CALIFORNIA STATE UNIVERSITY, NORTHRIDGE

THEORIES, COUNTER-EXAMPLES AND THE SYMMETRY THESIS

A graduate project submitted in partial satisfaction of the requirements for the degree of Master of Arts in Philosophy

by

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January, 1973
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ABSTRACT

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In his book, The Anatomy of Inquiry, Israel Scheffler rejects what is known as "The Symmetry Thesis of Explanation and Prediction." The Symmetry Thesis asserts that explanation and prediction have the same logical form. Scheffler's reason for rejecting the Symmetry Thesis is that he believes there are examples of predictions that do not take the same form that explanations do.

I argue that this approach to the Symmetry Thesis is misguided. I show this through an analysis of "theorizing." My argument is that theoretical terms (e.g. "mass" and "force") are defined with specific goals in mind. Since definitions cannot be refuted by counter-example, I argue that theoretical terms can only be criticized in connection with some goal. I then show that "explanation" and "prediction" are such theoretical terms, and hence that the Symmetry Thesis cannot be refuted by counter-example. I then offer some goals
that could possibly have lead someone to assert the Symmetry Thesis. It is in connection with these goals that we can criticize the Symmetry Thesis.

My defense of the Symmetry Thesis is supported by the view that Thomas S. Kuhn takes in *The Structure of Scientific Revolutions*. Kuhn's position is that scientists supply "articulations" of theories when confronted with unexpected findings and that these findings are not regarded as counter-examples.
Theories, Counter-Examples and the Symmetry Thesis

In this paper I hope to shed some light on the relationship between theories and counter-examples by discussing a particular theory and specific attempts to find counter-examples to that theory. The theory in question is variously known as the "Covering Law Model of Explanation" or the "Deductive-Nomological Model of Explanation." One claim that is made by this theory is that "explanation" and "prediction" have the same logical form. The claim is known as the "Symmetry Thesis." A popular method of undermining this claim, and hence the theory in question, is to cite examples of "predictions" that are not "explanations." An example of this approach, which I will discuss, is that of Israel Scheffler. After describing the Deductive-Nomological Model and Scheffler's criticism of it, I will argue that the attempt to refute the Symmetry Thesis by the method of counter-example shows a fundamental misunderstanding about theories. This conclusion will be argued for by discussing the nature of theories and the relevant criteria by which we can criticize a theory.

According to Hempel and Oppenheim, the account of explanation and prediction that they give is by no means novel; it merely summarizes and states explicitly some fundamental points which have been recognized
These fundamental points are captured in the form of four "conditions of adequacy" for any "sound explanation."

(R1) The explanandum must be a logical consequence of the explanans.

(R2) The explanans must contain general laws actually required for derivation of the explanandum.

(R3) The explanans must have empirical content.

(R4) The sentences constituting the explanans must be true.

The explanans is made up of statements of antecedent conditions, along with general laws. From these statements can be deduced a statement describing the event or law to be explained (the explanandum).

These conditions are supposed to have been abstracted from actual examples of scientific explanation. They constitute the necessary conditions that any statement or body of statements must meet to be considered an explanation. Thus, Hempel and Oppenheim have taken a concept that is used by both scientists and non-scientists, and have given it a precise explication. As a result, the concept of explanation provided by R1-R4 may not conform to what non-scientists (and even some scientists) conceive of as an explanation. This point will be discussed in detail later. Now it will simply be said that Hempel and Oppenheim feel that their conception of explanation is in some important ways an improvement over previous conceptions of explanation.

According to them,

... the same formal analysis, including the four necessary conditions, applies to scientific predictions as well as to explanation. ... It may be said, therefore, that an explanation is not fully adequate unless its explanans, if taken account of in time, could have served as a basis for predicting the phenomenon
under consideration.  

It will be noticed that both explanation and prediction, as they are conceived here, take the form of deductive arguments. To the extent that we can get hold of the "ordinary" or "non-scientific" uses of these terms, it would seem that they do not always appear in actual arguments. For example, we say "This car has turned over because a tire blew out" or "Because of the pain in my body, it will rain." In fact, it would appear that the term "predict" is often used without any rational grounds. Thus, it would seem that Hempel and Oppenheim's formal analysis of prediction and explanation does not conform exactly to ordinary use of these terms. It may also be found not to conform to how every scientist uses these terms.

This brings us to various criticisms that have been aimed at the Symmetry Thesis. I will concentrate on those of Scheffler, although the literature of the philosophy of science is full of other similar attacks (see footnote 2). All of these criticisms take the form of attempted refutation by counter-example. That is, Scheffler and others have given examples of "predictions" that are not "explanations." If there are indeed predictions that do not have the same logical form as explanations, then the Symmetry Thesis has been "refuted."

Scheffler's "counter-examples" are implied by the following considerations. (1) Predictions can be mere assertions, while explanations involve inferences based on reasons.  

(2) Explanations require general principles, and predictions do not.  

(3) Even if a predicted event does not take place, a prediction has still been made. An
explained event must take place for there to be an explanation.\(^\text{10}\) \(^\text{(4)}\)

The premises of an explanatory argument must be true, but the premises of a predictive argument need not be true. \(^\text{11}\) \(^\text{(5)}\)

We may have good grounds for predicting an event, but these grounds may not explain the event in question. \(^\text{12}\)

Thus, if (1)-(5) are accepted, we can find examples of "predictions" that do not "explain." That is, (R1), (R2) and (R4) are seen to be necessary conditions for all explanations, but not for all predictions.

Prima facie, there is something very naive about Scheffler's approach to the Symmetry Thesis. To show that this is in fact the case, I will provide a rough sketch of some of the fundamental features of theorizing. As I see it, a key concept in theorizing and in discussing the Symmetry Thesis is "explication," as used by Rudolf Carnap. \(^\text{13}\)

So that they can be understood unambiguously, the meaning of the concepts that make up scientific statements must be known precisely. To this end, a scientist might state in clear and unambiguous terms the meaning of a term already in use. In doing so, the resulting term may or may not have roughly the same meaning as the previous term. Whether or not this is the case can only be decided to the extent that we actually know what the term in question means in non-scientific or pre-scientific usage. One reason that a given term might be given a new meaning is that a scientist might feel that it would enable a theory to accomplish more by giving it a new definition. An example of an explication that Carnap cites is the word "fish," as used by zoologists. Ordinarily, we seem to include any animal living in water as a fish, but zoologists narrow the meaning of this term to include only
those animals living in water that are cold-blooded, vertebrates and who possess gills throughout life. Thus, seals and whales are not fish, according to this definition. The reason for excluding the latter is that the scientific definition is believed to be more useful than the ordinary meaning of the term.

A scientific concept is the more fruitful the more it can be brought into connection with other concepts on the basis of observed facts: in other words, the more it can be used in the formulation of laws. 14

The animals covered by the scientific definition possess more common properties than those covered by the non-scientific definition. Thus, more true general statements about fish can be made by using the scientific definition.

We have seen that a criterion for deciding the acceptability of rival definitions that appear in the statements of scientific theory is the usefulness of the definitions. What sense, if any, can be given to the notion of a counter-example to such statements? It is clearly the case that analytic statements cannot be refuted by counter-example. For instance, we cannot refute the statement "All bachelors are unmarried men" by counter-example, unless "bachelor" and "unmarried men" are independently defined. The statement is what we call "true by definition." The very meaning of the words is correlated with what the counter-example wishes to deny.

Thus, we can see the fruitlessness of trying to find counter-examples to definitions. For example, if someone were to try to find a counter-example to the zoologists' definition of "fish" as an "animal that lives in water, is a cold-blooded vertebrate, and has gills throughout life," we would try to explain the confusion involved in
such an attempt. Clearing up the confusion would involve pointing out that definitions can be good or bad, adequate or inadequate, useful or useless. A term does not "really mean" one thing and not another. Definitions can be criticized, but this must be done so by some legitimate criterion such as the one stated above for deciding which concepts would benefit science the most. Any given definition might be said to be "true" if you accept it and "false" if you do not. Thus, all we can say against a definition is that it should or should not be accepted. Criteria do in fact exist by which we can make such a recommendation. But no criterion exists for determining what a term "really means." Some linguistic recommendations are useful, and others are not, but all definitions are true, if the meanings of the terms involved are accepted. They cannot be falsified by coming up with an example that does not fit the definition, for such an example cannot, in principle, exist.

In what contexts, if any, are counter-examples appropriate? If analytic statements rule out the possibility of counter-examples, then synthetic statements would seem the likely candidates. For example, if I claim that all crows are black (where being black is not part of the meaning of being a crow) then it is in principle possible for this statement to be refuted by counter-example (e.g. a particular white crow). But if "All crows are black" is an analytic statement, then no counter-example can, in principle, be found to it.

Let us consider what role analytic statements play in theories. It is often thought that what scientists most commonly do is empirical research. They are confronted with some problem and they construct a hypothesis to solve the problem. A particular hypothesis can be tested
by isolating the relevant factors and observing whether the claim that the hypothesis makes is confirmed or refuted. Thus, the statements that the scientists asserts would seem to be empirically testable, and hence refutable by counter-example. But the terms that make up these statements contain, as we have seen, certain explicated terms (e.g. "mass," "force," "fish") that the scientist believes to be useful in forming general statements about the universe. Given the definitions, we can empirically test any synthetic statements that contain these defined terms. What we cannot do is empirically test the definition itself. We can only ask whether something worthwhile has been accomplished by defining a given term in a particular way.

A simple example from Newtonian theory will help illustrate this point. According to Newton, (I) the force of a body is equal to the product of its mass and acceleration, and (II) the mass of a body is equal to its weight divided by its acceleration due to gravity. If we construe (I) and (II) as definitions\(^1\) it is possible to logically deduce certain conclusions about the motion of bodies. Given these definitions, it is absurd to think that someone might claim e.g., that he had found an example where the force of a body was not equal to its mass times its acceleration. The force of a body must be equal to its mass times its acceleration since that is the way the word "force" is being used. One can urge another definition of "force" by arguing that the proposed definition would be more useful in some respect. But one may not legitimately claim to have refuted Newton's statements about force, if the refutation calls into question the very meaning of the word "force."

It cannot be overemphasized that by arguing that theories cannot
be refuted by counter-examples, I am not arguing that theories cannot be criticized at all. But if theories are formulated in such a way that counter-examples to them are, in principle, ruled out, then we must explain how it is that certain theories are rejected and others are accepted. It has already been argued that an "explicated" concept is judged according to its fruitfulness. Theories are also judged this way. That is, a theory is acceptable to the extent that it is useful in attaining some goal. With regard to science, for example, it can be roughly said that its goal is to provide knowledge and understanding of the world. To some this may be a means to another goal. That is, successfully predicting and manipulating the future. The point is not so much to precisely determine the goals of science as it is to realize that theories are judged according to their success in attaining some goal. This is the only way we can judge a theory, apart from secondary features like simplicity, etc.

Let us now consider how these central points relate to Scheffler's criticism of the Symmetry Thesis. What we must do is consider what Hempel and Oppenheim were doing in their characterization of explanation and prediction. It seems that they might be construed as laying down the foundations for a theory of explanation and prediction. The theory involves taking vague concepts and actually setting down the precise conditions under which something is an explanation or a prediction. What they came up with were four requirements that explanations and predictions must meet. They did not simply make precise our ordinary concepts, but defined these concepts in a way which (they thought) would best achieve "the goals of science." So these concepts are not precisely the same as our ordinary ones. If they were, the
theory would not tell us anything we did not already know, nor would
the theory do anything that our ordinary concepts do not.

Given this view, we see the irrelevance of Scheffler's "counter-
examples": they are not predictions because they do not meet the
requirements of the theory. Nor do many examples of "explanations"
that we ordinarily cite. The Symmetry Thesis is part of Hempel and
Oppenheim's theory of explanation and prediction. It is not claimed
that it holds between ordinary "explanation" and "prediction," nor
that it will hold between Deductive-Nomological Explanation and
ordinary prediction. The Symmetry Thesis is part of a theory that in
principle rules out as an "explanation" or "prediction" anything that
does not meet the four requirements. So there cannot be, in principle,
a counter-example to the Symmetry Thesis. All "predictions" are
Deductive-Nomological Predictions, and thus no example of Scheffler's is a prediction.

This is not to say that the Symmetry Thesis cannot be criticized,
but to say that most criticisms overlook the claim that is being made.
It is the claim that the goals of science can best be reached by
construing "explanation" and "prediction" on the Deductive-Nomological
Model. Ordinary examples of "predictions" do not refute the Symmetry
Thesis; they are ruled out by the theory, because the Symmetry Thesis
is a theorem of the Deductive-Nomological Theory. If we are to
criticize the Symmetry Thesis, we must do so by asking why "explana-
tion" and "prediction" have been construed the way they have.

We must ask for some justification for the Deductive-Nomological
Model. We judge a theory on how well it aids the performance of some
task, in this case "attaining the goals of science." If the Deductive-
Nomological Model helps achieve these goals, then to that extent it has been justified, and we will know what has motivated Hempel and Oppenheim to hold onto the Symmetry Thesis, in view of such extensive criticism.

The following quote sheds some light on the justification of the Symmetry Thesis.

It may be said, therefore, that an explanation is not fully adequate unless its explanans, if taken account of in time, could have served as a basis for predicting the phenomenon under consideration. . . . It is this potential predictive force which gives scientific explanation its importance: only to the extent that we are able to explain empirical facts can we attain the major objective of scientific research, namely not merely to record the phenomena of our experience, but to learn from them, by basing upon them theoretical generalizations which enable us to anticipate new occurrences and to control, at least to some extent, the changes in our environment. Many explanations which are customarily offered, especially in pre-scientific discourse, lack this predictive character, however. Thus it may be explained that a car turned over on the road "because" one of its tires blew out while the car was traveling at high speed. Clearly, on the basis of this information, the accident could not be predicted, for the explanans provides no explicit general laws by means of which the prediction might be effected nor does it state adequately the antecedent conditions which would be needed for the prediction.16

Thus, we see that the goal of scientific research is to "learn" from phenomena, so that we may "anticipate new occurrences." The important claim is that we must be able to explain an event in order to anticipate it. The Symmetry Thesis clearly follows from these remarks. That is, every explanation must serve as a prediction and every prediction must serve as an explanation. The latter claim is crucial, for it states that we can be reasonably certain that an event in the future is going to take place only if we can explain it. We can see that if we can deduce a statement of an event in the future, we clearly have good reason to believe it will take place. Inductive
grounds also provide good reasons, but not the guarantee that Deductive-Nomological Predictions provide. This guarantee makes the latter the ideal, as far as successful predictions go. Also, we have seen that we can predict more from Deductive-Nomological Prediction, because such predictions meet R2. The presence of general laws provides us with a wider range of applications than does e.g., statistical "predictions" which are not based on general laws. It is not crucial whether or not the Symmetry Thesis actually does aid in "anticipating new occurrences." The crucial fact is that we realize that the only relevant criticism of the thesis will deal with whether or not the Symmetry Thesis does aid in "anticipating new occurrences," or whether this is a worthwhile goal. It is here that Scheffler's criticisms miss the mark completely. He ignores the fact that "explanation" and "prediction" have been construed with respect to a particular theory and are not being used ordinarily. Rather than asking why they are used in the way they are, Scheffler looks for "counter-examples." The only relevant criticism would be to deny that success in "anticipating new occurrences" is the goal of science or that the Symmetry Thesis fails to aid in this task. But if you do not deny this the Symmetry Thesis follows, and there cannot, in principle, be a counter-example.

Nor need we abandon the attempt to justify the Symmetry Thesis on the basis of the goals of science, if we believe that success in "anticipating new occurrences" is not the goal of science. An alternate claim is that the goal of science is to provide knowledge and understanding of the phenomena we experience. We want not merely to anticipate new occurrences, but to actually understand them. If so, we mus-
judge the Symmetry Thesis on the basis of how well it aids science in attaining this goal. Clearly, any prediction that can serve as an explanation would provide us with knowledge and understanding. And any prediction that could not serve as an explanation would not provide us with knowledge and understanding. The latter "predictions" (such as the statistical "prediction" mentioned above) do not provide us with as much understanding, and hence do not qualify as "scientific predictions," according to this view of science.

So the Deductive-Nomological Model can be conceived as an attempt to formulate "explanation" and "prediction" in such a way to best achieve certain goals (knowledge and understanding). The Symmetry Thesis is part of a theory of what science ought to do and can only be criticized accordingly. The thesis is not simply true or false. It is reasonable or unreasonable according to how useful it is in achieving certain goals and how reasonable these goals are. To criticize the Symmetry Thesis we must argue that it is not useful in achieving knowledge and understanding or that the latter are not worthwhile goals. The conclusion we must reach is not as important as realizing which considerations are relevant and which are not. It is not relevant to find a "counter-example," for the theory will allow none. We must ask why the theory chose to construe "explanation" and "prediction" as having the same form. We cannot bring up cases that are of different form anymore than we can find a bachelor that is married. We can criticize the Symmetry Thesis, but we have to ask the right questions. Scheffler and other critics try to find examples of predictions that do not explain. But within the theoretical structure, predictions necessarily explain. Any relevant criticism will acknowledg-
ledge this and ask why the theoretical structure is as it is.

The defense of the Symmetry Thesis that has been developed so far is similar to the position of Thomas S. Kuhn. Kuhn argues that scientists do not simply abandon a scientific theory when confronted with an unexpected finding.

They do not, that is, treat anomalies as counter-instances, though in the vocabulary of philosophy of science that is what they are. . . . No process yet disclosed by the historical study of scientific development at all resembles the methodological stereotype of falsification by direct comparison with nature. . . . By themselves counter-instances cannot and will not falsify a philosophical theory, for its defenders will . . . devise numerous articulations and ad hoc modifications . . . so that these anomalies will then no longer seem to be simply facts. From within a new theory of scientific knowledge, they may instead seem very like tautologies, statements of situations that could not conceivably have been otherwise. 17

Applying this analysis to the Symmetry Thesis, we can look at Scheffler's criticisms as either puzzles that need to be explained (or explained away) by the theory or as counter-examples to the theory. Which alternative we choose will depend upon our acceptance or rejection of the theory in question, since we have seen that "within" a theory certain statements describe situations that could not conceivably be otherwise. Thus, Hempel and Oppenheim must further "articulate" their theory so that apparent facts (Scheffler's "counter-examples") no longer seem to be facts. For instance, it seems that there are many things we can predict without explaining. On the basis of past regularities we can anticipate the behavior of certain persons, and yet remain unable to explain this behavior. It is not sufficient for Hempel and Oppenheim to reply that explanation and prediction have the same logical form by definition. Though this is the case according to their theory, some reasons must be given as to
why it appears that there are counter-examples to the Symmetry Thesis. To continually emphasize the tautologous nature of the Symmetry Thesis is to make a trivial and uninteresting claim.

Since Hempel and Oppenheim have chosen familiar terms, they must account for the prima facie conflict between their use of these terms and the pre-theoretical use of them. This can be done by arguing that the pre-theoretical use of "prediction" does not always embody exactly what is expected from the concept "prediction." The examples of "predictions" that Scheffler gives seem to ignore some of the essential characteristics of predictions. For example, there is a difference between a prediction and a guess. A person who predicts something has to have good grounds for doing so. He must have some knowledge or understanding of the event in question which enables him to predict its occurrence. Hempel and Oppenheim want to claim that the person who lacks this knowledge and understanding of a future event cannot really be said to be "predicting."

If it is accepted that "prediction" is related to "knowledge," "good grounds," and "understanding," then the next step in the "articulation" is to show that these latter concepts play a role in the Deductive-Nomological Theory. That is, it must be shown that there is some relationship between "prediction" and R1-R4. If "good grounds" or "knowledge" are requirements of all predictions, then we can see the reason for emphasizing deduction and general laws. A deductive argument (R1) with all true premises (R4) offers the best possible grounds for asserting that an event will take place in the future. Since the premises contain general laws that are actually required in the argument (R2), the events in question are without exception sub-
sumed under the law.

To see that R1-R4 constitute the best possible grounds for predicting we only need to examine "predictions" that fail to meet these requirements. For instance, if our grounds for asserting that event E will take place are inductive, then we cannot say with absolute certainty that E will take place. But if we can deduce a statement describing E from true premises, then we can say with certainty that E will take place. With regard to general laws, we can see that where some statistical relationship holds between certain properties then again we cannot say with certainty that E will take place. At best we can conclude that a certain percentage of the time E will take place. But if our premises contain general laws, an invariable relationship between certain properties is established. With a relevant law or laws and certain antecedent conditions, we can conclude with certainty that E will take place. Thus, R1-R4 constitute the best possible grounds that one could have for asserting that an event in the future is going to take place. So ideally at least, all predictions will meet R1-R4. Those "predictions" that do not will be judged by how closely they approximate the ideal.

In the same way, Deductive-Nomological Explanation will be considered the ideal. According to Hempel and Oppenheim, this type of explanation may be referred to as "causal explanation." Certain antecedent conditions are said to "cause" an event E in that the empirical regularities expressed by certain laws imply that if the antecedent conditions are met, then E will occur. Thus, the presence of laws in a Deductive-Nomological Explanation establishes "general and unexceptional connections between specified characteristics of
Such connections are not established by statistical "explanations," even if such "explanations" are deductive. Thus, statistical "explanations" cannot be described as causal explanations, since statistical laws do not logically imply that E will take place whenever certain antecedent conditions are met. Statistical laws only make E "probable."

Explaining an event E in terms of knowing the cause of E reflects a long philosophical tradition. For instance, Aristotle claimed that knowledge and understanding belong to art rather than to experience, and we suppose artists to be wiser than men of experience (which implies that wisdom depends in all cases rather on knowledge); and this because the former know . . . the why and the cause. According to Mill, "an individual fact is said to be explained by pointing out its cause, that is, by stating the law or laws of causation of which its production is an instance." For reasons stated above, Hempel and Oppenheim feel that their emphasis upon deduction and general laws has effectively captured this tradition. Since statistical and other types of "explanation" fail in this respect, Deductive-Nomological Explanation is considered to be the ideal.

Now it can be seen that in their ideal forms explanation and prediction both meet R1-R4, which is to accept the Symmetry Thesis. We might also expand the thesis so that there are various levels of symmetry, where level 1 represents the Deductive-Nomological Model. For instance, statistical explanation, of some specified high or low level in relationship to level 1, may be said to be symmetrical with statistical prediction of some corresponding high or low level. The same could be done with other types of explanation, depending on their
relationship to level 1. What we cannot do is use some "low-level" prediction as a counter-example to the Symmetry Thesis, by showing that the example does not meet Rl-R4. Some things that are called explanations do not meet Rl-R4, so that to find a "prediction" that also does not meet Rl-R4 is not surprising. Ideal explanation can only be attacked by comparing it with ideal prediction. It is foolish to compare Deductive-Nomological Explanation with, for example, low level statistical prediction and expect to find them symmetrical. The Symmetry Thesis was asserted as part of a theory of explanation. Within this theory, explanation takes on a precise definition, which is supposed to reflect philosophical and scientific tradition. Since Deductive-Nomological Explanation does not reflect exactly what those "outside" the theory take "explanation" to mean, it should not be surprising that Deductive-Nomological Prediction should also depart from what those "outside" the theory take "prediction" to mean.

With this "articulation" in mind, we are in a position to re-examine the puzzle that faced the Symmetry Thesis. The puzzle was that there seem to be predictions that have no explanatory value. Now we see that such "predictions" do not meet Rl-R4 and are thus not predictions. They are not, at any rate, "ideal" predictions and thus cannot be contrasted with explanations that do meet Rl-R4. If the prediction lacks explanatory power in that it departs from Rl-R4, it also lacks predictive power for the same reasons. Both explanations and predictions will ideally meet Rl-R4. So Rl-R4 have become the standard by which we decide whether or not something is an explanation or a prediction. It cannot be claimed that some statements predict
but do not explain, because to the extent they fail to do the latter, they also fail to do the former. The purpose of the "articulation" was to establish this fact. That is, prediction and explanation were shown ideally to meet R1-R4. Once this is accomplished, counter-examples are ruled out. A predictive "counter-example" necessarily fails to meet R1-R4 and is thus not a counter-example. Something is an explanation or a prediction to the extent that it meets R1-R4. A "counter-example" that fails to meet R1-R4 is not a counter-example.

We have seen Hempel and Oppenheim's defense of the Symmetry Thesis as "articulating" a theory. This articulation involved relating prediction to knowledge and understanding, and relating knowledge and understanding to R1-R4. We need not consider this attempt successful as much as we need to see how their attempt illustrates what theoreticians do in the face of problems. They do not treat the problem as a counter-example. To do so would mean abandoning the theory. The theoretical structure is laid bare so that the problem at hand can be dealt with. Here this involved relating prediction to knowledge and understanding. The result was that what appeared to be a counter-instance turned out not to be a fact at all; what appeared to be a prediction turned out on closer examination not to be one. Within the theory, all predictions must meet R1-R4. A person who accepts the theory can see only Deductive-Nomological Predictions as predictions. A person who rejects the theory sees Scheffler's cases as counter-examples, i.e., as facts that conflict with the Symmetry Thesis. By articulating the theory, we attempted to lessen the force of Scheffler's criticisms. What initially appeared
to be a prediction was shown not to be, according to the theory. Thus, refuting the Symmetry Thesis is not a simple matter of finding a counter-example to it. The theory T behind the Symmetry Thesis establishes a connection between explanation and prediction that makes the Symmetry Thesis a truth of T. Apparent conflicts will be explained by articulating or modifying the theory so that no conflict exists. Thus, whether or not one accepts the Symmetry Thesis will depend upon whether or not one accepts the theory behind it. Rejecting a theory is a complex procedure involving more than a counter-example. A "counter-example" may force a theoretician to re-examine his theory. As we have seen, he will have to articulate and modify his theory in order to account for apparent conflicts. The theoretician may even eventually abandon his theory, but he will not do so simply because of a "counter-example." He will do so because his theory is unable to explain the apparent conflict that the "counter-example" exhibited. Hempel and Oppenheim can try to account for apparent conflicts by bringing out a connection between prediction and explanation. Given this connection, the Symmetry Thesis follows, and no counter-example to it is in principle possible. This is not to say that the Symmetry Thesis cannot be criticized. What must be called into question is the theory itself along with the articulations and modifications that go with it.
FOOTNOTES


4 Hempel and Oppenheim, p. 324.

5 Ibid., p. 322.

6 Ibid.

7 Ibid., p. 323.

8 Scheffler, p. 43.

9 Ibid.

10 Ibid.
11 
\textit{Ibid.}, p. 44.

12 
\textit{Ibid.}, p. 42.


14 
\textit{Ibid.}, p. 6.


16 Hempel and Oppenheim, p. 323.

17 Kuhn, pp. 77-78.

18 Hempel and Oppenheim, p. 324.

19 \textit{Ibid.}
