists do actually aspire and which they do seem to be approaching asymptotically.

But one might object: if the specification of correspondence rules involves immense complexities, while promising very little return in the way of scientific import, then does not the received view obscure something significant about the real aim of scientific theories?

Consider, for example, the kind of correspondence rules that would have to be specified if one were to determine whether a glass jar is fragile, according to a partial definition like the one suggested by Hempel. As already noted, the partial empirical definition supplied by Hempel presumes that the notions employed can be correlated with (more or less direct) observables (e.g., overt physical contact, magnitudes as designated by the position of a needle on a meter, the number of cracks or chips in a jar, etc.). But even if this is achievable without too much difficulty, it is apparent that one would have to specify a number of further variables (e.g., the hardness of the instrument with which contact is made, the size of the area over which the force is exerted, the angle at which contact is made, etc.), along with the full range of possible relations that might hold between the numerous variables. (For example, the more acute the angle at which contact is made, [generally] the more difficult it would be to produce a chip or a crack in a glass jar.) The number of different experimental situations that could be constructed would almost be matched by the number of different sets of correspondence rules that would be needed.

Now while it may be logically possible to specify adequate correspondence rules for any given experimental situation, it is clear that the kind of specification requisite to even the most simple situation would be extremely complex. While such complexity, then, does not itself provide sufficient grounds for a "knock-down" argument against the received view, the fact that science manages to "get on with its business" without involving itself in such complexities does suggest that the received view obscures something significant about the way in which a scientific theory relates to the world of observable phenomena.[23] The common claim made by proponents of the semantic view can thus be summarized as follows: the semantic view may not have a strictly logical advantage over the received view, but it does seem to have a methodological, or heuristic, advantage when it comes to the analysis of how scientists actually construct theories and use them in making empirical claims.

While I would agree that the semantic view of scientific theories does have a genuine heuristic advantage over the received view, it is not at all clear that the arguments which are frequently put forth to demonstrate this are really very convincing. In fact, given the terms of the debate between proponents of the received view and the semantic view, it would appear that such arguments are ultimately question-begging.

A common argument made on behalf of the semantic view has to do with the number of scientific theories which apparently cannot be made to fit the rigid standards required by the received view (e.g., "evolutionary theory," "Freudian psychology," "theories of the origin of the universe," etc.).[24] But since such theories are genuinely scientific, and have real explanatory power, they provide *bona fide* counter-examples to the received view. Therefore, the received view is inadequate.

Now even if it is admitted that the kinds of theories in question cannot be axiomatized with correspondence rules as the received view would require,[25] it is clear that such counter-examples can be interpreted the other way around: the failure of certain theories to fit the mould of the received view can be just as much a reason for rejecting such theories as unscientific. In other words, the appeal to a theory as a counter-example can cut one of two ways: either to discredit the particular account of scientific theories, or to discredit the theory itself as unscientific. And the manner in which one uses a theory as a counter-example is determined already by the kind of view one has concerning how a theory (if it is truly "scientific") must be related to the world of observable phenomena. But the issue concerning how a theory must be related to the world is the very thing in question in the first place.

A similar kind of circularity characterizes other arguments which have been made in support of the putative "heuristic" advantage of the semantic view. It is generally argued that the semantic view of scientific theories is richer and more fruitful heuristically, because it succeeds in presenting a more accurate picture of the real aims of scientists as they construct and apply theories. But in the debate between proponents of the semantic and received views, the real aim of scientific theories (particularly as regards their relating to the world of observable phenomena) is, once again, the very matter in question. More specifically, proponents of the semantic view argue (under a number of different forms) that the received view is defective because it focusses on trivial difficulties and differences, and endows them with a significance that they do not really have.[26] The problem, however, is that, if one of the essential aims of a scientific theory really is to provide (approximately) explicit empirical interpretations for its theoretical terms through correspondence rules, then the kind of differences which the semantic account might dismiss as "trivial" may not be that trivial after all.

3. A "Lonerganian" Argument for the Semantic Account

Like proponents of the semantic account,[27] Lonergan does not deny the logical possibility of specifying deductive links between the theoretical terms of a theory and the observable phenomena to which they are said to correspond; he only denies that the specification of such links would in itself contribute anything of scientific import. What is valuable in the Lonerganian account is that he also suggests meta-methodological grounds by which this might be demonstrated.

It is not too difficult to articulate the basic contours of the Lonerganian argument: proponents of the received view may be quite correct in insisting that the aim of science is to achieve generality in explanation, and that such consists essentially in the extension of a theory's explanatory capacity to cover a greater number of possible phenomena. But if by such generality one means "invariance"—i.e., validity for a number of different possible observational contexts or frames of reference—then one might see how the abstractness of models and the open-endedness of their possible applications to observables does not imply a defect in the semantic account. Rather, if the aim of scientific theories is to specify sets of relations which would hold for a number of different observational contexts or frames of reference, then such abstractness and open-endedness would be a necessary feature of scientific theories themselves.

To implement this argument, we might first distinguish between two kinds of relations: the relations of things to us and the relations of things to one another. Thus, according to Lonergan, "similarities are of two kinds":

> There are the similarities of things in their relations to us. Thus, they may be similar in colour or shape, similar in the sounds they emit, similar in taste or odour, similar in the tactile qualities of the hot and cold, wet and dry, heavy and light, rough and smooth, hard and soft.

There also are the similarities of things in their relations to one another. Thus, they may be found together or apart. They may increase or decrease concomitantly. They may have similar antecedents or consequents. They may be similar in their proportions to one another, and such proportions may form series of relationships, such as exist between the elements in the periodic table of chemistry or between successive forms of life in the theory of evolution.[28]

Now the aim of a scientific theory is to specify sets of relations which might hold (i.e., be invariant) under a number of different observational contexts or frames of reference. To achieve such invariance, scientists seek to grasp the aspects of things as they relate to one another. As Lonergan writes, it is "not the appearance of colours but the general explanation in terms of wave-lengths of light that is exactly the same no matter what may be the state of observers' eyes, the lighting by which they see, or the speed with which they may happen to be in relative motion."[29] Similarly, relations between the elements are specified in the periodic table according to atomic number. And instead of dealing with life forms as they merely appear to us (i.e., as stable entities), biologists deal with them as they relate to one another, for example in the evolutionary chain.

Lonergan refers to the relations (or correlations) of things to us as "experiential conjugates" and to one another as "explanatory (or pure) conjugates." Thus experiential conjugates "are correlatives whose meaning is expressed, at least in the last analysis, by appealing to the content of some human experience. Pure (or explanatory) conjugates, on the other hand, are correlatives defined implicitly by empirically established correlations, functions, laws, theories, systems."[30]

Because it is the job of scientific theories to specify the relations of things to one another, qualitative observations "give way to measurements."[31] For "measurements relate things to one another rather than to our senses; and it is only the more remote relations of measurements to one another that lead to empirical correlations, functions, laws"[32] that are scientifically significant. Using an example from history, we might say that the Aristotelian notion of "weight" is an experiential conjugate; it resides in a kinesthetic sensation which may not be the same for all possible observers. Against the Aristotelians, Galileo insisted upon the replacement of experiential conjugates with explanatory conjugates. To achieve this, Galileo first showed the error in the ancient Aristotelian correlation which specified that bodies would fall according to their weight. He then selected and specified a correlation which can be shown to hold between two measurable aspects immanent in every free fall, namely the distance traversed and the time required. For all observers on earth, the distance traversed can be shown to be proportional to the time squared.[33]

The same tendency towards invariance or generality (based on the relations of things to one another) is also operative in later developments in physics. For example, Newton's second law of motion achieves some degree of generality insofar as it specifies correlations which are supposed to hold (be invariant) under extra-terrestrial as well as terrestrial contexts; and this invariance, or generality, is possible because the Newtonian notion of force derives its meaning from the relation between mass and acceleration (and not from any relatedness to us as terrestrial observers). In Einsteinian relativity, the invariance is articulated more fully. A Newtonian scientist could not have explained the anomalies which become noticeable as one approaches the speed of light. By understanding time and space as intrinsically related to one another, Einstein was able to articulate a set of correlations which would be invariant (and thus verifiable) even under observational contexts approaching the speed of light. Thus in Einsteinian relativity, velocity is no longer understood simply in relation to three spatial dimensions (with time as an external parameter), but in relation to four dimensions, of which three are spatial and the fourth temporal.

On the basis of this account, it becomes possible to articulate more adequately just what is wrong with the received view. The aim of a scientific theory is to achieve the kind of invariance noted above by specifying explanatory conjugates. (In their emphasis on generality in scientific explanation, the proponents of the received view admit this much.) But the requirement of the received view that such explanatory conjugates be given (linguistic) empirical interpretations (i.e., in terms of experiential conjugates)—which are always relative to a particular observer or observational context—flies in the face of the invariance which scientific theories seek to achieve. In other words, the aim of a scientific theory is to specify explanatory conjugates—correlations which might hold under a number of different observational contexts. The questions whether or how the correlations might hold under a particular observational context—and the question of what observational experiences the particular observers might have—remain extraneous to the scientific theory itself. Thus the received view's insistence upon correspondence rules which can link the explanatory conjugates of science to the possible experiences of observers (which are always context-dependent) does not reflect the real aim of scientific theories.

If the explanatory conjugates specified in a theory really do hold under a number of different observational contexts, then the possible experiential conjugates (empirical interpretations) corresponding to each would necessarily be multiple. It is not a defect in explanation that a number of different empirical interpretations might "work" for a single scientific formulation. Rather, such a possibility is a sign of invariance, which is the proper objective of scientific theorizing. It is only by prescinding from the particularities of the different possible observational contexts (by replacing experiential conjugates with explanatory ones) that invariance is achieved in the sciences.

If the foregoing account is correct, then it would be fundamentally misleading to ask how one is to "decide" among the many possible empirical interpretations of a scientific theory or law. Asking this would be like asking whether one should understand Hilbert's implicit definitions of "point" and "line" with reference to the Euclidean context (where "point" means "position without magnitude" and "line" refers to "length without breadth") or according to the Cartesian context of analytic geometry (where both point and line derive their meaning from mathematical equations). The essential value of Hilbert's implicit definitions of "point" and "line" in relation to one another resides in their generality: Hilbert's definitions cover what is meant in both the Euclidean and the Cartesian contexts but—because of their very generality—can be limited to neither context. The Euclidean and Cartesian notions of "point" and "line" are merely *special cases* of the more general (invariant) definitions of "point" and "line" as articulated by Hilbert.

For the same reason it would be altogether misleading to ask whether the Newtonian definition of force (based on the relation of acceleration and mass) is to be understood with reference to a terrestrial or extra-terrestrial context. The very generality of the Newtonian definition (based on an explanatory conjugate) makes it impossible to say in advance which "empirical interpretation" is appropriate; the Newtonian definition may be used to explain a kinesthetically felt force (on earth) as well as an observed curvature in the path of a meteor (in the heavens).

Finally, this Lonerganian argument does not rule out the possibility that adequate correspondence rules *might* be construed for any given experimental situation. Because observers are also "things" which might be made the objects of science, the relations of things to the observer are included within the relations of things to one another. Thus if one knows the relevant relations between things, as well as the relevant dispositional qualities of an observer, it is possible—at least in principle—to specify precisely what kind of observational experiences the observer could expect to have under any given test situation. In other words, any explanatory conjugate can—in principle—be given precise "empirical interpretation." But, of course, the specification of such "empirical interpretations"—which are necessarily relative to a particular observer or observational situation—would in itself add nothing to our scientific knowledge.[34]

Notes

1 H. Putnam, "What Theories Are Not," in *Logic, Methodology, and Philosophy of Science: Proceedings of the 1960 International Congress*, edited by Ernest Nagel, Patrick Suppes and Alfred Tarski (Stanford: Stanford University Press, 1962), p. 240-251.

2 I use this terminology deliberately, since the received and the semantic views are compatible with both realism and anti-realism. What is asserted by each view is that a scientific theory attempts to provide an explanation for what takes place in the world of observable phenomena—and not necessarily in the "real world" per se. The crucial issue separating the two views centres on the question of *how* a theory is supposed to be related to the phenomena it seeks to explain, and not whether or not these phenomena are part of the "really real world."

3 Ernest Nagel, *The Structure of Science: Problems in the Logic of Scientific Explanation* (London: Routledge & Kegan Paul, 1961), p. 97-105.
4 Carl G. Hempel, *Philosophy of Natural Science* (Englewood Cliffs, NJ: Prentice-Hall, 1966), p. 72-75.
5 Patrick Suppes, "What Is a Scientific Theory?" in *Philosophy of Science Today*, edited by Sidney Morgenbesser (New York: Basic Books, 1967), p. 56. Patrick Suppes is not a proponent of the received view, but makes allusion to these last two ways of speaking in his criticism of the received view.
6 Hempel, *Philosophy of Natural Science*, p. 74.
7 Ibid., p. 73.
8 In fact, Campbell uses the term "dictionary" to denote roughly what is meant here by the term "correspondence rules." See N. R. Campbell, *Foundations of Science* (New York: Dover Publications, 1957).
9 Hempel, *Philosophy of Natural Science*, p. 74.
10 On the problematic nature of the so-called "observational-theoretical distinction," see the following: Peter Achinstein, "The Problem of Theoretical Terms," *American Philosophical Quarterly*, 2, 3 (July 1965): 193-203; Peter Achinstein, *Concepts of Science* (Baltimore: The Johns Hopkins Press, 1968), chaps. 5 and 6; N. R. Hanson, *Patterns of Discovery* (Cambridge: Cambridge University Press, 1961), passim; and Thomas S. Kuhn, *The Structure of Scientific Revolutions*, enlarged ed. (Chicago: University of Chicago Press, 1970), esp. chap. 10.
11 Nagel, *The Structure of Science*, p. 97.
12 Rudolf Carnap, "Testability and Meaning," *Philosophy of Science*, 3, 4 (October 1936): 419-471; and 4, 1 (January 1937): 1-40.
13 By this I mean the received view as it has been developed by Carnap and Hempel. See Frederick Suppe, "The Search for Philosophic Understanding of Scientific Theories," in *The Structure of Scientific Theories*, edited by Frederick Suppe (Urbana: University of Illinois Press, 1974), p. 50-53.
14 Carl G. Hempel, "Empiricist Criteria of Cognitive Significance," in *Aspects of Scientific Explanation and Other Essays in the Philosophy of Science* (New York: The Free Press, 1965), p. 109.
15 Bas van Fraassen, "A Formal Approach to the Philosophy of Science," in *Paradigms and Paradoxes*, edited by R. Colodny (Pittsburgh: University of Pittsburgh Press, 1972), p. 310.
16 John Beatty, "What's Wrong with the Received View of Evolutionary Theory?" in *PSA 1980*, Vol. 2, edited by P. D. Asquith and R. N. Giere (East Lansing: Philosophy of Science Association, 1981), p. 410.
17 Ibid.
18 Frederick Suppe, "What's Wrong with the Received View on the Structure of Scientific Theories?" *Philosophy of Science*, 39, 1 (March 1972): 12. This article is reprinted in Frederick Suppe, *The Semantic Conception of Theories and Scientific Realism* (Urbana and Chicago: University of Illinois Press, 1989).
19 Suppe, "The Search for Philosophic Understanding of Scientific Theories," p. 45.
20 Suppe, "What's Wrong with the Received View on the Structure of Scientific Theories?" p. 12.
21 Ronald Giere, *Understanding Scientific Reasoning* (New York: Holt, Rinehart and Winston, 1979), p. 81.
22 For more on the notion of isomorphism, see Bas van Fraassen, *The Scientific Image* (Oxford: Clarendon Press, 1980), p. 41-46.
23 This is precisely the kind of argument urged by Frederick Suppe in his discussion of the observational-theoretical distinction (a distinction which is implied by the received

view's notion of correspondence rules). See Suppe, "What's Wrong with the Received View on the Structure of Scientific Theories?" esp. p. 9f.

24 These examples are suggested by Beatty in "What's Wrong with the Received View of Evolutionary Theory?" p. 419.

25 Even this is a disputed point, since a number of philosophers of science would want to argue that the kinds of theories sometimes used as counter-examples to the received view can, in fact, be adequately axiomatized with correspondence rules. On the axiomatization of evolutionary theory, for example, see Michael Ruse, *The Philosophy of Biology* (London: Hutchinson University Library, 1973), chaps. 3 and 4; and Mary B. Williams, "Deducing the Consequences of Evolution," *Journal of Theoretical Biology*, 29, 3 (December 1970): 343-385.

26 Some proponents of the semantic view have gone so far as to say that—on the received view—a change in something relatively unimportant, such as the method of applying a theory, would necessarily entail a corresponding change in the theory itself. (See, for example, Suppe, "What's Wrong with the Received View on the Structure of Scientific Theories?" p. 17).

Now, strictly speaking, this is not true. For contemporary proponents of the received view do acknowledge the necessary role played by intermediate theories and assumptions in the application of particular theories to actual phenomenal systems. (For example, see Nagel, *The Structure of Science*, p. 97-105, and Hempel, *Philosophy of Natural Science*, p. 74.) Thus a change in the method of applying a particular theory—even on the received view—need not necessarily entail a change in the theory itself (i.e., trivial differences remain trivial).

Of course, it is a further question whether the received view is really capable of accounting for all of the subtlety involved in the application of theories to actual phenomena via intermediate theories and assumptions.

27 Lonergan's philosophy of science should not be *identified* with the semantic account. Nevertheless, many ideas which underlie the semantic account in its present form were quite current in the late 1940s and early 1950s, when Lonergan was formulating his own philosophy of science.

28 Bernard J. F. Lonergan, *Insight: A Study of Human Understanding* (New York: Harper & Row, Publishers, 1978), p. 48.

29 Ibid., p. 41.

30 Ibid., p. 79-80. It is important to note that Lonergan's distinction between experiential and explanatory conjugates parallels neither (1) Galileo's distinction between "secondary" and "primary" qualities (see p. 84-85); nor (2) the "observational-theoretical" distinction implied by the received view. The "theoretical" term "unexpectedness" would be included among experiential conjugates (since this term has meaning only in relation to our expectations); and the *observed* priority or posteriority of events in a temporal sequence would be included among explanatory conjugates (since such priority or posteriority pertains to the relations of events to one another, and not to us).

31 Ibid., p. 41.

32 Ibid.

33 This, of course, is not to imply that Galileo's law of falling bodies can hold anywhere except for in a perfect vacuum, which is unrealizable in nature. But the more closely one does approximate to the conditions of a vacuum, the more accurate the law of constant acceleration is found to be.

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